STRANDLOPERS AND SHELL MIDDENS

An investigation into the identity, nomenclature and life-style of the
indigenous inhabitants of the southern African coastal region in the prehistoric
and early historical period, with a recent example.

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This study is dedicated
to the memory of
FRANZ RICHARD (FRANK) SCHWEITZER
(1925-1979)
friend and mentor,
who called his home
'Strandlopernes'.

ABSTRACT

This study investigates the use of the name 'Strandloper' and its application to people and archaeological sites in the southern African coastal region.

The evidence in the early records left by voyagers and settlers from Europe is that the name was originally applied to a small band of Khoisan people, the Goringhaicona, who lived on the shore of Table Bay during the seventeenth century and whose way of life was typical of neither the Khoikhoi herders nor the San hunter-gatherers. Towards the end of the last century the name was applied to human skeletons and archaeological sites found in the coastal region, thence finding its way into the scientific and popular literature.

The early claims of physical anthropologists for a separate 'race' of 'Strandlopers' are shown to be unsubstantiated. An investigation of the stature of skeletons from the coastal region has shown that the stature of the prehistoric people was within the range of modern Khoisan and South African Negro samples, but generally closer to that of the San.

The archaeological evidence also does not support the suggestion of a culturally distinct people whose technology differed from that of other people in the region, and who subsisted solely by 'strandloping', or collecting and scavenging along the shore. It is shown that sites in the coastal region are the result of the activities of three groups with differing economies: hunter-gatherers, herders and farmers. Most of these sites contain a terrestrial component as well as a marine one, and it is argued that these sites represent only one part of these groups' subsistence activities.

The name 'Strandloper' is thus shown to be misleading in its implications and therefore correctly applicable only to the Goringhaicona, who were given that name and applied it to themselves. Even then, the implications of the name misrepresent these people's actual life-style, so it should only be used informally, as a sobriquet.

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A thesis is expected to bear the hallmark 'all my own work'. However, as will be seen from the text and bibliography, I am indebted to all the researchers and recorders whose publications I have consulted, or to which I have referred, and the results of whose work I have used. Without the benefit of their spadework, this study would have been immeasurably the poorer - if, indeed, it would have been possible at all. To this great extent, then, I acknowledge my debt to them. Where I have disagreed with them, I trust that they will take this in good spirit as one of the bases of scientific enquiry, and in accordance with the principle of *audi alteram parte*. If I have misquoted or misunderstood them, I apologise and ask that they accept that the error is mine.

In addition to the assistance provided by those whose works form the basis of this study, there are others who have contributed in numerous ways: my colleagues at the South African Museum, Margaret and Graham Avery, Bill van Rijssen, Vivien van Zyl and Louis Lawrence (Archaeology), Marilyn and John Pether and Noel Fouten (Palaeontology), Thea Reyneke, Rina Krynauw, Jacqueline Truman-Baker and Isgaq Bendie (Library), Bettie Louw (Publications) and Gerald Klinghardt (Ethnography); also to Richard Klein, University of Chicago; John Lundy, Clark College, Vancouver; Alan Morris, UCT Medical School; Judy Sealy, UCT Archaeology Department; Dieter Noli, an Archaeology Ph.D. student at UCT; and Thea Toussaint van Hove, friend and voluntary co-worker. Not least, I am grateful to my supervisor, John Parkington, for his wise direction.

I take this opportunity to pay tribute to Professor G. S. Nienaber of the Instituut vir Naam- en Kultuurnavorsing, Human Sciences Research Council, Pretoria, octogenarian and still going strong, whose onomastic research on the Khoekhoen (to use his preferred orthography) is without equal, and whose friendly and instructive correspondence over the years has benefited me greatly.

Whatever merits this study may have are largely due to those whose work has contributed to it. The interpretations and any errors are, however, my own and the responsibility for these is entirely mine.

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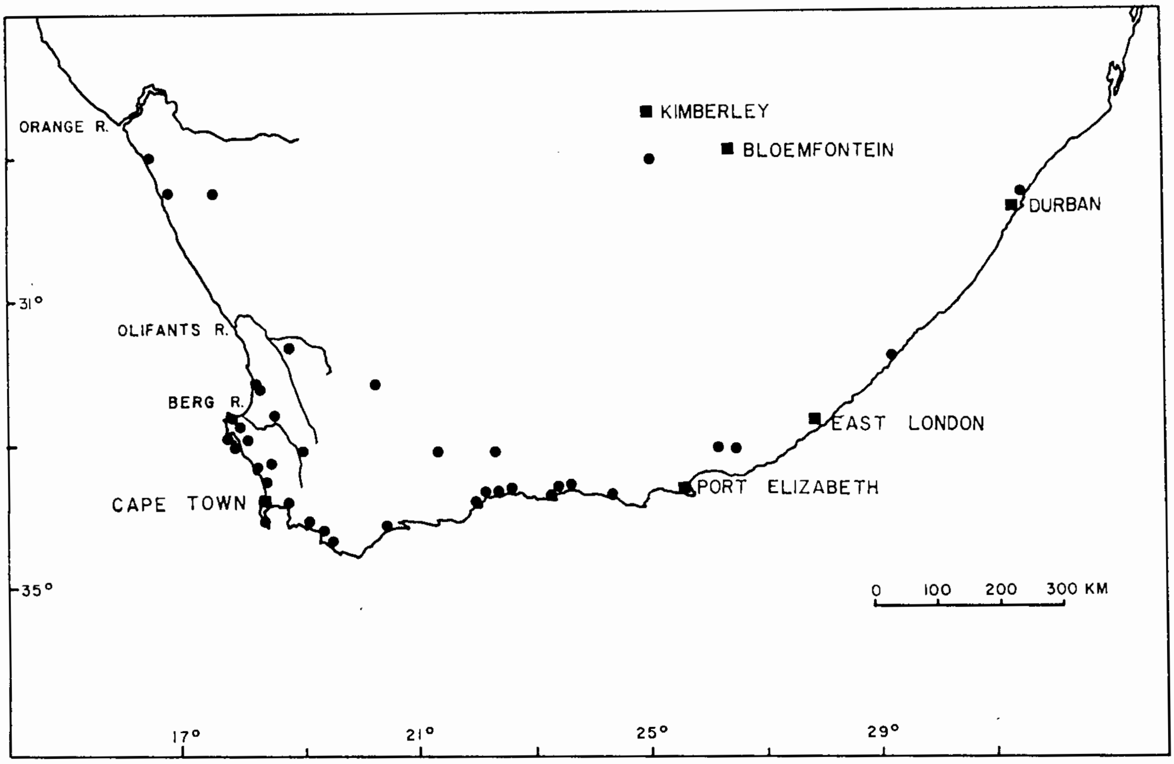
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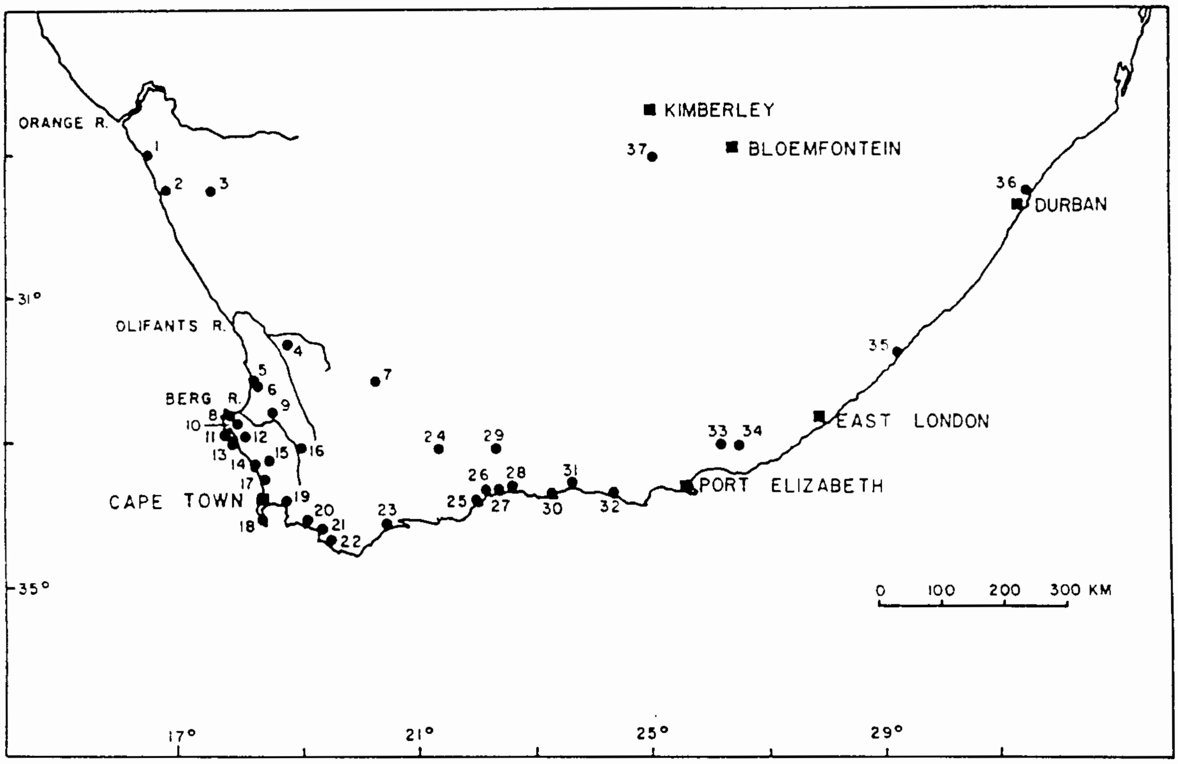

Figure 1. Map of South Africa showing the location of places and sites mentioned in the text. 1. Port Nolloth, McDougall's Bay. 2. Grootmis, Buffels River. 3. Springbok. 4. De Hangen. 5. Elands Bay Cave, Tortoise Cave, Hailstone Midden. 6. Diepkloof. 7. Sutherland. 8. Stompneus Bay (St Helena Bay). 9. Piketberg, Het Kruis. 10. Kasteelberg. 11. Saldanha Bay, Sea Harvest, Hoedjies Punt. 12. Elandsfontein. 13. Oudepost I, Stofbergsfontein. 14. Yzerfontein. 15. Malmesbury. 16. Tulbagh. 17. Duinefontein. 18. Cape Peninsula: (west) Sandy Bay, Hout Bay, Kommetjie, Bonteberg; Cape Point; (east) Miller's Point, Simon's Town, Kalk Bay. 19. Gordons Bay (False Bay). 20. Hawston. 21. Die Kelders. 22. Byneskranskop. 23. Koppie Alleen. 24. Buffelskloof. 25. Mossel Bay. 26. Great Brak River. 27. Herold's Bay. 28. Oakhurst. 29. Boomplaas. 30. Nelson Bay Cave. 31. Bloukrans. 32. Klasies River Mouth. 33. Melkhoutboom. 34. Wilton. 35. Mpame. 36. Mjonzani. 37. Koffiefontein.

INTRODUCTION

> 'It would be convenient if the people who lived so largely upon shellfish had a distinctive name given to them, for the word strandlopers (beach-rangers), applied to them by some recent writers, causes much confusion' (Theal 1918: 4).

In a paper on the nomenclature of the Khoisan (Wilson 1986*a*), I omitted discussion of one of the names, 'Strandloper'. This was used extensively during the earlier part of this century to denote the people believed to have been responsible for the creation of what are now archaeological deposits, particularly shell middens, in the southern African coastal region. The name is still to be found in more recent scientific publications and is the one most commonly used in the popular media. It is my view that the application of this name is based on misconceptions of the identity and life-style of the people so called, and that it thus merits discussion. The names 'Watermen' and 'Vismans' are also discussed, since they were sometimes also applied to the 'Strandlopers' of the early historical period.

Archaeological research during the past two decades has suggested that occupation of the coastal region and the exploitation of marine resources was only a part of the seasonal round of the indigenous San hunter-gatherers. More recent research has indicated that this pattern was disrupted after about 2 000 years ago by the incursion of the pastoralist Khoikhoi and black farmers into the region, and again by white settlement and expansion after 1652. This general theory of seasonal movement has been challenged lately by isotopic analyses of human bone that appear to show that part of the population studied had a predominantly marine diet and therefore spent most, if not all, of their time at the coast. This, then, suggests that there were coastal people who were more sedentary than the seasonally mobile ones who moved between the coast and the interior and thus supports the 'Strandloper' theory.

This study investigates a range of aspects in an attempt to determine whether either of these conflicting theories is correct, or whether they can both be interpreted as facets of a life-style that was perhaps not as rigidly defined as either hypothesis supposes. Research has been mainly documentary and has concentrated on the Khoisan who occupied the western and south-western parts of the coastal region. This is because the name 'Strandloper' was first given to some of the Khoisan and later applied to their sites in the coastal region. The records of the Khoisan, both documentary and archaeological, are also more comprehensive than those of the black people who moved into the eastern and south-eastern parts of southern Africa during the early centuries of the present era.

Part I (Chapters 1-4) concentrates on the evidence provided by the records left by early travellers and settlers in the period 1488-1690 and again in 1779-80. Part II (Chapters 5-10) deals with the revival of the 'Strandloper' concept in the late nineteenth century and examines the evidence of physical anthropology, archaeology, archaeometry, botany and ethnography. Each chapter ends with a discussion that partly summarizes the information therein and adds comment on certain aspects.

As part of the physical anthropological investigation, a practical element is introduced in Chapter 5 with the measurement and analysis of the femora of dated human skeletons from the south-western and southern Cape coastal region. The application of a recently developed, universally applicable formula for the conversion of femur length to living stature enables comparison of these measurements with those for living populations (Khoikhoi, San and South African Negro) in order to determine whether there was any change in the stature of the Khoisan during the period for which dated specimens were available.

The last part of this study (Chapter 11) summarizes the information provided in the preceding chapters. The evidence is that the southern African coastal region was occupied by San hunter-gatherers, Khoikhoi pastoralists and black (Negro) farmers and that most of the midden deposits in the coastal region include a terrestrial component as well as a marine one. This shows that the people responsible for the deposits did not subsist only by 'strandloping' - living only on what they could obtain by beachcombing - but that they exploited all the resources of the coastal zone, in some cases including exotic animals and plants brought into the region by the herders and farmers. It is therefore concluded that the name 'Strandloper' may be applied validly only to a small Khoisan group, the Goringhaicona, who lived on the Cape Peninsula in the seventeenth century and then only as an informal sobriquet that was given to them by the Dutch, and which they used themselves.

This study expands discussion of the same topic in an article published in a popular magazine (Wilson 1989*a*).

The Appendix contains my evaluation of the reconstruction of prehistoric human diet by Buchanan (1988), with particular reference to the Elands Bay area. Dietary reconstruction is an important aspect of archaeology, but I consider Buchanan's method to be defective, so that his reconstruction is unacceptable, as are any deductions derived from it. My evaluation is appended to this study because in Chapters 6 and 8 I have commented adversely on Buchanan's reconstruction, and these comments need to be substantiated.

THE INFORMATION BASE

Scientific integrity requires that all information relating to a particular piece of research be presented and evaluated, also that any hypothesis be tested against alternative hypotheses in order to determine which is the most tenable in the light of the information provided or available. I have not provided all the information that is available (to do so would be the labour of a lifetime), nor have I proposed alternatives to the hypothesis underlying this study: that the 'Strandlopers' did not exist as a biological and socio-cultural entity distinct from the other populations in the southern African coastal region. The records, whether documentary or archaeological, do not contain *all* the information necessary for a proper, scientific, evaluation, nor for testing alternative, or competing, hypotheses. What I have done, therefore, has been to select information that I consider relevant to discussion of the topic on a fairly broad level and I have examined it to see whether or not it supports my hypothesis.

The information base is far from perfect. The documentary evidence is incomplete and reflects biases resulting not only from the particular interests of the recorder (the reason for making the record), but also from his or her sociocultural background. The archaeological record is also incomplete, in that sites contain the remains of only a part of the total activities of the people who occupied them. Part of the original information has been destroyed by post-depositional processes, and more is lost during and after excavation. What is carried back to the laboratory and analysed there is filtered through the intellectual screens of the analysts, so that the published information, like that of the documentary records, reflects the interests and sociocultural background of the author(s).

My selection of information for inclusion in this study necessarily reflects my own interests and sociocultural background: another filter through which the information has been passed. Value judgements, whether implicit or explicit, are inherent in the inclusion or exclusion of information, and in my comments on what I have included. This study, based as it is on incomplete and imperfect information, is thus not to be seen as definitive, but rather as providing a basis for further research.

NOTES ON NOMENCLATURE

For the sake of consistency, all names such as ‘Strandloper’, ‘Vismans’, ‘Saldanhamen’ and ‘Sonqua’ are generally used here in this form, regardless of how they are given in the works cited. This has necessitated some minor editorial changes to the texts quoted, such as the changing of verbs from singular to plural and the addition of the definite article. However, for ease of reading, these changes are not enclosed in square brackets, as is the convention.

'Khoikhoi' is used instead of the Nama 'Khoekhoen', although the latter is now considered to be the correct spelling (Haacke 1982: 78; Nienaber 1990: 616-625). The reason for this is that unless the correct sounds of the vowels, which are pronounced separately and not as diphthongs and also have different tonal values, are used, 'Khoekhoen', which has no vocal equivalent in English, is likely to be mispronounced. Moreover, because of its visual similarity to the Afrikaans 'koekoek', cuckoo, and its auditory similarity to the English word, it is liable to be used pejoratively. The English pronunciation of 'Khoikhoi' is a reasonable approximation of the correct Nama pronunciation (Wilson 1986*a*: 253-254).

'San' is used in place of the orthographic 'Saa(n)' because it has generally been sanctioned as an alternative to 'Bushman' and despite the fact that the name has pejorative connections (Hahn 1881: 3; Wilson 1986*a*: 255-256). Yates *et al.* (1990: 14) stated that they preferred not to use the term 'Bushman' 'because it has over the last two centuries become a derogatory term with little useful meaning'; and that the name 'San' was applied to the hunter-gatherers by the Khoikhoi, 'and it may be that their use of San was derogatory. The name San is thus used as the "lesser of two evils", and it has now gained a general acceptability'. I have discussed the use of the two names elsewhere (Wilson 1986*a*: 254-259), as has Nienaber (1989, under various headings), and while there is no certainty as to the precise meaning of 'San' - probably 'bush people', however that is to be understood - there is no question that the use of 'San' or its variants by the Khoikhoi *was, and still is*, derogatory, particularly in conjunction with the elitist interpretation of 'Khoikhoi' as 'men of men, i.e men *par excellence*' (Hahn 1881:2). By definition, racism exists when people of one race assert their superiority over people of another race, but since the Khoikhoi and San are arguably members of the same race, the former cannot be charged with racism. The charge can, however, be levelled at the Dutch, who took over the name and its implications from the Khoikhoi, transferring these to their own word, 'Bosjesmans', as well as at all those non-Khoisan who now use the word. 'San' was selected as a suitable biological alternative to 'Bushman' *by white academics* (Jenkins & Tobias 1977: 51), at least some of whom must have been aware of its derogatory connotation, *and without reference to the people to whom the name was given.* To use the name 'San' is thus to derogate from the dignity of the people so called, and its use by people of other races is racism and the perpetuation of an injustice that is centuries older than that caused by the use of 'Bushman'. That 'San' is perceived as 'the lesser of two evils' is because relatively few people know that it has derogatory connotations. The reason for this is that those responsible for educating people in such matters have been less assiduous in publicizing the fact than they have been in drawing attention to the 'evil' of using 'Hottentot' and 'Bushman'. Even though it may be 'the lesser of two evils', it is none the less an evil and we should therefore acknowledge our culpability, rather than exercise our moral superiority over those for whom 'Bushman' has no pejorative connotation. Lorna Marshall, whose integrity is surely above challenge, said 'In my feelings, I accord "Bushman" the dignity of any dignified race name' (Marshall 1976: xxi). The problem, as I have said elsewhere (Wilson 1986*a*: 256), is to find an alternative that will be acceptable to all concerned.

'Khoikhoi' and 'San' are often used interchangeably for pastoralists and hunter-gatherers respectively, although it is recognized that these equivalences are not necessarily, or always, accurate (e.g., Schrire 1980; Wilson 1986*a*: 261). 'Khoisan' is used as a generic to include both Khoikhoi and San. The use of these names can be justified on the grounds that in all languages people have their own names for themselves and for other people, those for the latter generally not being the same as those used by the other people themselves. Names fall into two groups, one in which the name is recognizable as a variant of the 'own name', as in the case of 'English/Engels/Anglais/Inglese', the other in which the name is not a recognizable variant, as in the case of 'Deutsch/German/Allemand/Tedesco'. 'Khoikhoi' falls into the first category, and 'San', 'Hottentot' and 'Bushman' into the second, while 'Khoisan' is comparable to 'Eurasian' and 'Amerindian'.

A trend that appears to be gaining currency, through its use in government parlance, is to use 'African' as the name for the peoples previously called 'blacks', and to use the latter term to include all the peoples of South (or southern) Africa who are not classified as 'African' or 'white' (e.g. Saunders 1990). To my mind the exclusive use of this term to describe only *part* of the peoples of Africa does a grave injustice to others, such as the Khoisan, and denies them their birthright: they are 'of Africa' and therefore *African*. In this study I therefore use the term 'black' to describe those peoples who are biologically classified as Negroes. The use of colour ('black', 'coloured', 'white') to define sociocultural and/or racial groups is invidious in any context but is likely to persist for the foreseeable future, and not only in South Africa.

The terms 'prehistoric' and 'historical' are used in preference to 'pre-colonial' and 'colonial'. The first two are generally accepted to refer to *written* history, which none of the indigenous peoples of southern Africa possessed until relatively recently. The second two are now generally used with reference to white settlement of the region, but it is arguable that *all* the people who occupied the region colonised it from other parts and that the general use is thus too restrictive. Yates *et al.* (1990: 4) have suggested that the implications of 'prehistory' are pejorative, and that people who had, or have, oral traditions consequently have a history. However, since this study deals chiefly with peoples who left no record, oral or written, of their past, and whose descendants have little or no memory of their ancestors' past, I consider it legitimate to use 'prehistoric' and 'historical' here, in the sense given above. The goal of archaeology is to turn prehistory into history, and to do so as accurately as possible. This study is offered as a contribution to that goal.

PART I. THE DOCUMENTARY EVIDENCE

CHAPTER ONE

EARLY CONTACTS

FIRST MEETINGS

When the first Portuguese explorers under Bartolomeu Dias had rounded the southernmost point of Africa in 1488, they put in at a bay they named *Angra dos Vaqueiros*, 'Bay of the Herders' (now Mossel Bay), because the people seen there had many cattle. These people were terrified by the arrival of the strangers from the sea and fled inland without the Portuguese being able to make contact (Raven-Hart 1967: 1).

In November 1497, however, when a second fleet under the command of Vasco da Gama put in at a bay on the (Cape) west coast that they named *Santa Ellena* (now St Helena), the Portuguese were able to make contact with some of the local inhabitants.

> 'In the land the men are swarthy. They eat only sea-wolves and whales and the flesh of gazelles and the roots of plants. They wear sheaths on their members. Their arms are staffs of wild olive trees tipped with fire-hardened horns. They have many dogs' (Raven-Hart 1967: 3).

The following day, the Portuguese 'captured' a man, who 'was small of body . . . and was going about gathering honey on the moor'. He was fed and clothed and put ashore the next day, when 14 or 15 other men visited the ships. They caught a 'sea-wolf' (seal), which they roasted and gave, with 'some of the roots of plants which they eat', to one of the Portuguese crew who accompanied them (Raven-Hart 1967: 3-4).

During the eight days that the fleet spent in the bay, the number of the indigenous visitors, all men, rose to between 40 and 50. There was no evidence of cattle, but the men indicated that their village was at the foot of some hills about two leagues (approx. 12 km) distant (possibly the Patrysberg, south-west of the Berg River mouth). The Portuguese bartered with the men for the shell ornaments they wore in their ears, also for 'fox-tails, which they carry fastened to sticks, and with which they fan their faces'. One of the men also bartered his 'sheath' for a *ceitil*, a small copper coin: 'From this it seemed to us that they prized copper; and they also wore small beads of it in their ears' (Raven-Hart 1967: 4).

The identity of these men is not certain. Penis sheaths are not recorded as being part of the apparel of the Khoikhoi, which was a frontal flap or apron of dressed hide or skin (e.g. Kolb 1738: 187-191; Smith & Pheiffer 1988). It was considered that 'sheaths' might have been an incorrect translation or a wrong usage on the part of the original writer, but J. M. F. Morais (1990 *in litt.*) has confirmed that this is the correct translation of the Portuguese word *bainhas*. There is, however, evidence in the reports of early travellers (e.g. Moodie *ed.* 1960: 224) that the San men south of the river dressed similarly to the Khoikhoi. The Namaqua as seen, described and illustrated in Namaqualand in 1685 (De Wet & Pheiffer *eds* 1979: 145, 413,) did not wear penis sheaths. On the other hand, one of the illustrations accompanying the journals of the late eighteenth- century soldier and explorer Robert Jacob Gordon is of a Khoikhoi man wearing what the editors called a penis sheath (Raper & Boucher *eds* 1988, pl. 57). Unfortunately, however, there is no information as to this man's tribal affiliation or the locality in which the illustration was made. In another illustration (Raper & Boucher *eds* 1988, pl. 50), a man wears a similar piece of apparel, but this is not called a penis sheath. It thus seems that temporal and/or spatial differences may be involved as well as the apparent cultural ones.

Some of the information given about these people, that 'They eat only sea-wolves and whales and the flesh of gazelles [antelope] and the roots of plants' (Axelson 1954: 3; Raven-Hart 1967: 3) may, apart from seals and the roots of plants, be discounted as the interpolation of a later editor, since these details were not recorded as having been observed by either Dias or Da Gama - or Velho, the supposed author of the narrative of Da Gama's voyage (Raven-Hart 1967: 2); moreover, as mentioned above, the man first seen was collecting honey.

Metal, especially copper, appears to have been known to the Khoikhoi of the Cape Peninsula and vicinity, since they were eager to barter their livestock for it, but they apparently had none until the arrival of the Europeans (Goodwin 1956). This is surprising because St Helena Bay is only about 140 km from Table Bay as the crow flies and both bays seem to have been included in the annual migration route of the chief tribe of the area, the Cochoqua (Smith 1984). It may be that there was no contact between the people seen by Da Gama and his crew and those who lived to the south-east of St Helena Bay. In 1661 it was recorded that the Namaqua 'were always at variance' with the Cochoqua (Moodie *ed.* 1960: 228), and it may be that this enmity was of long standing and thus a barrier to barter or the exchange of gifts. However, the same record stated that the Namaqua 'were Hottentots like themselves [the Cochoqua], also dressed in skins . . . not wearing chains on the arms or neck, nor in the ears, but beads round the waist and copper bracelets'. The comment about 'chains' not being worn in the ears does not preclude the wearing of other ornaments, such as beads: just over a century later Gordon (or his artist) illustrated a family of Little Namaqua, the branch of the tribe who lived south of the Orange River, all of whom are depicted as wearing bead necklaces and pendant copper earrings (Raper & Boucher *eds* 1988: 377, pl. 79).

In 1657 it was recorded that the Namaqua were the source of all the indigenous copper, although it was not known at that time whether the metal originated in their territory or was obtained from other people further away (Moodie *ed.* 1960: 116).

This suggests that the men seen at St Helena Bay in 1497 were not members of the tribes seen by later voyagers and settlers in the Saldanha Bay-Cape Peninsula region. In 1661 travellers on an expedition, of which Pieter van Meerhoff was a member and its recorder, were the first settlers to meet the Namaqua, at a point somewhere north of the Olifants River. Meerhoff described the Namaqua as being abundantly adorned with copper in the form of beads, necklaces, some with pendant discs, and armbands. They also wore ornaments of iron and ivory (Moodie *ed.* 1960: 233). In May 1668 a Corporal Bosman at the Company's post at Saldanha Bay reported to Commander van Quaelbergen that a great number of Hottentots, called Namaqua, had stolen the stock of the local people as well as some belonging to the Company. Bosman was instructed to send men with a wagon and merchandise in an attempt to continue bartering with the Namaqua, towards whom they were to show no hostility. The following month Bosman reported that, even though the party had crossed the Olifants River, they had been unable to make contact with the Namaqua and had returned to base as they had run out of provisions (Wilson 1986*b*: 33).

The foregoing provides grounds, even if they are somewhat tenuous, for suggesting that the men seen by Da Gama were Namaqua. There is no proof that the stock thieves were actually Namaqua; but that they were identified as such does allow for the possibility that these people, whose territory lay north of the Olifants River, made excursions to the south. Where the Namaqua were located in 1497 cannot now be known; but it seems more reasonable to assume that they, the only people *known* to have copper, were the people seen by Da Gama and his crew than that the wearers of the copper beads were San or other Khoikhoi. Had it been otherwise, later voyagers would surely have commented on the fact that the people they met, at Saldanha Bay, Table Bay, and eastwards, wore copper beads and/or other ornaments of the same metal.

It was not until the expedition of 1685-6 under the command of Simon van der Stel that it was established that the source of the indigenous copper was the

Koperberge, or Copper Mountains (Moodie *ed.* 1960: 407), in the vicinity of present-day Springbok in the north-western Cape, some 350 km north of St Helena Bay. These are in the former territory of the Little Namaqua and almost certainly were the source of the copper beads the men of St Helena Bay wore in their ears; but it provides only a speculative clue as to the identity of the latter.

Nienaber (1989: 105-116) was unable to reach a firm decision as to whether the 'Amaquas' and the 'Amas' were the same as the Namaqua. *Inter alia*, he cited evidence in the journal of Van der Stel's expedition that, in the vicinity of Piketberg, Van der Stel had effected a reconciliation between the Amaquas and the Sonqua on condition that the latter restored to the former the cattle they had stolen (Valentyn 1971: 244; other sources under 6 September 1685). Nienaber also cited the map published by Valentyn (1971, facing p. 34) in which the Namaqua were shown as occupying the northern reaches of the Olifants River and east of the Little Grigriqua. However, this map, particularly with regard to the known distribution of the Khoikhoi tribes (e.g., Maingard 1931), is so defective as to be almost worthless as a source of information; and there is no evidence that the 'Amaquas' were actually present when Van der Stel effected the reconciliation, the Sonqua being the only people mentioned as involved in his negotiations.

The description of the men of St Helena Bay mentions that 'Their arms are staffs of wild olive trees tipped with fire-hardened horns' and, later, when the Portuguese went to the rescue of one of the crew, the men attacked them with 'assegais' (Raven-Hart 1967: 4). Maingard (1932: 712-713), discussing the historical evidence for the use of bows and arrows in South Africa, mentioned that the assegai was the weapon principally used by the 'Hottentots' for hunting and war, and suggested that the 'Sonqua . . . were the great bow-users as compared with the Hottentots', which could be taken as an indication that the men of St Helena Bay were not San. Nicholas Downton, who was at Table Bay in 1610 and 1613, said of the people there that 'In there hands they carrye a small Launce or Darte, that hath a small Iron head, . . . they have also bowes and arrowes, but when they came downe to vs they would leave them in some hole or bush by the way' (Raven-Hart 1967: 47-49). The identity of the people at Table Bay, from whom the English were able to barter cattle and sheep, is unclear, but it is possible that they were either Khoikhoi herders or their agents, the 'Strandlopers', whose identity is discussed in Chapter 3. On the basis of Maingard's and Downton's observations, the men of St Helena Bay could have been herders who had brought only their assegais as means of protection, but this is by no means certain, particularly as both Khoikhoi and San were said to have used both types of weapons (e.g. Thom *ed.* 1954: 211; Moodie *ed.* 1960: 158).

Other than the foregoing, it is only the number of the men, 40-50, that leaves room for speculation as to their identity. From information in the early records, that the Goringhaiqua had 'about 300 men capable of bearing arms' and the Gorachouqua 600-700 (Moodie *ed.* 1960: 247), Elphick (1977: 92) estimated that the total of the tribes in the region of the Cape Peninsula numbered between 4 000 and 8 000 men, women and children. On this basis, therefore, 40-50 men would be indicative of a group total of about 150-200 people including women and children. While this would not have been unusual for a small Khoikhoi tribe, or part of a larger tribe temporarily broken up, for example, to facilitate grazing their herds and flocks, it would have been unusual for a San hunter-gatherer band, the size of which was usually under 20 people in all (Inskeep 1978: 103). The numbers of 'Sonqua' seen by the early travellers were usually small; for example, when Van Meerhof travelled northwards along the inland part of the west-coast region in 1661, the number of men met ranged from one to twelve. He commented, however, on seeing 'many Sonqua houses', all of which were unoccupied and in some of which the party slept overnight (Moodie *ed.* 1960: 231). On their return journey, the party met 40 Sonqua (Moodie *ed.* 1960: 237), but it was not stated whether these were only men, or whether the number included women and children. Also, with regard to the estimates of tribe size given by Elphick, Van Meerhof observed that the (Little) Namaqua under Akembie consisted of about 300 men and 400 women and children (Moodie *ed.* 1960: 233). This would give a greatly different group size for the people of St Helena Bay from that based on Elphick's estimates: 50-70 people in all, which would not have been excessive for two or three San bands attracted to the area by the strange visitation.

Smith (1984: 139) considered significant the absence of any mention of cattle, 'since [the Portuguese] spent eight days careening their ship there and would have been delighted to exchange for fresh beef had it been available'. However, since Da Gama and the crews of his fleet were the first Europeans to call at St Helena Bay, it is not necessary to assume that they considered that the men they met there were culturally the same as the herders seen by Dias at Mossel Bay, several hundred kilometres to the east, and whom the second fleet encountered later (Raven-Hart 1967: 5-7). Here, the men soon brought their cattle and sheep to the shore and the following day were also accompanied by their women and children, though these 'remained on a hillock near the sea'. The behaviour of the people of Mossel Bay was thus manifestly different from that of the men of St Helena Bay.

The identity of the men of St Helena Bay has been discussed at some length, not because they are particularly important - if they, or their descendants, were encountered later, this cannot be determined from the records - but in order to show the problems inherent in using the often scanty information provided in the early records to try to identify the indigenous people and/or their culture.

LATER CONTACTS

For the next century and more, almost all the descriptions of the indigenous inhabitants of the Cape coastal region, from present-day Saldanha Bay to Plettenberg Bay, were of the pastoralists (Raven-Hart 1967: 8-41), the people now identified as Khoikhoi. However, John Jourdain, whose ship called at Table Bay (then still called 'Saldanha Bay') in July 1608, made mention of about 20 people or more in small 'cottages' made of branches, who had no stock of their own, but notified others inland of the coming of the ships, so that cattle and sheep were brought daily. Later in his narrative, Jourdain commented that the people, whom he called 'Saldanians', had feasted on the flesh of seals from which the English had cut the blubber to make train-oil (Raven-Hart 1967: 41-42).

Augustin de Beaulieu, who visited the Cape in March 1620 (late summer) and May 1622 (early winter) commented

> 'The inhabitants of this country towards the point of the Cape are, I believe, the most miserable savages which have been discovered up to now, since they know nothing of sowing or of gear for ploughing or cultivating the soil, nor anything of fishing or of going even two paces into the sea. They are of very low stature, especially the women, thin, and seem always to be dying of hunger. They eat certain roots, which are their chief food . . . they are pretty tasty. Also they go along the seashore, where they find certain shellfish, or some dead whale or other fish, however putrefied it may be, and this they put on the fire for a little and make a good meal of it. . . . We saw no huts where they could take shelter, as have other savages, and some of our men met them with their wives and children at the place where they had gone apart to pass the night, where they had no shelter other than bushes and some skins stretched on two crossed sticks, with another in the middle to thrust into the ground like a parasol, under which their wives and children set themselves, buried to the waist in the sand . . .' (Raven-Hart 1967: 100-101).

DISCUSSION

These observations, from the first 134 years of contact between the voyagers from Europe and the indigenous inhabitants of the coastal region, provide evidence of at least two groups with apparently differing resource bases. There were the people at Mossel Bay and Table Bay, who were seen to have cattle, and there were those at St Helena Bay and Table Bay, who were not. Although any determination of the identity of the men met by Da Gama and his crew at St Helena Bay in 1497 cannot be other than speculative because of the scantiness of the information, on the basis of what was stated it is clear that the economy of these people was not based solely on marine resources. The identity of the people described by Jourdain and De Beaulieu is also not clear, but these descriptions may be early references to the people later called 'Watermen', who are discussed in the following chapter. De Beaulieu's observations are typical of the generally prejudiced attitude of the voyagers towards the people they met, but they also provide evidence that the group he saw subsisted on terrestrial resources (plant foods) as well as on marine ones. It is necessary, however, to bear in mind that the observations of the early voyagers were based on only *partial* evidence obtained during relatively short visits: they are descriptions of what was seen at the time, not of the people's annual life-style.

CHAPTER TWO

STRANDLOPERS AND RELATED NAMES

WATERMEN

Johan Albrecht von Mandelslo, a passenger on the English ship *Mary* which called at Table Bay in May 1639, observed that

> 'The inhabitants of this country are of two sorts; some of which live very miserably by the waterside, but without ships or boats. They live on herbs, roots and fishes, especially on the dead whales, which must serve as their best food. They are called the *Watermen*, because they live by the shore. The other sort, which live further inland, are called *Saldanhamen*, from which this bight is called *Saldanha Bay*. They live somewhat better than the Watermen. They also do not cultivate the soil, which is excellent and produces all sorts of ground- and tree-fruits. But they have lovely cattle, sheep and goats. ... Both . . . these sorts live in small leaf-huts' (Raven-Hart 1967: 152).

Von Mandelslo's report errs in at least three respects. Saldanha Bay, an earlier name for Table Bay, was named after Antonio de Saldanha who, with his crew, in 1503 were the first Europeans to come ashore in the bay (Raven-Hart 1967: 8). The people were named for the bay, not vice versa. Secondly, the first goat recorded as having been seen at Table Bay was one sent in March 1661 by the Namaqua chief Akembie as a gift to Van Riebeeck (Thom *ed.* 1958: 343). Lastly, the 'Saldanhamen' were Khoikhoi (see below), whose hemispherical mat-and-withy huts were often described by the early writers (e.g. Kolb 1738: 220-222), although the possibility should not be overlooked that Khoikhoi out hunting, or herdsmen away from the main encampment, may also have used temporary shelters such as those described by Von Mandelslo. Such shelters are used today by herders in Namaqualand when they are away from their home base (W.J.J. van Rijssen 1986 pers. comm.).

The shelters described by Von Mandelslo were, however, similar to those described by Jourdain and De Beaulieu some two decades earlier (see above); and De Beaulieu mentioned that these were occupied by the women and children, who would not have been likely to have accompanied the men when they were out hunting or herding. Earlier callers had seen and described the huts of the herders (Raven-Hart 1967: 30, 46, 60, 66), so that there is no likelihood that the accommodation mentioned by Jourdain and De Beaulieu was inaccurately or pejoratively described. Despite Von Mandelslo's incorrect ascription of such shelters to the 'Saldanhamen', his observation reinforces those of his predecessors: that there were people whose housing differed from that of the herders and who subsisted on plant and marine foods.

Discussion of the 'Watermen' is continued in the following sections.

STRANDLOPERS

According to Elphick (1977: 83, footnote 41 - references in Elphick (1985) are generally identical to those in his earlier publication) the earliest unambiguous references to the Strandlopers are in a letter from the Dutch ship *s'Gravenhage* written in Table Bay on 2 December 1632, followed by the entry for 20 May 1636 in the log-book of the ship *Banda*, both of which are in the Algemeen Rijksarchief in The Hague.

The first published use of the name, however, appears to be that in the diary of Leendert Janssen, under-merchant on the *Haerlem*, which was wrecked in Table Bay on 25 March 1647. Janssen recorded on 29 March that a party of the survivors had set off overland to meet two English long-boats that had been sent ashore, but that some of them had been wounded in an attack by the Strandlopers. However, later entries in Janssen's diary show subsequent relations between the two groups to have improved. The Strandlopers were said to number about fifty men, women and children, and one of the men spoke English (Raven-Hart 1967: 168-169).

Raven-Hart (1967: 169) identified the man who spoke English as 'Hadah', who was taken by the English to Bantam in the East Indies in 1630 or 1631 and brought back the following year (Raven-Hart 1967: 127, 136). This man was later to become known to Jan van Riebeeck and the Dutch garrison on the shore of Table Bay as 'Harry' (Thom *ed.* 1952: 71) or 'Herry' (Moodie *ed.* 1960: 14, footnote 1). Again for the sake of consistency, 'Herry', the name by which this man is best known, is used in preference to the less commonly known but probably more correct 'Harry', although his name was actually Autshumao (Moodie *ed.* 1960: 135).

In 1613, Captain Towerson of the *Hector* took two of the local inhabitants of the Table Bay area on board with the intention of taking them to England. One died during the voyage, but the other was returned to the Cape the following year (Raven-Hart 1967: 54, 64). This man, known to the English as 'Cory' or variants thereof, acted for several years as the principal agent in the bartering of livestock with the Khoikhoi, and appears to have been a Khoikhoi. According to Walter Peyton, who was at the Cape in 1615, Cory's 'village' was eight miles (approx. 13 km) from the bay and consisted of 100 huts; and there was another consisting of 10 huts on the east side of Table Mountain (Raven-Hart 1967: 72). The description of the huts is identical to those of Khoikhoi huts (e.g., Kolb 1738: 221-222); and, although it is not specifically stated, it appears that the sheep and cattle that were bartered came from the inland village.

Cory seems to have disappeared from the scene around 1627 (Raven-Hart 1967: 120), and Herry commenced his duties as 'postman' and intermediary in the bartering by at least 1632 (Raven-Hart 1967: 137 ff.). It seems unlikely that this Cory was the same man as Choro, the chief of the Gorachouqua, nicknamed 'tobacco thieves' by the Dutch, who met Van Riebeeck in May 1660 (Moodie *ed.* 1960: 207) and whom Nienaber (1989: 682) considered to have been the eponymous ancestor of a branch of the Korana. In 1614 Nicholas Downton recorded that Cory 'departed from us carrying with him his Copper Armour and Javelin, with all things belonging to him' (Raven-Hart 1967: 66). Cory learned some English during his captivity in England (Peyton 1615 *in* Raven-Hart 1976: 72), of which Van Riebeeck would surely have been apprised, even if the astonishment caused by Cory marching around in his copper armour had long been forgotten - it was, after all, forty-six years later. Choro was last mentioned in the official records in 1662 (Moodie *ed.* 1960: 247), by which time, if he were in fact Cory, he would have been well over sixty years old. The indications are, however, that Cory, unlike Herry, was a member of one of the recognized Khoikhoi tribes.

Peter Mundy, whose ship called at Table Bay in May 1634 said of the people 'Theis that are hereabouts (by report) are of a baser Sort and live in feare of others called Saldanhamen, whoe are further in the Land'. Herry, meanwhile, with about 60 men, women and children, 'better apparelled than those living on the Maine, though after the same manner', was living on Robben Island, in '7 litle Cottages' (Raven-Hart 1967: 141, 143). This report is ambiguous in that it suggests that there was yet another group besides Herry's people and the 'Saldanhamen', the latter at that time being the Khoikhoi *sensu lato*.

On 13 November 1652 Van Riebeeck questioned Herry about the inhabitants of the area. Herry told him that

> 'this Table Valley was annually visited by three tribes of people, similar in dress and manner . . . namely, themselves, the Strandlopers, or as they call themselves in the broken English they have learned, *Watermen*, because they live on muscles [*sic*], which they find on the rocks, and some roots out of the ground, without always having cattle - and who, as far as we have as yet ascertained, are not above 40 or 50 in number' (Moodie *ed.* 1960: 25, footnote).

It is not clear whether 'without always having cattle' means that they sometimes did have them, or whether this is merely the writer's (or translator's) literary style. The other two groups were the 'Saldanhamen', later identified as the Cochoqua (Moodie *ed.* 1960: 247), and the 'Vismans', the latter of whom are discussed below.

In a despatch to the Here Sewentien (the Council of the Dutch East India Company) dated 5 March 1657, Van Riebeeck referred to 'the Watermen or Strandlopers (a poor tribe with whom Herry used formerly to live . . .)' (Moodie *ed.* 1960: 94), which suggests that Herry had severed his connection with the group. However, on 2 July 1658 Herry was described as 'nothing but a naked beggar and merely the captain or head of the Watermen or Strandlopers hereabouts' (Moodie *ed.* 1960: 131).

Herry and his people were identified in October 1657 as being of the Choeringaina tribe (Thom *ed.* 1954: 172), the name being given the following month as Goeringaina or Goeringaijqua (Thom *ed.* 1954: 184). However, in the list of tribes left by Van Riebeeck in 1662 for his successor, Herry and his people were named as the Goringhaicona, the Goringhaiqua being identified as the larger tribe of 'Caepmans' whose chief was Gogosoa (Moodie *ed.* 1960: 247). In the lists of 1657, but not that of 1662, the Goringhaicona (correcting the original error) were said to comprise Herry, his people, 'and all the Watermen' (Thom *ed.* 1954: 172, 184).

The records continued to be inconsistent in their use of the two names, for example: 'as soon as Herry has been sent to the [Robben] island, peace shall be offered to all his people *and also* the Watermen' (my emphasis); 'all the Strandlopers and vagabonds of Watermen'; 'the said men, who were all well known, and were accustomed to live at the Fort, called out to our people that they were Watermen, alias Vismans'; 'as to the Vismans or Watermen, such poor naked creatures dared not go anywhere but must keep under the protection of the Company' (Moodie *ed.* 1960: 136, 170 footnote, 185, 188, 193, 217).

Dapper (1668 *in* Schapera *ed.* 1933: 9) stated that 'The Goringhaicona or Watermen are under a chief, whose native name is Demtäa, but our countrymen call him Klaas Das . . . They have a camp of only four or five huts, number about fifty souls with women and children, and are the poorest of all the Hottentots'. Schapera

(1933: 8-9, footnote 6) said that he had been unable to find any evidence in the official records to support Dapper's statement that Klaas Das was the leader of the Goringhaicona, and that his 'native' name was Khaik Ana (or Khaikana) Makouka. Moodie (*ed.* 1960: 136, footnote 1, 188) gave his name as both Humthasoankhumma and Kaikana Makoukou. I have likewise been unable to find any confirmation that Klaas Das was the leader of the Watermen or the Goringhaicona. He was generally described in the official records as one of the interpreters (Moodie *ed.* 1960: 70-71, 77-78, etc.) and once as 'one of the fishers, or Watermen' (Moodie *ed.* 1960: 198).

VISMANS

The Vismans ('Fishermen' is probably more correct than the literal translation, 'Fishmen') are another source of confusion. In the previously-mentioned journal entry of 13 November 1652, when Herry told Van Riebeeck of the three groups that visited Table Bay annually, he said that, in addition to the Strandlopers/Watermen and 'Saldanhamen' (Cochoqua),

> 'the third sort was called by them *Vismans*, who after the departure of the Saldanhamen come here with cows only, and without sheep, and who subsist by fishing, without boats, by lines from the rocks - who are from 400 to 500 in number; [against] these the Watermen and Saldanhamen . . . are at constant war . . . he told us that those Vismans always travelled secretly, concealing themselves from the Saldanhamen, and not like them lighting fires, for if the Saldanhamen, who are of countless numbers, perceived their fires, they always tried to catch them, for which the Vismans were much afraid, residing beyond the mountains Eastward of the Cape, towards the bay of Sambras [Mossel Bay], and in that direction' (Moodie *ed.* 1960: 25-26. The interpolated 'against' is taken from Moodie's footnote 1 on p. 59).

The Cochoqua told Van Riebeeck that the Vismans were 'all robbers, who would come here and try to steal our cattle' (Moodie *ed.* 1960: 26), an accusation supported by Herry, who also called them 'Sonqua' (Moodie *ed.* 1960: 28) In 1654

'Herry's allies', probably the Goringhaiqua, complained to Van Riebeeck that 'the Vismans (called by them Sonqua) had robbed them of all their cattle' (Moodie *ed.* 1960: 46). The following year a party sent into the interior under the command of Jan Wintervogel met 'some Sonqua, called Vismans, and enemies of Waterman and Saldanhaman, who had no cattle'. These were a different group from those described later in Wintervogel's report as 'a certain tribe, very low in stature, and very lean, entirely savage, without any huts, cattle, or any thing in the world, clad in skins like these Hottentots, and speaking nearly like them' (Moodie *ed.* 1960: 59). The Vismans were, however, never seen at Table Bay and were last mentioned in 1660 (Moodie *ed.* 1960: 217).

From the foregoing, it is evident that the name 'Vismans' was applied to, and used by, the Goringhaicona and also to the people, 400-500 in number, called cattle thieves and Sonqua by Herry, the Cochoqua and Goringhaiqua. Whether the 'Sonqua, called Vismans' mentioned by Wintervogel were the same as the 'cattle thieves' is not clear. That they had no cattle is perhaps not important: they could have disposed of them, or lost them, to another tribe, especially since they were accused of being cattle thieves. On the other hand, they may simply have been a party separated from the main group, which might have gone into hiding so as to conceal their cattle from the Europeans. Their leanness suggests, however, that they were not people who benefited from the possession of domestic stock.

Maingard (1935: 486-487) was of the opinion that the Vismans 'were the same people as the *Ubiquas* who are also called *Sonquas*' (Maingard's emphases). He disputed the etymology given by Theal (1897: 173) as 'murderers, presumably from the Hottentot *//o:*, "to die" ' and stated that 'Since *Ubiqua* is equated with the "Fishmen", it should be derived rather from Hottentot *//au*, "fish" + *ube*, "to take away" + *kwa* [*sic*] (plural suffix)'. This seems somewhat circular reasoning. Although there appears to be some validity in Maingard's equating 'Ubiqua' with 'Sonqua', both having been used with reference to hunter-gatherers without domestic stock, at least one group of 'Vismans' had cattle, and were only called 'Sonqua', not 'Ubiqua', as indicated in the excerpts quoted above.

Nienaber (1989: 898-901) attempted to identify the Vismans, chiefly from the sources cited above. He pointed out that the people met by Wintervogel were encountered west of the Drakenstein Mountains, in the vicinity of present-day Tulbagh, an unlikely locality in which to find people who were alleged to subsist by fishing, although the possibility that they caught river fish cannot be excluded. Nienaber also drew attention to the absurdity or incongruity (ongerymdheid) of Herry's assertion that the relatively large band of Vismans, 400-500 in number, *subsisted* by fishing, when they also had cattle, though he was unable to determine from the scanty information whether these people were only cattle-thieves or whether they were herders.

Nienaber (1989: 901, in translation) was of the opinion that 'Ethnonymically, the name *Visman(s)* indicates a necessary connection with fish, [and] in the explanation of this specific reference is made to sea-fish, so that the location always assumes a beach or coast within easy reach'. His conclusion was that 'The identification of [the] *Vismans* remains the great problem. If they did not disappear [after 1660 - see above], under what name are they to be found again? The problem is thus more ethnic than ethnonymic.' He found interesting the inference of Goodwin (1952: 142) that the 'Strandlopers and Fishmen . . . were Hottentots and Bushmen, herding and hunting people respectively who had turned for part of the year at least to the abundant sea-food to augment their normal sources of subsistence', but considered that Goodwin, too, had not answered the basic question, of the *specific* identity of the people called 'Vismans'.

STRANDLOPERS (continued)

Although it seems clear that the Strandlopers and Watermen were one and the same, Herry's association with, and leadership of them remains problematic. In

1659 a man called Trosoa was mentioned as chief of the 'Vismans alias Watermen' (Moodie *ed.* 1960: 188), and in January of the following year Herry was said to be living with the Little Chariguriqua in the vicinity of Saldanha Bay (Moodie *ed.* 1960: 199). In a despatch to the Here Sewentien dated 16 March 1660, Van Riebeeck reported that during a skirmish with a band of stock-thieves three were killed, 'one of the killed being the chief of the Strandlopers, named Trosoa' (Moodie *ed.* 1960: 203). As mentioned earlier, Herry's actual name was Autshumao (Moodie *ed.* 1960: 135), and in 1666 it was reported that he had died three years previously, the report adding that he had been 'chief or captain' of the Goringhaicona (Moodie *ed.* 1960: 291).

It seems likely that the Dutch perception of Herry as leader of the Goringhaicona was the view he presented to them. On the other hand, it is possible that Trosoa was the *de jure* chief of the tribe (assuming that the Goringhaicona were a proper tribe and not the collection of outcasts Theal (1897: 155) and Maingard (1931: 492) assumed them to have been), while Herry, by virtue of his influence with the Dutch and some of the Khoikhoi tribes, was their *de facto* leader. The Goringhaicona, although they had a Khoikhoi 'tribal' name, seem to have been no more than a loosely-knit group that fluctuated in size (e.g., Moodie *ed.* 1960: 291), and of which the leadership was probably nominal rather than actual (see Elphick 1977: 103-106). Herry's association with, or separation from them was doubtless related to the fluctuations in his relationships with the Dutch and the (other) Khoikhoi. Theal (1897: 155-156) stated that after Herry's death in 1663 the 'Hottentots residing permanently in Table Valley . . . about eighty souls . . . were nominally under the government of Jan Cou'. Jan Cou, alias Khamy, Chamy or Khuma, was the third son of Gogosoa, the chief of the Goringhaiqua (Moodie *ed.* 1960: 133, 135, 137, 141), but I have been unable to find any support in the official records (Moodie *ed.* 1960) for Theal's claim.

In October 1652 the bookkeeper Hendrik Verburgh was a passenger on the Company's yacht *Goede Hoope* that called at Saldanha Bay. Here they met on the shore some 'Hottentots', and 'firmly believed them to be Strandlopers, as they had no cattle or sheep or elephants' tusks . . . they were all very lean and thin'. They visited the yacht again the following day, but 'these poor people had nothing to barter but tortoises, ostrich egg shells . . . and similar trash' (Thom *ed.* 1952: 93). Two days later, after finding a kraal containing fresh manure, the Dutch fired a gun, after which some men arrived. 'They were three fat, sleek and robust fellows, quite friendly, and we could understand them better than we could the Strandlopers' (Thom *ed.* 1952: 95). The men promised to bring sheep, but did not do so; and the Dutch could find no other people in sight when they climbed a 'high mountain'. This report is interesting in that it provides some evidence of two different groups in the vicinity of Saldanha Bay: the emaciated people with no domestic stock and little of value (to the Dutch) to barter; and the well-fed men, who apparently had access to sheep, even if they did not bring them. Whether the comment about the Dutch being able to understand the second group better than they could the first refers to language or mime (see below) is not clear; but, in the former case, it may indicate linguistic or dialectal differences between the two groups.

In January 1653 Verburgh was again at Saldanha Bay and again encountered 'some Strandlopers. We asked them whether they would help us kill *hercas* [seals] and have the meat to eat, but understood that they did not want to do any work for it'. A day or two later, the Dutch bartered 25 hippopotamus tusks from the 'Strandlopers' for some tobacco. Asked, with signs, whether they had any elephant tusks, 'they shook their heads, indicating that these animals were too large and powerful to be attacked by so few of them' (Thom *ed.* 1952: 134-135). This report reinforces the impression gained from the first, that there were people at Saldanha Bay who were not herders and who were few in number; but if they were people who would otherwise be classified as hunter-gatherers, it is difficult to understand why they were reluctant to help kill seals, which would have provided them with a good supply of meat more easily than if they had done the trapping themselves. The size of the group was not mentioned but, as mentioned above, there were too few men to engage in the dangerous task of hunting elephants, so that their number was probably small.

Avery (1976, table 3) listed ten occasions on which the 'Strandlopers' were recorded as having been seen elsewhere than at Table Bay, three of which were the occasions mentioned above. In January 1655 Jan Sijmonssen, captain of the galiot *Roode Vos*, reported from Saldanha Bay that he had been there for ten days, waiting for the 'natives', presumably to barter stock, but they had been visited daily only by the Strandlopers, who had brought them one 'beast . . . but there were no sheep to be obtained from them' (Thom *ed.* 1952: 283). The following month, Verburgh reported that 'he had traded only 2 head of cattle and 10 sheep from Strandlopers who, he thought, had stolen [them] from other natives' (Thom *ed.* 1952: 297). In October 1655, Corporal Muller, who was on a bartering expedition east of the Table Bay settlement, reported that at their camp on the False Bay coast (identified by Avery, 1976, table 3, as near Gordons Bay) Herry had been visited by eight 'strange Hottentots', who were later identified as being 'the Strandlopers who were formerly at the Fort bartering cattle' (Moodie *ed.* 1960: 73). There are apparently only three other records of Strandlopers being seen elsewhere than on the Cape Peninsula. Again, these were at Saldanha Bay, in 1659 and 1660 (Moodie *ed.* 1960: 195, 202, 212) but the reports are inconclusive. The first, in November 1659, reported that the Strandlopers had informed the Dutch that the Cochoqua had retired inland. The second, in March 1660, merely referred to 'naked Strandlopers' and the last, in July of the same year, stated that there was nobody but Strandlopers there at that time. An earlier report, in August 1659, mentioned 'shore people, who lived by fishing', also at Saldanha Bay (Moodie *ed.* 1960: 190). The Dutch text of the Van Riebeeck journal (Bosman & Thom *eds* 1957: 131) uses the term *strantluyden*, which in the English version (Thom *ed.* 1958: 129) is translated as 'beach rangers'). The use of 'Strandlopers' in these references may possibly be a precursor of the manner in which 'Hottentots' - those with domestic stock - and 'Bushmen' - those without - were identified in later periods. In the cases mentioned above, people who were at the coast, for whatever reason, and had no stock were *de facto* 'Strandlopers' as far as the Dutch were concerned. That the Strandlopers at Saldanha Bay in 1655 had some few cattle and sheep gave rise to the suspicion that these had been stolen, presumably because of the general belief that the only natives who could legitimately possess domestic stock were the 'real' Khoikhoi: people who were recognizably herders.

Elphick (1977: 134-135) considered plausible the reconstruction by Vedder (1928: 114) of the tribal name Chariguriqua as *ǂkari-huri-qua*, meaning 'Lesser Sea People' and cited a personal communication from S. Marks 'that the original Guriqua may have been a Strandloper group which acquired livestock: this theory would explain their name (Sea People), their comparative poverty, their political decentralization, and the fact that other Khoikhoi occasionally referred to them as "San"'. That the name Guriqua/ *huriqua* means 'Sea People' receives some support from the name of one of the branches of the Nama ǂAonin, 'Hurĩnin', which also has the meaning 'sea people' (Budack 1977: 12 - see Ch. 10). However, Nienaber (1989: 437, translated) commented

> 'We are reasonably well informed as to their positions, [but] the Chariguriqua did not really live so close to the sea, compared with, for example, the Saldanhars [Cochoqua] or Kaapmans [Goringhaiqua] that this [would] make them distinguishable in name, and even less so in the case of the Grigriqua. It seems to us better rather to consider the measure of resemblance between the *Guri-* of the name and the *Huri* [i.e., the Hurĩnin] as a coincidence, although the *g-* and the *h-* of the various words are in some cases dialectally interchanged' (my interpolations).

The only reference I have been able to find of the Chariguriqua being called 'San' is in the journal of the journey of Jan van Harwarden, who was sent with a party early in 1658 to see if they could ascertain whether fires in the distant mountains signalled the seasonal approach of the Khoikhoi from the north-west (Moodie *ed.* 1960: 119, footnote 2). On 10 March, apparently somewhere in the vicinity of present-day Malmesbury, they saw some cattle and asked the natives who had accompanied them who were the owners of these cattle. They were told that the people were Sonqua, but when they went to their kraal 'found that they were the same Hottentots with whom the Fiscal [Abraham Gabbema] had formerly been, named Charingurinas' (the -na suffix has the same meaning as -qua, people), and that the small group of five huts was merely an 'advance party' of the main group, which was still some distance away (Moodie *ed.* 1960: 122). Whether this incorrect identification of the Chariguriqua as San was deliberate or not cannot now be determined, although it could have been expected that Khoikhoi would recognize the huts used by the other people as being like their own. 'Sonqua' may, however, have been used intentionally as a term of contempt, especially if Van Harwarden's guides were Cochoqua or their allies. In 1662 Van Riebeeck recorded in the memorandum left for his successor that the Little Chariguriqua were

> 'subject to Oedasoa [the senior chief of the Cochoqua] though they have rebelled against him; they were accustomed to be his stock keepers but appropriated his cattle to their own use; and therefore they are not recognized by any of the Hottentots as a people who have a Choeque or Hunque, that is a hereditary king or chief' (Moodie *ed.* 1960: 247; my interpolation).

On the basis of the claim that the Little Chariguriqua were herdsmen for the Cochoqua, it is possible to suggest that they were former San who had improperly acquired stock and thus became herders in their own right. On the other hand, given their relatively small numbers - Van Riebeeck estimated the tribe to be 'a people about as numerous as the Goringhaiqua', who had 'exclusive of women and children, about 300 men capable of bearing arms' (Moodie *ed.* 1960: 247) - it is also possible that they had earlier lost their stock to one or other of the Khoikhoi tribes and had been forced into a subservient position with the Cochoqua. The suggestion that they might previously have been robbed receives some support from the record of the first meeting in 1657 between the settlers and the 'Charigrina', as they were then called: 'that tribe had all fled, out of fear that they would be robbed' (Moodie *ed.* 1960: 109). Their flight could, of course, have been prompted by guilty consciences if the stock they feared to lose was actually the property of the Cochoqua.

The uncertainty of the Dutch with regard to the identities of the various groups of people in the area is exemplified by the entry for 21 March 1658 in the Company's journal dealing with Harwarden's journey, in which it was recorded that the party had 'met 3 or 4 small parties of natives (*Negryen*) and also the Swarte Captain, being all of the Chariguriquas' (Moodie *ed.* 1960: 120). The 'Swarte (Black) Captain' was Ngonomoa (or Gonnema), one of the two chiefs of the Cochoqua (Moodie *ed.* 1960: 148, 182, 199, 214).

The name 'Strandloper' was last used in the official records on 1 March 1681 (Moodie *ed.* 1960: 376, footnote 2), and it is likely that by that time most of them would either have gone into the service of the settlers or have joined other tribes. Kolb, who was at the Cape from 1708 to 1713 and published one of the most extensive early treatises on the indigenous peoples, did not mention either the Strandlopers or the Goringhaicona in his description of the Khoikhoi tribes (Kolb 1738, ch. 6). However, Valentyn, who spent a total of about six months at the Cape between 1685 and 1714 (Serton *in* Valentyn 1971: 9), listed among the tribes 'the *Goringhaicona* or Watermen' (Valentyn 1973: 61; his emphasis). Elsewhere, he observed that

> 'Those who have no herds, and especially the Kaapmans and the Strandlopers who live near the Castle, were accustomed, before the smallpox ruled so strongly in 1713, to do very great service to the Burghers in their corn- and grape-harvests, and to their wives in the households . . . but since then they have for the most part disappeared, although in 1714 I still saw Hottentot men and women here and there, the latter often busying themselves with the digging out of . . . little roots' (Valentyn 1973: 75).

Valentyn's mention of the 'Kaapmans' (Goringhaiqua) not having cattle at that time, and his vague use of 'Hottentots' - then applied to the Khoisan in general - suggests that by then the name 'Strandloper' was just a catch-all used to describe the various impoverished Khoikhoi (and possibly San) who lived in and around the Table Bay settlement.

The 'tribal identity' of the Goringhaicona is discussed in the following chapter.

CHAPTER THREE

THE GORINGHAICONA: KHOIKHOI OR SAN?

THE SOURCES

According to Maingard (1931: 492), the Goringhaicona were originally part of the Goringhaiqua but had been 'for some reason, unknown, but not inconsistent with Herry's scoundrelism, outlawed from the larger tribe'. There was, none the less, constant interaction between the two groups, the Goringhaiqua often being called 'Herry's allies' by the Dutch (Moodie *ed.* 1960 *passim*: see Index, p. v, under 'Caepmans'). Nienaber (1989: 409) commented that Herry acknowledged the overlordship of Gogosoa, the Goringhaiqua chief, and that Eva was kin of the latter. Nienaber did not cite his original sources, and I was unable to find them.

Elphick (1977: 94, text and footnote 13) dismissed as 'misleading' the suggestion by Stow (1905: 245) that the Strandlopers were, or had been, Bushmen, observing instead that 'The Goringhaicona belong to that group of peoples . . . which are Khoikhoi rather than aboriginal in language and culture'. 'Aboriginal' is one of the terms that Elphick (1977: xxi-xxii) used in preference to 'San'.

Stow (1905: 245) considered that the description by Oedasoa, the senior chief of the Cochoqua, of the Goringhaicona as murderers and stock-thieves who preyed on the other tribes when they were at their weakest, was

> 'so at variance with the more indolent mode of life in which the normal Hottentot was so prone to indulge, that one feels almost forced to the conclusion that these marauders must have belonged to the more energetic Bushman race, who harboured a feeling of revenge against the pastoral intruders into their ancient territories'.

There is, however, abundant evidence that the Khoikhoi also murdered one another and stole each other's stock when they could (e.g. Thom *ed.* 1952: 127), so that these qualities were not restricted to the San, as Stow suggested, and consequently do not serve to support his identification of the Goringhaicona as San on these grounds.

Elphick (1977: xxi-xxii) considered 'a Khoikhoi to be *any person accepted as a full* (i.e. not a subordinate) *member of a Khoikhoi community.* A Khoikhoi community was *one where a dialect of the Khoikhoi language was spoken and where pastoralism was the preferred mode of economic life*' (his emphases). He also commented that 'Goringhaicona is usually construed as "children of the Goringhaiqua", since */kona* means "children"' (Elphick 1977: 94, footnote 13). Nienaber (1989: 409-410, 419, translated) gave the meaning of the name as 'children of the high kraal' and commented '"children" has here a special connotation for those "who are like children, immature, humble in status, and live down there on the beach, not here, up high"', that is, on the higher ground away from the shore where the Goringhaiqua, the 'people of the high kraal', lived.

The Khoikhoi have a classificatory kinship system, in which the relative ages of the speaker and the person addressed are taken into consideration (Schapera 1930: 230-231), as Elphick (1977: 107) was aware. This being the case, it is reasonable to suppose that the Goringhaicona, as 'children' of the Goringhaiqua, were subordinate to them, and thus not accepted as full members of that tribe, perhaps even of the Khoikhoi. This suggestion is reinforced by the translation of 'Khoikhoi' as meaning 'men of men, i.e. men *par excellence*' (Hahn 1881: 2; but see Wilson 1986*a*: 253, Nienaber 1989: 619-622). Amongst people who considered themselves *men*, those they called *children* must have had an inferior status.

G. Klinghardt (1988 pers. comm.) has drawn attention to the fact that when tribes split, the breakaway group retains the tribal name and is still recognized as part of the main tribe. However, such groups were always referred to as 'little', for example, Little Chariguriqua, Little Namaqua, not as 'children'. As mentioned above, Elphick (1977: 134-135) stated that the prefix *ǂkari-* has this meaning of 'little', which is confirmed by the identical translation by Nienaber (1989: 248). This suggests that a special circumstance applied in the case of the Goringhaicona, and that they were not considered as a group normally separated from the Goringhaiqua, and enjoying junior status within the whole tribal body. Had this been the case, their tribal name would rather have been something like 'Charigoringhaiqua'.

That Herry's niece Eva (Krotoa) had a sister who was a wife of Oedasoa, one of the chiefs of the Cochoqua (Moodie *ed.* 1960: 145), is also no certain indicator of the Goringhaicona being Khoikhoi. Oedasoa's wife was a 'prize of war', having been taken from Goeboe, son of Sousoa, the Chainouqua chief (Moodie *ed.* 1960: 217). Elphick (1977: 107) may have been correct in suggesting that the terms 'niece' and 'sister' were classificatory rather than actual, although he supposed that Eva's 'mother', who lived with the Goringhaiqua (Moodie *ed.* 1960: 145), was her biological parent; but this does not prove that Herry or any of the women were Khoikhoi. There is no evidence that the Khoikhoi considered themselves too superior to take San women as wives: the Korana accepted former 'Bushmen' into their tribe (Engelbrecht 1936: 17), although such a practice need not necessarily have been the case in earlier times. However, since Elphick (1977: 11) considered that 'one or several hunting bands - consisting of Central "Bush" speakers in or near northern Botswana - acquired stock and became by that act the first Khoikhoi' it is arguable that the Khoikhoi were biologically the same as the San, and that any reason for their not mixing would, in the later period at least, have been social rather than biological: status-related, that is, rather than arising out of a distaste for miscegenation - not, as far as I know, that the latter has ever been suggested.

The Hessequa, Cochoqua, Namaqua and Inqua are amongst the tribes mentioned in the early records as having from time to time used the services of people they called 'Sonqua' or 'Obiqua' (Moodie *ed.* 1960: 324, 342, 410, 436), who appear to have been hunter-gatherers rather than pastoralists; and there seems no good reason why the Goringhaicona were not also in this category. In this connection, the observation by Jourdain (see above) - that the people he met on the shore of Table Bay in 1608 had no domestic stock of their own, but notified those inland of the arrival of the ships, who then brought the animals - tends to support the suggestion that the Goringhaicona were subservient to the Khoikhoi, and possibly had a clientary relationship with the Goringhaiqua.

That the Goringhaicona had a Khoikhoi 'tribal' name (Elphick 1977: 94) and were not called 'Sonqua' or 'Obiqua' is, although an apparently unusual circumstance, also not necessarily an indication that they were Khoikhoi. Because of their location close to the Dutch settlement and their interaction with the settlers, the Goringhaicona were the best-described non-herder group, but it does not follow from this that the people called 'Sonqua' or 'Obiqua' did not also have other names bestowed on them by the Khoikhoi with whom they associated. Isak Schryver, who journeyed to the Inqua in 1689, recorded meeting 'a party of Hottentots called *Hougliquas*, and *Sonquas* or *Thonuny*' (Moodie *ed.* 1960: 437 - Moodie's emphases), so that it is likely that other individual bands did have specific names. As Elphick (1977: 24) observed, the names 'Sonqua' and 'Obiqua' referred 'not to specific bands but to a *category* of people scattered all over southern Africa' (my emphasis).

That the Goringhaicona spoke a Khoikhoi dialect is also no sure criterion of their being Khoikhoi: any group in a subordinate position will - *must* - learn the language of its superiors, as is evidenced by the fact that Herry learned English, and probably also Dutch. The Dama of Namibia, a people of Negro origin who long had a servile relationship with the Nama, speak the language of their former overlords (Schapera 1930: 3; Hiernaux 1974: 110; also various authors cited by Nienaber 1989: 290-307).

That pastoralism was 'the preferred mode of economic life' is not disputed. In 1653 Herry and some of his band were implicated in the murder of one of the Company's herd-boys and the theft of all the Company's small herd of cattle

(Moodie *ed.* 1960: 35-38); and the early records provide abundant evidence that the Goringhaicona had associated with herders long enough for them to have appreciated that ownership of domestic stock was an indicator of status, and thus desirable, especially for people of evidently low status like the Goringhaicona.

However, apart from the language they spoke and their desire to own cattle, there is no evidence in the early records that the culture of the Goringhaicona was specifically Khoikhoi. Indeed, Elphick (1977: 94, footnote 13) himself stated that 'They belong to the category "hunter-gatherers" . . . The term *Strandloper* usefully denotes a subgroup of hunter-gatherers, namely those with a predominantly scavenging and gathering economy' (his emphasis). Although Elphick (1977: xxi-xxii) preferred not to use the name 'San' for various reasons, this, or its variants, was the name generally applied by the Khoikhoi to hunter-gatherers or those without domestic stock (see Wilson 1986*a*: 254-256) and it is thus arguable, on the basis of the foregoing, that the Goringhaicona should be identified as San rather than as Khoikhoi. The processes whereby herders lost their stock and had to revert to being hunter-gatherers (and scavenging seems to have been part of this economy), and former hunter-gatherers acquired stock and thus became pastoralists have been discussed by a number of writers (e.g. Wilson 1969, Marks 1972, Elphick 1977, 1985, Schrire 1980); but, while it is generally necessary to avoid a rigid application of the Khoikhoi = herder, San = hunter-gatherer dichotomy (Wilson 1986*a*: 261-264), it is on occasions legitimate to use these equivalences when dealing with the peoples of the early historical period, and probably also some of those in the prehistoric period.

The foregoing tends to show that Elphick's assertion that the Goringhaicona were Khoikhoi is not supported by the evidence of the early records or, for that matter, by the arguments he adduced in support of his claim. In July 1656 the Goringhaiqua said of Herry that he was 'a good-for-nothing and was already conspiring with the robbers, named Sonqua, with a view to enlarging his own following' (Thom *ed.* 1954: 50). Neither Herry nor the Goringhaicona were ever recorded as having been called 'Sonqua' by the Khoikhoi, and this seems to be the strongest, perhaps *only*, argument in favour of their having been Khoikhoi. The accusation that Herry was attempting to recruit Sonqua into his band is interesting in that it suggests that, even at that time, there were San bands close enough to the Cape Peninsula for Herry to be able to do so. Regrettably, however, this matter was not elaborated in the official records.

DISCUSSION

The way of life of the Goringhaicona, the 'Strandlopers' of the early Dutch records, was clearly not typical of that of either the Khoikhoi herders or the San hunter-gatherers. Unlike Cory, his predecessor as 'postman' and intermediary in the bartering of livestock, Herry was - if only by default - not a herder, nor did he and his people live in kraals as a matter of course, as did Cory and the Khoikhoi. Whether the Goringhaicona were outcasts of other Khoikhoi tribes, as suggested by Maingard, and possibly augmented by San, or whether they were San 'clients' of the Goringhaiqua is something that, on the basis of the documentary evidence presently available, cannot be established with certainty.

As was mentioned at the end of Chapter 1, while it is probable that observations such as those of Von Mandelslo, that the people lived on shellfish and the flesh of beached marine mammals, were correct, this evidence is only *partial*: it was not based on daily and round-the-clock observation. It is, moreover, based on a European view of what constituted a 'decent' way of life and, from that point of view, biased. However, even the sparse information provided by the travellers and settlers of the time shows that the Goringhaicona included plant foods in their diet and that they had access to the flesh of sheep and cattle. They, therefore, did not live solely by 'strandloping'.

It may be suggested that the Goringhaicona, whatever their tribal identity, were a group that, under Herry's leadership, saw the advantages of living close to the shores of Table Bay, where they could be the first to avail themselves of the benefits to be derived from contact with the passengers and crews of the ships that called there. At first, this may have caused an occasional change in their way of life (whatever that may have been), for example, when ships called; but after the establishment of the Dutch settlement in 1652 they took up more or less permanent residence in the vicinity. There, when Herry's conduct did not alienate the Dutch, they made themselves useful to the settlers, who provided them with protection against the Khoikhoi when Herry's conduct had turned the latter against him and, consequently, the rest of his band; and through the agency of Herry, Eva and Doman as interpreters and facilitators in the trading between the Khoikhoi and the Dutch, the Goringhaicona reaped the benefits of their association with both groups.

Etienne de Flacourt, who visited Saldanha Bay in October 1648 commented 'All those who come to these coasts are merely the poor slaves of others, who are the masters and possess cattle, which they have in great numbers' (Raven-Hart 1967: 174). While calling them 'slaves' may not be quite accurate, this is an apt assessment of the status of the Goringhaicona and other 'strandlopers'.

CHAPTER FOUR

'STRANDLOPERS' IN THE LATE EIGHTEENTH CENTURY

GORDON AND PATERSON

The preceding chapters have dealt almost exclusively with the documentary evidence for the life-style of people in the south-western coastal region, the area where, until the later part of the seventeenth century, most contacts between the voyagers and settlers from Europe took place. During the eighteenth century the Europeans travelled further afield, and information was obtained about more distant peoples. From a research point of view, it is unfortunate that Part II of Moodie's *The Record*, which covered the period 1691-1769, was never published, since the parts that were published (Moodie *ed.* 1960) are a valuable, even if somewhat limited, source of information.

The late eighteenth century soldier and explorer Robert Jacob Gordon was probably the first European to realize that some of the shell deposits on the Cape littoral were of human rather than of natural origin. In August 1779 he found, in the hills near the mouth of the Buffels River in northern Namaqualand,

> 'many sea shells and although there were signs of marine erosion, many of these shells seemed to have been brought here by people or baboons, since I have heard that beach-Hottentots lived here, who fed themselves on whale meat and shellfish. In addition, whole heaps of shells were too new to have been at this spot since the sea was here' (Raper & Boucher *eds*. 1988: 256; see footnotes 79-80 re the identification of Gordon's 'Gouwsi' or 'Sand' River).

Further north, near the present Grootmis, Gordon and his party found 'seven huts standing together which these wild Bushmen had made of whale bones, all protected towards the NW. At these huts were found large amounts of the shells mentioned above. . . . We found sea shells everywhere, apparently brought by the Hottentots' (Raper & Boucher *eds*. 1988: 258; see footnote 83, p. 257 re locality).

Gordon's somewhat indiscriminate and confusing use of 'Bushmen' and 'Hottentots' is typical of the usage of the period, when it was considered that both Khoikhoi and San were parts of the same broad community of 'Hottentots'. However, in this specific connection, it should be noted that it was not Gordon but his editors who used the term 'wild Bushmen'. Gordon's term was simply '*wilden*': 'wild people' (Wilson & Klinghardt 1989: 50; see also Wilson 1989*c*) - possibly in contrast to the 'tame' ones who were taken into service by the farmers (e.g., Burchell 1967: 227).

An interesting observation, made still further north, probably in the vicinity of McDougall's Bay, was that there were 'many large heaps of shells all of the abovementioned [sorts] and never mussels, although these are very good here' (Raper & Boucher *eds*. 1988: 264 - editors' interpolation; see footnote 87, p. 260 re locality). Quite what were the previously-mentioned sorts of shells is not clear, since the only specific prior mention of shellfish types was 'rock-suckers' (Raper & Boucher *eds*. 1988: 261), presumably limpets *Patella* spp.

Gordon provided a comprehensive description of a camp of the 'wild people' that he found on the north bank at the mouth of the Orange River, which also gives information on the resources exploited by these people. There were the skins of 'rock rabbits' (*Procavia capensis*), jackals and seals, drying whale-meat, ostrich eggshells, some filled with water, *canna* (*Sceletium tortuosum*), eland horns filled with buchu (*Agathosma* spp., also other aromatic species) and fat (Raper & Boucher *eds*. 1988: 269). The illustration of the camp (Raper & Boucher *eds*. 1988, pl. 52) shows whale vertebrae and a rib, as well as what are probably the shells of limpets *Patella* sp(p).

William Paterson (1790: 115), who accompanied Gordon, said it was 'several species of fish' that were drying on the branches, rather than whale-meat. Paterson

(1790: 116) also mentioned that

> 'Their dress is composed of the skins of Jackals and Seals, the flesh of which they also eat. When it happens that a Grampus [whale] is cast ashore, they remove their huts to the place, and subsist upon it as long as any part of it remains; and in this manner it sometimes affords them sustenance for half a year, though in a great measure decayed by the sun. . . . They carry their water in the shells of Ostrich eggs and the bladders of Seals, which they shoot with bows. Their arrows are the same as those of all the other Hottentots' (my interpolation).

It is of some interest that although Gordon and Paterson visited this camp towards the end of winter (20 August 1779), the only plant material found, other than the grass and branches of trees used in the hut structures, etc., was *canna*, much sought after by the Khoisan because of its narcotic qualities (Smith 1966: 276, under kanna) and buchu, used medicinally and - as evidently in the present case - mixed with fat for use on the person because of its pleasant aromatic quality (Smith 1966: 135-141, under boegoe). However, the people later informed Gordon that they were almost always away from their camp 'to collect veld roots, bulbs, etc.' (Raper & Boucher *eds*. 1988: 274), so that the lack of mention of these and other plant foods in their camp cannot be accorded undue significance.

Gordon was told that the people on that side of the river had all died out, except for two women who were now with them. They denied that this had been caused by eating poisoned fish, although one of their women had died after eating one cast up on the shore (Raper & Boucher *eds*. 1988: 272). There is no information as to what prompted this statement: Gordon made no reference to having heard elsewhere of this sort of thing, which may possibly be a reference to the killing of marine life as a result of a 'red tide'.

It should be noted that the claim (Raper & Boucher *eds*. 1988: 256, footnote 81) that 'The Strandlopers, or "beach walkers" as they were called, were neither Khoi nor San but a closely related negroid group', attributed to Nienaber (1988: 867 -

correctly 1989: 866) is not that author's opinion but part of an extract from Van der Horst *et al.* (1970: 10). Nienaber's own view was considerably different (Wilson 1989*c*). Paterson (1790: 117) commented that 'Those that remain are distinguished by the name of the Shore Boshmen [*sic*]'.

DISCUSSION

The evidence of Gordon and Paterson makes it quite clear that the people they saw were not herders but hunter-gatherers whose subsistence included the flesh and fat of seals, whales and fish, and, if I have correctly interpreted the illustration of their camp, shellfish. Terrestrial food resources included ostrich eggs and plant foods, and the first sentence of the above quotation from Paterson could be interpreted as indicating that the flesh of jackals was also eaten. Although the visit by these two travellers was only a short one during a specific season - winter - Paterson's evidence appears to be that the 'shore Bushmen' moved along the coast, since they would otherwise have been unlikely to find the stranded whales he mentioned. This does not, of course, preclude the possibility that they also went into the interior; but what is clear is that these people exploited both the marine and terrestrial resources of the coastal region.

PART II. THE EVIDENCE OF ARCHAEOLOGY AND OTHER DISCIPLINES

CHAPTER FIVE

PHYSICAL ANTHROPOLOGY

INTRODUCTION: THE REVIVAL OF THE 'STRANDLOPERS'.

The name 'Strandloper' appears to have been revived during the latter part of the last century, probably as a result of the publication of *The Record* by Donald Moodie. Part 1, published in 1838, deals with the period 1649-1690, during which the Goringhaicona were in evidence (Moodie *ed.* 1960).

In 1871 a correspondent to *The Cape Monthly Magazine*, identified only by the initials 'S. T.', continuing the debate on the origin of shell deposits in the cliff-top caves at Cape Point that had been carried on sporadically in the magazine since 1858, observed

> 'The tribes of Hottentots who peopled that part of the country on the arrival of the Dutch were called the Goringhaicona; to this branch belonged the Strandlopers, Watermen, or Vismans, so often referred to in the early records. They frequented the coast extending on the west from Hout Bay, and from Kalk Bay on the east, to Cape Point. They subsisted on fish and shell-fish; and it is but natural that they should, in the course of ages, have left large deposits of shells by their kitchen middens' ('S. T.' 1871: 174-175).

It is not clear why the writer should have restricted the Strandlopers to the southern part of the Cape Peninsula when the most frequent references to them in the early records show them to have been in the vicinity of Table Bay, at the northern end (Moodie *ed.* 1960 *passim*).

In the same magazine the following year, Martin (1872: 55), having mentioned the presence of shell middens at Kommetjie, Simonstown and Miller's Point, reported that 'There has lately been sent to the South African Museum a very perfect skull of probably one of the early Strandlopers, who roamed the beach at Cape Point ages ago'. This skull (accession no. SAM-AP24), along with many of the other early acquisitions of human remains from the coastal region, is described in the Museum's physical anthropology catalogue as 'Strandloper'.

Part of the comment by the historian Theal (1918: 4) on the nomenclature and identity of the 'strandlopers' was given at the beginning of this study. It continues

> 'That word was used in the middle of the seventeenth century by the first Dutch settlers in South Africa to denote a very different class of people, an impoverished people of mixed Hottentot and Bushman blood, speaking the Hottentot language and wherever possible following Hottentot customs, who from dire necessity were reduced at times to eke out a miserable existence in the same manner as the far more ancient men of the shell mounds, and it has since been used in history to signify them alone. The others - those alluded to in this paragraph - were beachrangers, it is true, but that was their normal mode of existence, and to distinguish them from the very different beachrangers of modern times I propose to call them the Ancient Shellmound Men'.

Theal's opinion of the identity and 'culture' of these 'strandlopers' appears more substantiable than that of Elphick discussed in Chapter 3, although his suggestion that they were biologically, rather than culturally, 'hybrids' may be disputed. Fortunately, however, the name he chose for the earlier creators of the shell middens did not find favour. It would have been as confusing - and as misleading - as the one he sought to replace. However, the ascription of shell middens to the 'Strandlopers' of the early records and the application of their name to human remains found in the coastal region set a precedent to be followed when physical anthropological and archaeological research began, as is shown below.

CRANIOLOGY

In an early study of the crania of African 'Bush races', Shrubsall (1898: 264) referred to the crania of three 'Strandlopers or coast Bushmen' and included the craniometric data for these with those of his 'Bush' sample. In a later study he observed

> 'The earliest remains of the Bushmen peoples of South Africa would appear to be those of the Strandlopers found in the caves along the south-eastern seaboard. On cultural grounds these are said to be of a somewhat different type to the inland Bushmen of the present day. It therefore becomes a matter of some interest to compare the features of the two' (Shrubsall 1907: 227).

In this later study Shrubsall kept his measurements and indices for the 'Strandloper' crania separate from those for the 'Bush' crania, and concluded that 'The Strandlopers appear in all respects to be a purer group than the Bushmen, and to be distinct from the Hottentots' (Shrubsall 1907: 249). The study was based on 23 crania (not 24 as he stated) 'some of which were in a fragmentary condition' (Shrubsall 1907: 228) from the collections of the South African Museum, as well as those of a Dr Duckworth, the Anatomical Museum at Cambridge and the Royal College of Surgeons (Shrubsall 1907: 250-251). The 'Strandloper' crania in the South African Museum's collection (10 male, including one whose 'race' was doubtful, 1 ?male and 3 female) are from coastal contexts that range from as far afield as Port Nolloth in the north-western Cape to Bloukrans in the southern Cape, not the 'south-eastern seaboard' mentioned above. The seven 'Bush' crania (3 male, 2 ?male and 2 female) are from inland localities or have no locality recorded (Shrubsall 1907: 250-251; South African Museum Physical Anthropology Register: 1, 3).

Shrubsall appears to have treated his small sample as if all the individuals were contemporary (none has been dated subsequently). Moreover, in assigning all the skulls from coastal contexts to a specific 'Strandloper' category, he ignored the evidence of the early records, which reveal beyond doubt that the Khoikhoi ('Hottentots' in his terminology) occupied the greater part of the coastal region in the early historical period, and that many of them therefore probably died and were buried there.

There are problems in evaluating Shrubsall's metrical and other data. In his second study (Shrubsall 1907: 250-251), he gave cranial measurements for 14 'Strandloper' males, 1 ?male, 1 ?Strandloper, and 7 females (not 6 as stated on p. 240), as well as for 5 'Bushman' males (including 2 ?male) and 2 females. On pages 240-241 he gave summary statistics (number, mean and standard deviation) for nine measurements on 9-15 'Strandloper' male skulls (the number varies according to the measurement), and 3-6 female skulls, omitting the standard deviation for the latter, as he said (p. 240) that they were too few. Statistics were also given for 17-21 male and 12-14 female 'Bushman' skulls, also 17-19 male and 7-11 female 'Hottentot' skulls.

The summarized statistics for the 'Strandloper' male and female skulls in Shrubsall's table on page 240 do not accord with those obtainable from his table on pages 250-251. For example, the mean for maximum length of the male skulls is given on page 240 as 181,07 mm for a sample of 15. If the sample of 14 definitely-assigned 'Strandloper' males on pages 250-251 is augmented by inclusion of the doubtful male, the mean is 178,97 mm. If it is increased by adding the doubtful 'Strandloper' instead of the doubtful male, the mean is 179,50 mm. The statistics on page 240 are thus based either on information not provided on pages 250-251 or on incorrect calculations. Moreover, the measurements on page 251 for naso-alveolar height for four of the male skulls are followed by a question mark, suggesting that these are estimates rather than actual measurements. The statistics for the total 'Bushman' sample are based on more information than is included in the publication under review; and individual measurements were not provided for the 'Hottentot' sample.

Although the sample sizes are in most cases too small for meaningful testing of statistical significance, it was considered useful to do so, since this was the basis for

Shrubsall's determination of the differences between the three groups (Shrubsall 1907: 236 ff.). Table 1 provides statistics for the 'Strandloper' male sample derived from the measurements in Shrubsall's table on pages 250-251, excluding those for the ?male and ?Strandloper and those for the four questionable naso-alveolar heights. The Student's *t* test (Simpson *et al.* 1960: 176-184) was applied to these statistics and those for the 'Bushman' and 'Hottentot' male samples given in the table on pages 240-241. Shrubsall did not provide statistics for nasal breadth for the 'Bushman' sample, so that this was excluded from the tests. As shown in Table 1, there is a statistically significant difference between the 'Strandloper' and 'Bushman' samples (1:2) in the means for only one out of the eight measurements, maximum breadth. When the 'Strandloper' and 'Hottentot' samples (1:3) are compared, there are significant differences in seven of the eight of the means, the single non-significant difference being in the means for maximum breadth, the only measurement in which the 'Strandloper' and 'Bushman' skulls were *not* significantly different. Comparison of the 'Bushman' and 'Hottentot' samples (2:3) yields significant differences in the means of five of the eight measurements.

Shrubsall (1907: 242) provided a table giving the results of his tests of statistical significance for the pairs of measurements that I used in my Table 1, and he included the results of tests for nasal breadth (no significant difference in any of the pairs) which I omitted from my table because, as mentioned above, Shrubsall did not provide the necessary information for the 'Bushman' sample in his table on page 240. Discussing the results of his tests, Shrubsall (1907: 242) commented that 'A figure above 2.0 indicates considerable odds against the explanation of the differences between the respective average being random sampling, while a figure of above 3.0 indicates the practical exclusion of chance'. On this basis, the results of his tests for the 'Strandloper:Bushman' series are identical to mine, showing a significant difference (2,7) only in maximum breadth. In the 'Strandloper:Hottentot' series, Shrubsall's results show no significant difference in maximum length (1,2), or in

TABLE 1. STATISTICAL COMPARISON OF CRANIAL MEASUREMENTS FOR MALE 'BUSHMAN' AND 'HOTTENTOT' SKULLS GIVEN BY SHRUBSALL (1907: 240-241) WITH THOSE FOR 'STRANDLOPER' SKULLS (SHRUBSALL 1907: 250-251)

Measurement	no.	mean	std dev.	t test results		
				pair	t	sig.
Maximum length						
1. Strandloper	14	179,03	2,04	1:2	0,11	ns
2. Bushman	29	178,83	6,40	1:3	2,96	s
3. Hottentot	19	183,21	3,47	2:3	2,72	s
Maximum breadth						
1. Strandloper	14	137,64	1,27	1:2	2,27	s
2. Bushman	29	134,66	4,81	1:3	3,50	s
3. Hottentot	19	133,47	4,31	2:3	0,87	ns
Basibregmatic height						
1. Strandloper	11	128,27	2,30	1:2	1,22	ns
2. Bushman	29	126,41	4,84	1:3	1,44	ns
3. Hottentot	19	130,63	5,10	2:3	2,89	s
Bizygomatic breadth						
1. Strandloper	5	124,00	1,04	1:2	0,89	ns
2. Bushman	17	121,35	6,51	1:3	0,69	ns
3. Hottentot	17	125,76	5,57	2:3	2,12	s
Naso-alveolar length						
1. Strandloper	10	61,40	2,90	1:2	0,70	ns
2. Bushman	21	60,24	4,80	1:3	5,23	s
3. Hottentot	18	65,50	1,26	2:3	4,51	s
Nasal height						
1. Strandloper	10	43,25	2,48	1:2	0,37	ns
2. Bushman	25	42,76	3,87	1:3	2,16	s
3. Hottentot	18	46,22	3,91	2:3	2,88	s
Basi-nasal length						
1. Strandloper	11	96,68	5,98	1:2	1,02	ns
2. Bushman	28	94,93	4,32	1:3	0,89	ns
3. Hottentot	19	98,32	4,14	2:3	2,68	s
Basi-alveolar length						
1. Strandloper	10	92,50	3,85	1:2	1,19	ns
2. Bushman	22	94,91	5,84	1:3	3,62	s
3. Hottentot	19	99,58	5,50	2:3	2,62	s

Notes. The result of the Student's t test is given for each pair. 1:2 = Strandloper:Bushman, 1:3 = Strandloper:Hottentot, 2:3 = Bushman:Hottentot. t is the result and the letter or letters (sig.) indicate whether the result is significant (s) or not significant (ns) at p = 0,05 (2-sided test).

nasal height (1,7), while the difference in basibregmatic height (2,5) and basi-nasal length (2,8) was significant, so that the two samples are not significantly different in three measurements, as against my one. In the 'Bushman: Hottentot' series, Shrubsall's results are the same as mine.

The results of these tests show the problems inherent in using Shrubsall's data and of applying tests of statistical significance to small samples, especially when these are a series of individual measurements. The tests also show that the 'Strandloper' sample differs very little from the 'Bushman' sample, which calls into question Shrubsall's assertion quoted above, that 'The Strandlopers appear *in all respects* to be a purer group than the Bushmen' (my emphasis) when their measurements differ statistically significantly in only one of eight (12,5% of the total) - one out of nine (11,1%) in the case of Shrubsall's tests. However, the standard deviations given in Table 1 for the three groups are lowest for the 'Strandloper' sample in six of the eight measurements, showing that that there was less variation in this sample than in the 'Bushman' and 'Hottentot' samples, and this may have been Shrubsall's criterion of 'purity'. However, on the basis of the statistics he provided on pages 240-241 this is not the case, since the standard deviations for the 'Strandloper' sample are lowest for only four of the eight measurements while the 'Hottentot' sample has the lowest standard deviations for the other four. Shrubsall's criterion for assigning the 'Strandloper' skulls to this category was probably their origin in coastal contexts, but his criteria for distinguishing between the 'Bushman' and 'Hottentot' skulls were not stated.

Shrubsall's identification of the 'Strandlopers' as distinct from both Khoikhoi and San found its way into the pioneering monograph on the Stone Ages of South Africa by Péringuey (1911: 189-201; Shrubsall 1911), and thence into the archaeological and anthropological literature, both technical and popular. It has proved extremely difficult to dislodge this concept.

Schapera (1930: 29, footnote 1) commented

> 'The term "Strandloper" (coast ranger), applied by several writers on the prehistory of S. Africa to the people associated with the kitchen middens found along the south and west coasts of the Cape, should be abandoned, as the latent implication that these people form a distinct racial group is not justified. For the most part they were merely Bushmen who took to the seashore, so that we have to do with a particular mode of life rather than with a particular people'.

He accepted, however, that the 'Strandloper' remains from the Tsitsikama (southern Cape) coast referred to in a series of papers by Dart, Laing, and Gear (references cited by Schapera) 'represent a mixture of these coast-dwelling Bushmen and people of the Boskop type'. Singer investigated the evidence for the existence of a 'Boskop race' and concluded that 'It is now obvious that what was justifiable speculation (because of paucity of data) in 1923, and was apparent as speculation in 1947, is inexcusable to maintain in 1958' (Singer 1958: 177).

Schultze (1928) showed that the modern Khoikhoi and San are sufficiently like each other physiologically, and sufficiently unlike the other peoples of Africa, to warrant their being classified as a separate race, now known as the Khoisan (Schapera 1930: 5; but see Wilson 1986*a*: 259-260; 1986*c*: 17; also the section on stature hereafter). Recent studies of the cranial and postcranial morphology of human remains in museum and medical school collections (e.g., Hausman 1980, 1984; De Villiers & Wilson 1982; Morris *et al.* 1987) have tended to show that most, if not all, fall within the normal range of variation of the Khoisan. Hausman (1980: 161-162, 1984: 270), however, concluded that the crania she identified as being of 'coastal San' differed morphologically from those inland, and that there was thus a possibility of a biologically distinct population living at the coast during the later Holocene.

Morris (1986: 5) questioned the accuracy of Hausman's identifications, and showed (Morris 1986, 1987) that the validity of the identifications of most early acquisitions of human remains in museum and medical school collections cannot be supported, apart from those of 5 San and 9 Khoikhoi individuals who were known during their lifetimes (Morris 1986: 6) - a mere fraction of the collections; and he concluded that 'all that can be assumed is that the known-in-life reference samples provide a range of variation of Khoisan morphology which cannot be reliably separated into Khoi[khoi] and San categories' (Morris 1986: 9).

The major problem with biometric studies such as those of Shrubsall and Hausman is that although the geographic location of the skeletal remains is known, sometimes precisely, in most cases there is little or no information regarding their archaeological context. This lack of information is aggravated by the fact that very few of these remains have been radiocarbon dated, so that the samples, or 'populations', studied are treated as if they were all contemporaneous. This ignores the possibility of changes through time in the genetic make-up of peoples and their consequent biology.

A further problem is that the early studies appear to have determined, on whatever basis, the craniometric characteristics of the 'pure' 'Hottentot' and 'Bushman', so that any intermediate individuals were classified as 'Bush-Hottentot hybrids', as, for example, in Keen's craniometric survey of skulls in the South African Museum's collection. Keen (1952: 223-224) mentioned that there were two opposing schools of thought regarding the physical anthropology of the 'Hottentots' and 'Bushmen', one holding that the two are so alike as not to be separable into different races, the other holding that they can be distinguished apart. Keen held the latter view, and stated that 'The characteristic, or "typical" Bushman crania are easily separated from characteristic Hottentot skulls, as they show very opposing tendencies', some of which he mentioned:

> 'The "typical" Hottentot cranium is large . . ., narrow in the forehead region and markedly dolichocephalic . . .; while the "typical" Bushman cranium is small . . .,

> relatively wide in the forehead region and with a tendency to brachycephaly' (Keen 1952: 216).

Of the 178 crania Keen studied, he classified 43 (24,2%) as 'Bushman', 21 (17,4%) as 'Hottentot' and 104 (58,4%) as 'Bush-Hottentot hybrid' (Keen 1952, table 2). Since the present study is concerned with the inhabitants of the coastal region, it is worth mentioning that 44 of the crania were from the 'southern coastal area and near Cape Town', but of these only 4 (9,0%) were identified as 'Hottentot', while 17 (39,0%) were 'Bushmen' and 23 (52,0%) were 'Bush-Hottentot hybrids'. I used the measurements given by Shrubsall (1907: 250-251) to calculate the cranial indices of his 'Strandloper' and 'Bushman' samples, using the formula given by Bass (1971: 63). Of the 'Strandlopers', 3 of the males are dolichocranial, 8 mesocranial and 3 brachycranial, while none of the females is dolichocranial, 6 are mesocranial and 1 (hyper)brachycranial. In the 'Bushman' sample, 1 male is dolichocranial and 2 are mesocranial, while the one of the females is mesocranial and the other brachycranial. Thus, on the basis of this single criterion, 3 of the 'Strandloper' males and 1 of the 'Bushman' males would qualify as 'Hottentots'; another 3 of the 'Strandloper' males and 1 of the females would qualify as 'Bushman', but none of the 'Bushman' males and only one of the females; and the bulk of the sample (65,4%) would qualify as 'Bush-Hottentot' hybrids. Clearly, simple 'rules of thumb' such as those given by Keen are unacceptable.

STATURE

Parkington (1989 pers. comm.) suggested that the observations of the early travellers that the San were small were probably accurate, and that it might be of value to test this by osteometric analysis.

Lundy & Feldesman (1987: 54) stated that, while 'the combined length of femur+tibia+lumbar vertebral segment represents the best composite predictor of stature . . . the femur is the best single bone to use for estimating stature in both males and females'. Accordingly, measurements were taken of the femora of 38 of the dated skeletons used by Sealy (1989) for her isotopic analyses (see Ch. 8 below), 2 from Byneskranskop 3 (De Villiers & Wilson 1982) and 3 from the Oakhurst shelter (Patrick 1988). These measurements were converted to estimates of living stature using the ratio of femur length to stature recently developed by Lundy & Feldesman (1989). Although the measurements can be used on their own, it was considered useful, for comparative purposes, to provide measurements obtained from living populations.

Modern Khoisan and South African Negro populations

Details of the statures of 73 'Hottentot' adult males from Namibia were extracted from the study by Schultze (1928), also those of 20 male and 19 female adult */ʔauni-ǂkhomani* 'Bushmen' from the northern Cape studied by Dart (1937*a*, 1937*b*). These measurements and other information are given here in Tables 2 and 3, with the statistics summarized in Table 4. Schultze's no. 68 was excluded because its stature was not given, and Dart's no. 38 because the gender was given as 'F?'. So as to conform with the modern usage followed in the present study, the former group are called 'Khoikhoi' and the latter 'San'. Feldesman & Lundy (1988: 585; table 1) and Lundy (1989 *in litt.*) provided metrical statistics for the stature of 175 South African Negro males and 122 females. These are included in Table 4 as another set of observations against which the statures of the skeletal sample can be assessed. The statistics for all these samples are shown in diagram form in Figure 2.

The first 55 of Schultze's sample were members of various subdivisions of the Nama, followed by 17 members of subdivisions of the Oorlams (G. Klinghardt 1989 pers. comm.) and 2 Griqua. Serogenetic studies by Nurse (1983) of the Keetmanshoop Nama - the same general population as comprised Schultze's sample - showed that the Nama are now so heavily hybridized that, serogenetically at least, they are closer to several Negro populations than they are to the San (see also

Wilson 1986*c*: 21). Nienaber (1989: 794-803) provided considerable documentary evidence that the Oorlams, 19th-Century immigrants from the then Cape Colony, were also hybridized: Khoikhoi with admixtures of the genes of Europeans and slaves of various nationalities. Studies of the Griqua of the northern Cape by Nurse (1975) and Nurse & Jenkins (1975) have also provided evidence of considerable hybridization of these people, with Tswana genes being added to those mentioned for the Oorlams. The 'Khoikhoi' male sample is therefore to be understood as probably being genetically hybridized, but is the only 'Khoikhoi-like' sample for which information is available.

According to Dart (1937*a*: 164-165) the people he studied were all 'Bushman speaking' except in the case of the occupants of Huts 10 and 11, 'where the language is Hottentot although the individuals claim themselves Bushmen, but cannot define their tribe very closely'. The individuals in question are males nos 61 and 69 and females nos 62 and 70. However, since Dart (1937*a*: 174) stated that 'As far as we are able to determine they are the purest group of Bushmen now extant in the Union of South Africa', the details for these four individuals have been included in the statistics given in Tables 3 and 4. Where the two males are concerned, their statures are close to the sample mean. One of the women, no. 70, has the greatest stature of all the female sample and exceeds the mean for the male sample, while the stature of the other woman, no. 62, is near the bottom of the female range. Flawed though these comparative samples may be, they provide the only available measurements against which the calculated statures of the skeletons can be judged.

As indicated in Table 4 by the standard deviations and coefficients of variation, among the living samples there is the least variation (3,5%) in the San males, and the greatest (4,2%) in the South African Negro males, although this is only marginally higher (0,1%) than in the females of both the San and South African Negro samples. Table 4 and the 95 per cent confidence limits (solid blocks) in Figure 2 also show that the mean stature of the Khoikhoi male sample is statistically

TABLE 2. DATA FOR KHOIKHOI MALES FROM SCHULTZE (1928, TABLES A & 1)

No.	Age	Height (mm)	No.	Age	Height (mm)
1	50	1622	38	50	1686
2	50	1533	39	40	1585
3	60	1612	40	30	1556
4	20	1566	41	30	1684
5	20	1548	42	40	1546
6	30	1591	43	30	1560
7	20	1664	44	20	1663
8	40	1610	45	20	1655
9	20	1669	46	25	1709
10	20	1571	47	30	1761
11	25	1505	48	20	1584
12	20	1515	49	20	1586
13	45	1686	50	40	1705
14	30	1601	51	45	1749
15	30	1591	52	25	1660
16	25	1580	53	20	1641
17	35	1551	54	30	1622
18	45	1620	55	30	1655
19	30	1575	56	22	1667
20	25	1671	57	20	1712
21	25	1623	58	20	1661
22	35	1534	59	20	1524
23	40	1638	60	20	1557
24	25	1621	61	30	1752
25	25	1643	62	25	1659
26	50	1591	63	40	1631
27	20	1727	64	45	1727
28	20	1579	65	30	1566
29	22	1544	66	25	1621
30	25	1586	67	25	1660
31	55	1679	68	55	not given
32	35	1649	69	20	1638
33	20	1602	70	25	1726
34	20	1696	71	25	1598
35	20	1625	72	20	1630
36	20	1624	73	25	1511
37	75 ca.	1710	74	20	1586

No.	73
Range	1505-1761 mm
Mean	1624,2 mm
Std dev.	62,2 mm
95% conf.	14,6 mm
Coeff. var.	3,8%

TABLE 3. DATA FOR <u>/?AUNI-ǂKHOMANI</u> MALES AND FEMALES MEASURED BY DART (1937, A-H)

MALES No.	Age	Height (mm)	FEMALES No.	Age	Height (mm)
20	19	1500	49	17	1529
19	23	1600	54	18	1400
53	22	1590	39	18	1539
74	22	1585	51	18	1454
76	22	1605	21	19	1453
14	29,5	1496	30	20	1442
75	29	1518	45	23	1464
77	27	1616	2	28	1457
1	32,5	1491	23	27	1469
22	34,5	1601	29	29,5	1518
69	34,5	1554	70	29,5	1594
44	40	1586	10	34,5	1356
47	44,5	1595	5	39,5	1385
28	49,5	1575	48	44,5	1503
37	49,5	1648	52	49,5	1487
61	49,5	1524	62	49,5	1401
17	54,5	1599	18	54,5	1436
73	69,5	1560	15	69,5	1480
16	69,5	1478	58	80	1402
4	70-75	1441			

No.	20	19
Range (mm)	1441-1648	1356-1594
Mean	1558	1460
Std. dev.	55,1	59,6
95% conf.	25,8	28,7
Coeff. var.(%)	3,5	4,1

Note. Dart's no. 38 has been excluded because the gender is given as F?.

TABLE 4. SUMMARY OF DATA FOR KHOIKHOI MALES, SAN MALES AND FEMALES, SOUTH AFRICAN NEGRO MALES AND FEMALES AND THE MALE AND FEMALE SKELETONS OF WHICH THE FEMORA WERE MEASURED.

Group	no.	range (mm)	mean (mm)	s.d. (mm)	95% conf.	Coeff. var.(%)
Khoikhoi (m)	73	1505-1761	1624	62,2	14,6	3,8
San (m)	20	1441-1648	1558	55,1	24,6	3,5
San (f)	19	1356-1594	1460	59,6	27,3	4,1
Negro (m)	175	1494-1765	1629	67,8	10,2	4,2
Negro (f)	122	1413-1669	1541	63,9	11,6	4,1
Skeletons (m)	26	1416-1723	1560	83,9	32,9	5,4
Skeletons (f)	19	1404-1678	1527	70,5	32,3	4,6

Notes

See Tables 2, 3 & 5 for specific details of the individual samples other than the S. A. Negro samples.

(m) = male, (f) = female. s.d. = standard deviation. 95% conf(idence limits) = 2 x standard error of the standard deviation. Coeff. var. = coefficient of variation.

RESULTS OF THE STUDENT'S t TEST OF THE SIGNIFICANCE OF THE DIFFERENCE BETWEEN THE MEANS OF THE PAIRED SAMPLES

Pair	t	p
Khoikhoi (m):San (m)	4,302	s
Khoikhoi (m):San (f)	10,322	s
Khoikhoi (m):S.A. Negro (m)	0,542	ns
Khoikhoi (m):S.A. Negro (f)	8,865	s
Khoikhoi (m):skeletons (m)	4,094	s
Khoikhoi (m):skeletons (f)	6,747	s
San (m):San (f)	5,336	s
San (m):S.A. Negro (m)	4,513	s
San (m):S.A. Negro (f)	1,112	ns
San (m):skeletons (m)	0,092	ns
San (m):skeletons (f)	1,534	ns
San (f):S.A. Negro (m)	10,421	s
San (f):S.A. Negro (f)	5,183	s
San (f):skeletons (m)	4,436	s
San (f):skeletons (f)	3,163	s
S.A. Negro (m):S.A. Negro (f)	11,266	s
S.A. Negro (m):skeletons (m)	4,688	s
S.A. Negro (m):skeletons (f)	6,205	s
S.A. Negro (f):skeletons (m)	1,298	ns
S.A. Negro (f):skeletons (f)	0,876	ns
Skeletons (m):skeletons (f)	1,638	ns

Note. The differences for the pairs are significant (s) or not significant (ns) at p = 0,05.

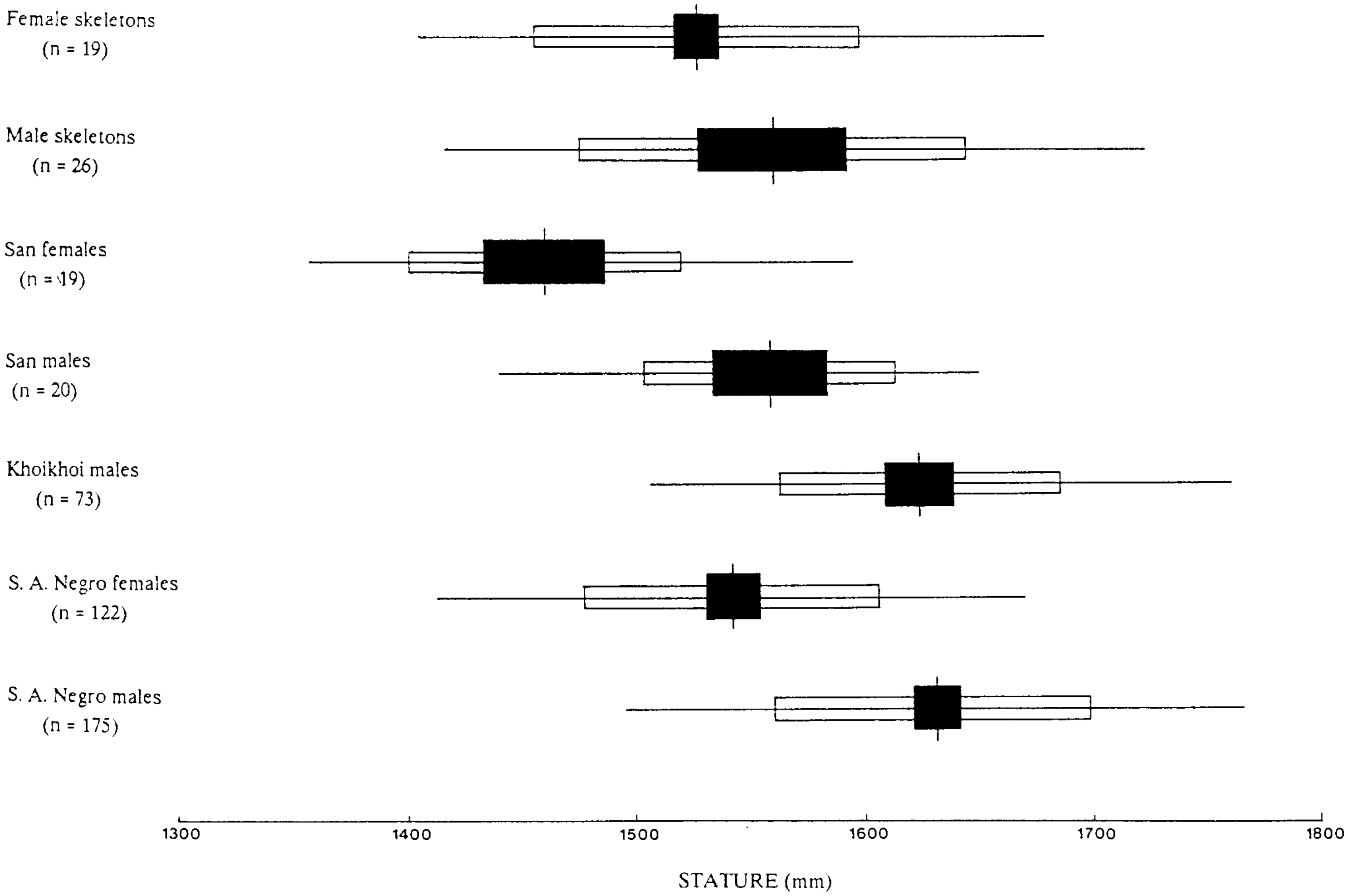

Figure 2. Modified Dice-Leraas diagrams of the calculated statures of female and male skeletons, with those of San females and males, Khoikhoi males and South African Negro females and males.

significantly different from that of the San males and females and the South African Negro females, but not from that of the South African Negro males. The mean stature of the San males is statistically significantly different from that of the Khoikhoi males and South African Negro males, but not from that of the South African Negro females, while that of the San females is statistically significantly different from those of all the other living samples.

The stature of fifteen (75,0%) of the San males and four (21,0%) of the San females falls within the range of the Khoikhoi males, while that of fifteen (63,0%) of the San females falls within the range of the San males. The whole Khoikhoi male sample falls within the range of the South African Negro males while only three San males (15,0%) are below this. In the case of the San females, five (26,0%) are below the South African Negro male range, and the same number below the South African Negro female range.

Thirty-two (43,8%) of the Khoikhoi male statures are greater than the mean for the South African Negro male sample, and only four (5,5%) are less than the mean for the South African Negro female sample. Only one of the San males has a stature greater than that of the mean for the South African Negro males, while thirteen (65%) have statures greater than the mean for the South African Negro females. In the case of the San females, none has a stature greater than the mean for the South African Negro males, and only one, no. 70, exceeds the mean for the South African Negro females. These statistics caution against the use of means as sole determinants of similarity or difference when comparing statistical populations.

Although it is necessary to emphasize again that Schultze's Khoikhoi male sample probably represents a hybridized population, the data for height in Table 4 and Figure 2 indicate that the mean stature of the Khoikhoi male sample, 1624 ± 62 mm (range 1505-1761 mm), is greater than that of the San male sample, 1558 ± 55 mm (range 1441-1648 mm). The mean stature of the San male sample is, likewise, greater than that of the San female sample, 1460 ± 60 mm (range 1356-1594 mm).

Information regarding the stature of Khoikhoi females was not available, but, on the basis of the foregoing, their mean stature may be assumed to be greater than that of San females, and possibly similar to that of San males and South African Negro females.

Prehistoric populations as indicated by skeletal remains

On the basis of published measurements from studies on 'more than 10 500 contemporary humans sampling all parts of the world', Lundy & Feldesman (1989) were able to compute the mean ratio of femur length to stature for these populations at 26,7 ± 0,55 per cent. Lundy & Feldesman concluded that this ratio 'which is gender- and ethnic-group neutral, may provide more satisfactory results than the group-specific regression formulae'. Lundy (1989 *in litt.*) considered that the previously-developed regression formulae for the South African Negro (Lundy 1983; Lundy & Feldesman 1987) should not be applied to Khoisan samples, but that the new ratio is applicable.

This ratio has therefore been applied to convert the femur lengths of the skeletal sample to estimated living stature. Following standard procedure, the left femur was selected for measurement where this was available. Where it was not, the right femur was measured and this is indicated in Table 5 by (r) after the measurement. The maximum femur length was measured on an osteometric board in the manner described by Bass (1971: 168) and converted to stature by multiplying it by 3,745, the reciprocal of 26,7 per cent. The standard deviation has been ignored, since this is a constant for all the conversions. In the case of the greatest stature, 1723 mm, the standard deviation is ±9,5 mm; in the case of the least, 1404 mm, it is ±7,7 mm. The statistics for the skeletons are summarized in Table 4 and shown in diagram form in Figure 2, along with those for the living samples.

The skeletons selected for this study are all from coastal contexts, and all have been radiocarbon dated. Of the 72 from the south-western Cape studied by Sealy

(1989, table 1), 11 were excluded because they were juveniles or their gender could not be determined. SAM-AP5068, identified as female by Sealy, was also excluded because its gender cannot be determined indisputably as the pelvis is incomplete and broken (A. G. Morris 1990 pers. comm.). A further 19 were excluded because their femora were incomplete or absent. This left a total of 23 male and 16 female skeletons, and to augment this the femora of two dated female skeletons from Byneskranskop 3 (De Villiers & Wilson 1982) were also measured, as were those of 3 male and 1 female skeletons from Oakhurst shelter studied by Patrick (1988, tables 3, 4 & 16).

De Villiers (*in* De Villiers & Wilson 1982: 216-217) provided estimated living statures for three of the adult skeletons from Byneskranskop 3 calculated by using a regression equation provided by Lundy (see Lundy 1983, table 2). It is of some interest that the Lundy & Feldesman ratio used in the present study for calculating the living stature of the skeletons in each case gave a *higher* estimate than that calculated according to Lundy's formula. In the case of SAM-AP6049, not included here because it was not dated, the femoral length of this adult male, 401 mm (De Villiers 1982, table 3) yielded a calculated living stature of 1420 mm (De Villiers & Wilson 1982: 217), while application of the Lundy & Feldesman ratio yielded an estimate of 1502 mm, some 5,8 per cent higher than De Villiers's estimate. In the case of the two skeletons for which details are provided in Table 5, the estimate using the Lundy & Feldesman ratio is even higher than that using the Lundy regression formula: 8,5 per cent in the case of SAM-AP6050; and 8,0 per cent in the case of SAM-AP6051. Lundy & Feldesman (1987) published revised regression equations that have the effect of increasing De Villiers's estimates by 6,7-7,0 per cent, but these are still 1,2-1,5 per cent lower than the estimates based on the Lundy & Feldesman (1989) ratio. This ratio yielded *lower* estimates of the living stature of 13 of the 15 fossil hominids they studied than those calculated by using the equations developed by Trotter & Gleser (1952, 1958) for American whites and 7 of the 15 using the equations for American blacks (Feldesman & Lundy 1988, table 4).

1. Female skeletons

As indicated by the results of the Student's *t* test in Table 4 and the 95 per cent confidence limits in Figure 2, the mean stature of the 19 female skeletons is statistically significantly different from that of the Khoikhoi males, San females and South African Negro males, but not from that of the San males, South African Negro females and the male skeletons. Table 4 also indicates that the sample is more variable (v = 4,6%) than any of the living samples (v = 3,5%-4,1%).

In the post-2000 B.P. sample, allowance must be made for the possibility that some of the individuals may have been Khoikhoi. It was suggested above that the mean stature of Khoikhoi females may be greater than that of San females, but similar to those of South African Negro females and San males. On this basis, only SAM-AP6221 (1614 mm) and SAM-AP6075 (1607 mm) may be suggested as possibly being Khoikhoi. However, it will be seen from Table 5 and Figure 3A that the two tallest females in the skeletal sample, UCT 248 (1637 mm) and SAM-AP5068 (1693 mm) are from the pre-4000 B.P. period, when it is highly improbable that the Khoikhoi were in the area.

There are temporal differences in the mean stature of the skeletons in this sample. It is greater in the post-2000 B.P. sample (1528 mm) than in the 2000-3000 B.P. sample (1508 mm), while the mean for the pre-3000 B.P. sample (1554 mm) is the highest for the three groups. The range for this last temporal group, 1404-1678 mm, is also the widest of the three, containing both the greatest and least statures in the sample. The inter-group differences are small, however: the mean for the post-2000 B.P. sample is 1,3 per cent higher than that for the 2000-3000 B.P. period, and that for the pre-3000 B.P. group 3,0 per cent higher. The overall trend indicated in Figure 3A is for a decline in stature through time, but undue importance should not

TABLE 5. DATA FOR THE HUMAN SKELETONS OF WHICH THE FEMORA WERE MEASURED, RANKED ACCORDING TO GENDER AND RADIOCARBON DATE.

Accession no.	$\delta^{13}C$ $^{o}/oo$	Radiocarbon date lab. ref.	 date B.P.	Femur (mm)	Stature (mm)
MALES					
SAM-AP6020	-15,4	Pta-4189	620 ± 30	460	1723
UCT 60	-14,6	Pta-2005	955 ± 50	458	1715
UCT 230	-16,6	Pta-4736	1110 ± 50	443	1659
SAM-AP4905	-14,7	Pta-4349	1210 ± 50	415	1554
SAM-AP6149	-14,4	GX-13182	1440 ± 70	442	1655
UCT 97	-11,8	Pta-4828	1560 ± 40	436	1633
SAM-AP6041A	-12,0	Pta-4722	1800 ± 50	417	1562
SAM-AP6041B	-15,7	Pta-4768	2010 ± 45	431	1614
SAM-AP1443	-11,8	Pta-2309	2050 ± 50	385	1442
SAM-AP4305	-12,5	Pta-4660	2100 ± 45	393	1472
SAM-AP5082	-11,6	Pta-4199	2150 ± 60	425	1592
SAM-AP1441	-13,0	Pta-4201	2170 ± 60	414	1550
SAM-AP4308	-11,8	Pta-4404	2170 ± 60	422	1580
SAM-AP4720	-12,1	GX-13179	2195 ± 80	416	1558
SAM-AP6023	-12,2	GX-13180	2355 ± 85	430(r)	1610
SAM-AP4899	-14,2	Pta-4149	2440 ± 60	400	1498
SAM-AP6017	-13,3	Pta-4293	2490 ± 50	390(r)	1461
SAM-AP5075	-10,6	Pta-4669	2530 ± 60	382	1431
SAM-AP5091	-14,9	Pta-4724	2830 ± 50	405(r)	1517
UCT 222	-12,6	GX-13184	2830 ± 85	381	1427
UCT 162	-11,5	Pta-929	2880 ± 50	378	1416
SAM-AP1149	-12,3	Pta-4690	3970 ± 50	411	1539
UCT 112	-11,2	Pta-2003	4445 ± 50	414	1550
UCT 206(1)	-12,4	Pta-4637	5450 ± 70	436	1633
UCT 199	-14,2	Pta-3718	6180 ± 70	432	1618
UCT 202	-13,4	Pta-3724	9100 ± 90	418	1565
FEMALES					
SAM-AP1863	-10,9	Pta-4708	800 ± 50	396	1483
SAM-AP6221	-16,1	Pta-4356	880 ± 50	431	1614
SAM-AP1247A	-15,2	Pta-4281	1180 ± 50	400	1498
SAM-AP6075	-15,0	Pta-4186	1330 ± 40	429	1607
SAM-AP5034	-16,0	Pta-4771	1390 ± 40	410	1535
SAM-AP6050	-13,9	Pta-2855	1480 ± 50	405	1517
SAM-AP5083	-14,5	Pta-926	1490 ± 55	385	1442
SAM-AP6083	-13,4	Pta-4358	2000 ± 50	390	1460
SAM-AP5041	-17,9	Pta-4376	2010 ± 50	400	1498
SAM-AP4309	-11,7	Pta-4385	2120 ± 45	413	1547
SAM-AP4813	-14,9	Pta-4204	2140 ± 45	390	1461
SAM-AP4306	-13,3	Pta-4350	2210 ± 50	421	1577
SAM-AP6031	-11,6	Pta-4814	2560 ± 50	417	1562
SAM-AP5095	-13,2	Pta-4674	2660 ± 70	388(r)	1453
SAM-AP6051	-11,9	Pta-2969	3190 ± 50	401	1502
SAM-AP5040	-17,6	Pta-4225	3570 ± 60	375	1404
UCT 373	-14,0	Pta-1754	3835 ± 50	413	1547
UCT 248	-14,2	GX-13185	4730 ± 95	437	1637
UCT 200	-12,3	Pta-4354	7120 ± 60	448(r)	1678

Statistics

Gender	no.	range (mm)	mean (mm)	s.d. (mm)	95% conf.	coeff. var.(%)
males	26	1416-1723	1560	83,9	32,9	5,4
females	19	1404-1678	1527	70,5	32,3	4,6

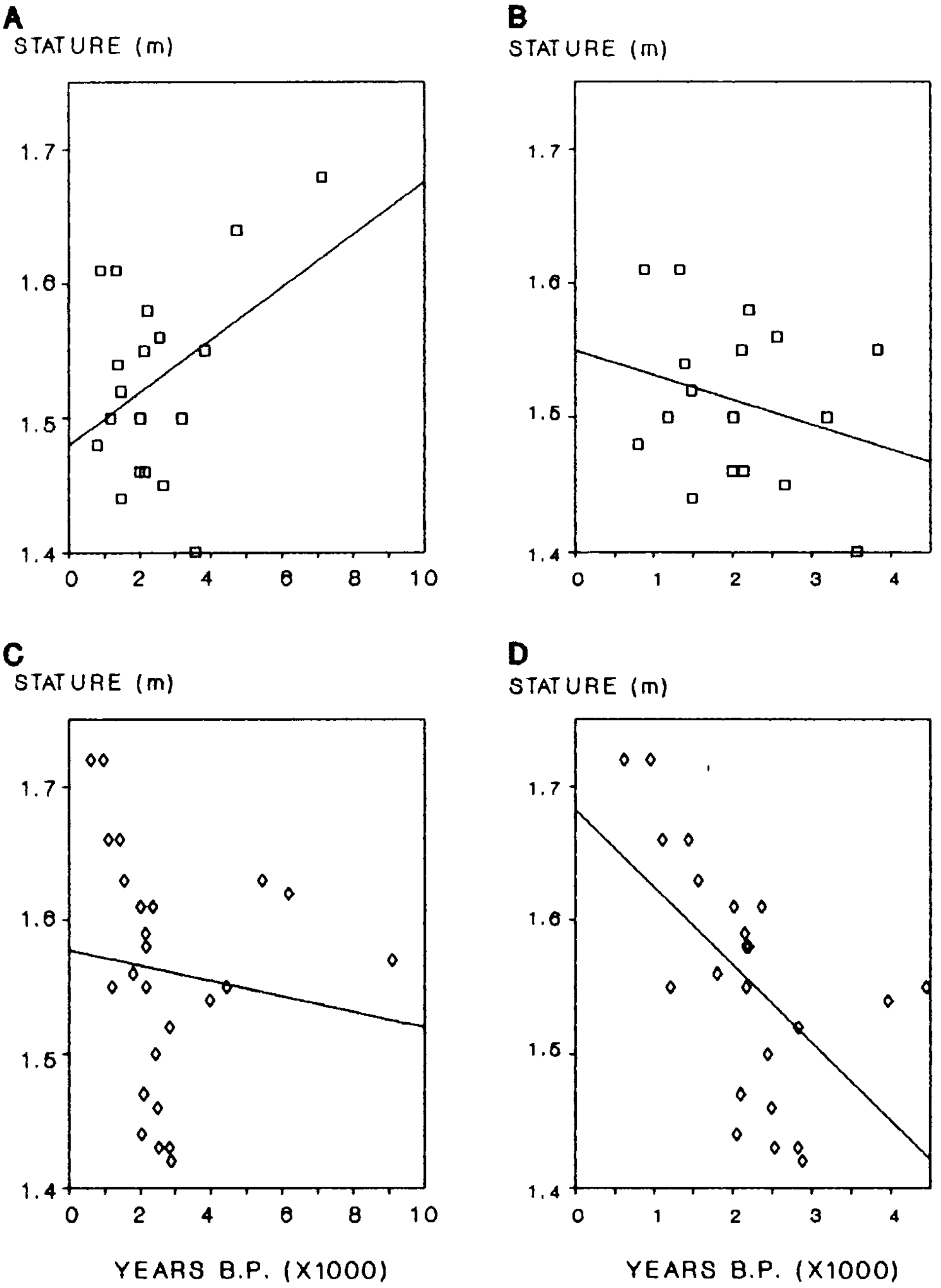

Figure 3. Stature plotted against chronology.

A females (total sample), B females (post-4000 B.P.)

C males (total sample), D males (post-4000 B.P.).

be attached to this in view of the fact that the two oldest-dated skeletons are also those with the greatest stature, and because the trend line averages out the total distribution. Confirmation of this is provided by Figure 3B, from which the two oldest-dated skeletons have been omitted and the time range reduced to 4500 B.P. Here, the trend line is the reverse of that in Figure 3A and indicates an increase in stature though time that is confirmed by the fact that the two greatest statures in this sample are near the modern end of the time range.

While the mean of the post-2000 B.P. sample is 115 mm below that of the contemporary male skeletons the difference is actually only 7 per cent. The mean of the female skeletons in the 2000-3000 B.P. group is only 0,3 per cent lower than that of the males, and that of the pre-3000 B.P. female sample 1,7 per cent lower. This indicates that although the skeletons of both sexes in the 2000-3000 B.P. group have the lowest means of the three temporal groups, there is actually little overall difference between male and female statures in the samples and relatively little change through time.

The mean stature of the whole female sample, 1527 mm, is greater than that of the San female sample, 1460 mm, and only three skeletons (15,8% of the total) have statures below this (Table 5). None of the skeletons has a stature less than that of the smallest in the San female sample (1356 mm), while four (21,0%) have statures greater than the maximum (1594 mm) of the San female sample.

The stature of only one of the skeletons, SAM-AP5040, 1404 mm, is below the range of the San male sample, 1441-1648 mm, and one, UCT 200, 1678 mm, is above it, while the statures of 13 of the skeletons (68,4% of the total) are below the mean of 1558 mm for the San male sample.

Nine of the skeletons (47,4% of the total) have statures below the range of the Khoikhoi male sample, 1505-1761 mm, only two (10,5%) are above the mean, 1624 mm, and none is above the range.

Only one skeleton has a stature below the range of the South African Negro female sample, 1433-1669 mm, while six (31,6% of the total) have statures above the mean, and one is above the range.

The stature of six of the skeletons (31,6% of the total) is below the range of the South African Negro male sample, 1494-1765 mm, while only two (10,5%) are above the mean, 1629 mm, and none is above the range.

2. Male skeletons

As indicated in Table 4, the sample of 26 male skeletons is the most variable (v = 5,4%) of all the samples in this study, for which the coefficient of variation of the others is in the range 3,5-4,6 per cent. The table and Figure 2 also show that the mean stature of this sample is statistically significantly different from those of the Khoikhoi male, San female and South African Negro male samples, but not from those of the San male, South African Negro female and female skeleton samples.

Comparison of the male and female skeletal samples was carried out above, where mention was also made of changes in the three temporal groups. Where the male skeletons are concerned, the mean stature for the post-2000 B.P. sample, 1643 mm, is 8,7 per cent higher than that for the 2000-3000 B.P. sample, 1512 mm, and that for the pre-3000 B.P. sample, 1581 mm, 4,6 per cent higher. However, as is the case with the female skeletons, these differences are not great. The skeletons with the greatest statures (1655-1723 mm: 4, or 15,4% of the total sample) are in the post-2000 B.P. group, while all those with statures below 1500 mm are in the 2000-3000 B.P. group. Results of the Student's *t* test (details of which are not given here) indicate that, at p = 0,05, there is a statistically significant difference between the means of only the males in the post-2000 B.P. and 2000-3000 B.P. groups; and it must be reiterated that the former group possibly includes some Khoikhoi, whose mean stature in the living sample was shown to be statistically significantly different from that of the modern San male sample as well as that of the male skeletons

(Table 4, Fig. 2). The overall temporal trend indicated in Figure 3C is the reverse of that for the female skeletons, with stature increasing through time. However, as in the case of the female skeletons, undue importance should not be attached to the trend line, which averages out the distribution, as is clearly evident from the post-3000 B.P. distribution, which contains the greatest number and range. Figure 3C shows a directional trend similar to that of the post-4000 B.P. female sample (Fig. 3B), while the trend line in Figure 3D merely confirms that the greatest statures are located near the modern end of the time range.

Given that the maximum stature in the San male sample is 1648 mm, and the mean of the Khoikhoi male sample is 1624 mm, it is possible to suggest that at least the five male skeletons in the post-2000 B.P. group with statures greater than 1630 mm may be those of Khoikhoi. However, given the wide range of the Khoikhoi sample, 1505-1761 mm, it is also possible that all the individuals in this time group may have been Khoikhoi. Seven of the skeletons (26,9% of the total) have statures below the range of the Khoikhoi male sample but none above it, and five (19,2%) are above the mean.

Three of the skeletons (11,5% of the total) have statures below the range of the San male sample, 1441-1648 mm, while 4 (15,4%) are above it and 10 (38,5%) are above the mean, 1558 mm. None of the skeletons has a stature below the range of the San female sample, 1356-1594 mm, while 10 (38,5%) have statures above it and 21 (80,8%) are above the mean. There is thus a greater difference between the sample of male skeletons and the San female sample than there is between the male skeletons and the San male sample.

Three of the skeletons have statures below the range of the South African Negro female sample, 1433-1669 mm, two (7,7%) are above it and 19 (73,1%) are above the mean, 1541 mm. Six of the skeletons (23,1% of the total) have statures below the range of the South African Negro male sample, 1494-1765 mm, while none is above it and five (19,2%) are above the mean, 1629 mm. The male skeletons are closer in stature to the South African Negro female sample than they are to the South African Negro male sample (Fig. 2).

DISCUSSION

My analysis of Shrubsall's craniological study has shown that there is no justification, on the basis of the statistics he provided, for his claim that the sample of 'Strandloper' skulls was markedly different from that of his 'Bushman' sample; and while there is no information as to the criteria by which he separated his 'Bushman' and 'Hottentot' samples, it may be suggested that their separation was on the basis of metrically determined morphological differences rather than on the crania having come from known-in-life individuals. Later studies, such as that of Keen (1952) seem to have been based on 'rule of thumb' concepts of what cranial characters should be found in 'Hottentot' and 'Bushman' skulls, so that any that did not conform to the 'ideal type' were automatically classified as 'hybrid'.

Genetic inheritance is the primary determinant of stature, but it may also be affected and modified by nutrition, disease and physical stress (Schepartz 1987: 36).

The San sample provides clear evidence of sexual dimorphism, although the difference between the male and female means, 98 mm, is only 6,7 per cent. Dimorphism is also evident in the South African Negro samples, in which the difference between the means, 88 mm, is 5,7 per cent. In the skeletal samples, dimorphism is less marked, with the difference between the means only 27 mm, or 2,2 per cent. As mentioned above, the possibility must be considered that some of the skeletons from the post-2000 B.P. period are those of Khoikhoi, although the female skeletons with the greatest stature are dated to older than 4000 B.P., when it is improbable that the Khoikhoi were in the region.

Where chronological distribution is concerned (Table 5, Fig. 2), there is a bias in favour of the male skeletons in the 2000-3000 B.P. group, which account for 53,8 per cent of the total male sample, while there are approximately similar numbers in the post-2000 B.P. sample (7) and pre-3000 B.P. sample (5). The distribution of the female skeletons is more even, with the same number (7) in the post-2000 B.P. as in the 2000-3000 B.P. group, and only two less in the pre-3000 B.P. group. Moreover, although the range of dates covers close on 8 500 years, the bulk of the skeletons (86,7%) are dated to within the last 3 500 years, or less than half the time range of the whole sample. This must result in problems in discussing diachronic change.

The mean stature of both the male and female skeletons in the 2000-3000 B.P. period is lower than that in the preceding and succeeding periods, but where the greatest mean stature in the male sample is in the post-2000 B.P. period, in the female sample it is in the pre-3000 B.P. period. Unfortunately, however, the samples are too small for any meaningful discussion of diachronic change, particularly when it is recalled that, although they cover a period of some 8 500 years, the bulk of the sample (77,8%) falls within a time range of less than 2 500 years, 620-2880 B.P.; and the matter is further complicated by the fact that the post-2000 B.P. sample possibly includes some Khoikhoi.

Tobias (1972, fig. 1) showed that, compared with measurements from before 1935, after 1950 there was an increase in the mean stature of San populations, both male and female. His statistics also showed that there was a clinal, north-south, decrease in the mean height of the males, while in the case of the females the Central San group were taller than the Northern San group. In an earlier paper, Tobias (1962: 804) suggested that these differences in stature might be the result of genetic differentiation between the various groups. He also questioned whether this increase had to do with 'a Bushman equivalent of the world-wide secular trend towards increase in stature', but concluded that it was, in part at least, due to improvements in nutrition resulting from improved access to foods such as cereals, milk and beef (Tobias 1962: 807-808). However, as Table 6 (overleaf) and Figure 4 indicate, there is virtually no correlation between the geographical latitude at which

TABLE 6. GEOGRAPHICAL LOCATIONS OF THE SKELETONS WHOSE FEMORA WERE MEASURED AND THEIR CALCULATED LIVING STATURES.

Acc. no.	Locality	Latitude (S)	Stature
MALES			
UCT 222	Stompneus	32°43'	1427
UCT 60	Saldanha	33°00'	1715
SAM-AP4899	Saldanha	33°00'	1498
SAM-AP6020	Saldanha	33°00'	1723
UCT 162	Ysterfontein	33°21'	1416
SAM-AP5091	Ysterfontein	33°21'	1517
UCT 112	Darling coast	33°23'	1550
UCT 230	Melkbos	33°44'	1659
SAM-AP1441	Melkbos	33°44'	1550
SAM-AP6017	Melkbos	33°44'	1461
SAM-AP6023	Melkbos	33°44'	1610
SAM-AP6149	Melkbos	33°44'	1655
SAM-AP1149	Blouberg	33°47'	1539
SAM-AP4905	Blouberg	33°47'	1554
SAM-AP6041A	Milnerton	33°52'	1562
SAM-AP6041B	Milnerton	33°52'	1614
UCT 206 (1)	Oakhurst	33°58'	1633
UCT 199	Oakhurst	33°58'	1618
UCT 202	Oakhurst	33°58'	1565
SAM-AP5082	Hout Bay	34°04'	1592
SAM-AP4305	Noordhoek	34°07'	1472
SAM-AP4308	Noordhoek	34°07'	1580
UCT 97	Kommetjie	34°10'	1633
SAM-AP4720	Kommetjie	34°10'	1558
SAM-AP1443	Gordons Bay	34°10'	1442
SAM-AP5075	Cape Point	34°21'	1431
FEMALES			
UCT 373	Elands Bay	32°28'	1547
SAM-AP5095	Saldanha	33°00'	1453
SAM-AP6075	Saldanha	33°00'	1607
SAM-AP6031	Langebaan	33°07'	1562
SAM-AP5083	Ysterfontein	33°21'	1442
SAM-AP6221	Ysterfontein	33°21'	1614
SAM-AP4813	Bokbaai	33°35'	1461
SAM-AP5040	Bokbaai	33°35'	1404
SAM-AP5041	Melkbos	33°44'	1498
SAM-AP1247A	Blouberg	33°47'	1498
SAM-AP6083	Milnerton	33°52'	1460
UCT 200	Oakhurst	33°58'	1678
SAM-AP5034	Hout Bay	34°04'	1535
UCT 248	Noordhoek	34°07'	1637
SAM-AP4306	Noordhoek	34°07'	1577
SAM-AP4309	Noordhoek	34°07'	1547
SAM-AP1863	Cape Point	34°21'	1483
SAM-AP6050	Byneskranskop	34°35'	1517
SAM-AP6051	Byneskranskop	34°35'	1502

coefficients of correlation between latitude and stature: males r = -0,070; females r = +0,158.

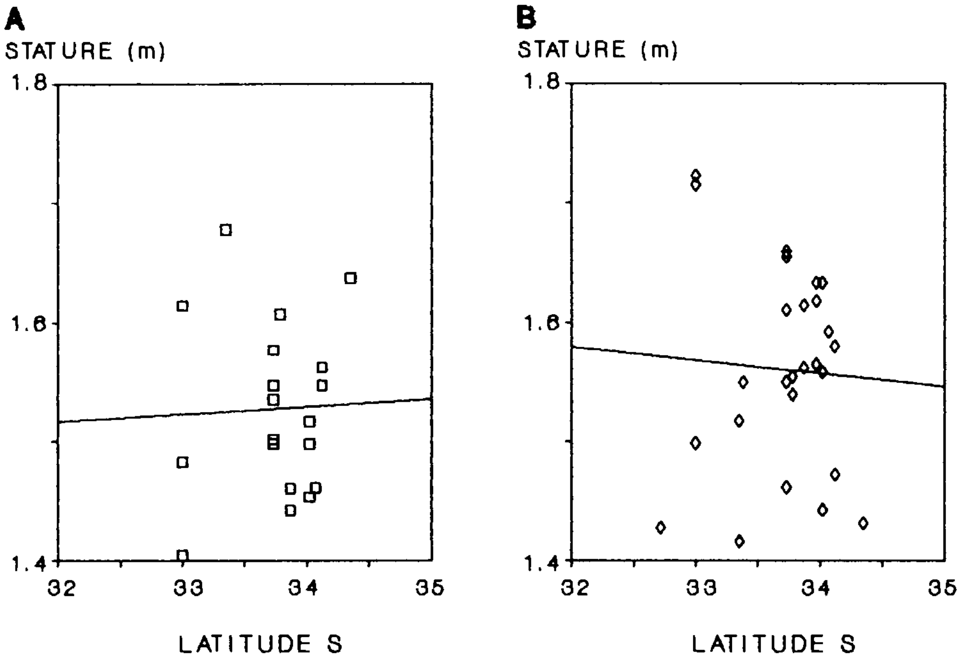

Figure 4. Stature versus latitude. A females, B males.

the skeletons were found and their statures. This is confirmed by the correlation coefficients: r = -0,070 for the males and +0,158 for the females.

As far as diet is concerned, this can only be *inferred in part* from archaeological deposits, since the actual evidence does not exist for the diet of the individuals whose stature was calculated; and the situation is further complicated by the possibility that some of the skeletons from the post-2000 B.P. period may be those of Khoikhoi, whose diet differed from that of the San - at least insofar as they had greater access to the flesh and milk of domestic animals. It may be assumed that the San of the pre-2000 B.P. period who lived in the coastal region would have had unrestricted access to the resources of both land and sea within their territories. They should thus not have suffered nutritional stress other than, perhaps, a seasonal shortage of carbohydrates resulting from the unavailability of carbohydrate-rich geophytes such as the corms of the Iridaceae. After 2000 B.P., once the Khoikhoi had entered the coastal region, access to marine resources might have been restricted to periods when the herders were absent from particular parts of the coast. There is likewise no reason to suppose that the Khoikhoi of the prehistoric period, who had access to the flesh, fat and milk of their domestic stock as well as to natural resources, would have suffered nutritional stress. Even those who had the misfortune to lose their stock and had to subsist by hunting and gathering until they could acquire more would have had available to them the relative abundance of the resources of the coastal region.

Figure 5 indicates that there is little correlation between the §^{13}C isotope readings and stature (Table 5). This is confirmed by the correlation coefficients: r = +0,504 for the males and +0,354 for the females. The overall trend is that there is an inverse relationship between stature and §^{13}C value in the female and male samples. In the case of the females, as stature increases the §^{13}C value become more positive (Fig. 5A), while in the case of the males it becomes more negative (Fig. 5B). However, given the very wide distribution of both samples and that the

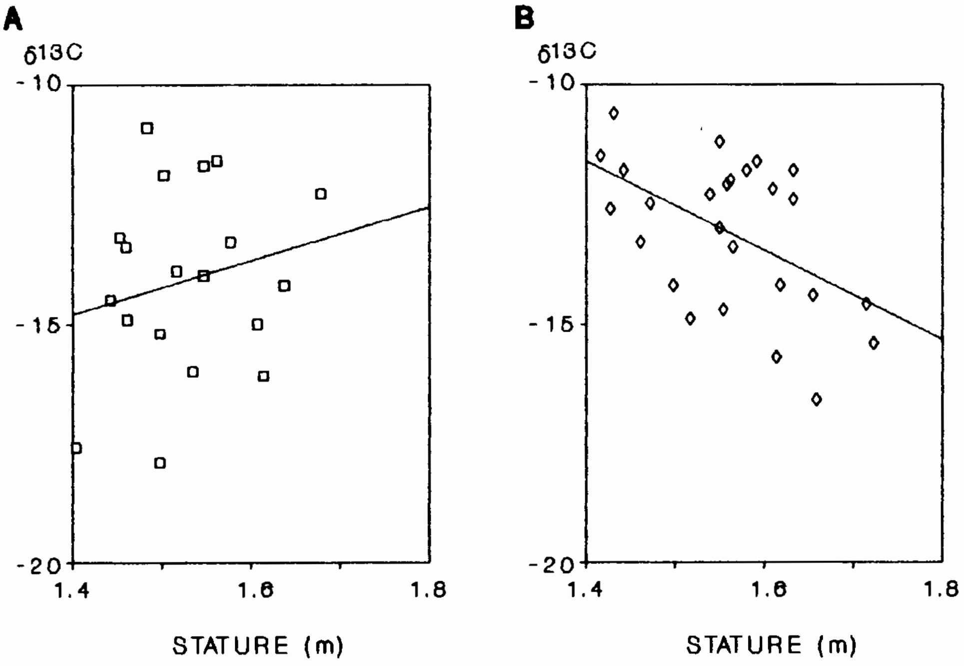

Figure 5. d13C isotope value versus stature.

A females, B males.

trend lines merely average out the distribution, little importance can be attached to these trends.

If the sample is divided into those with $\delta^{13}C$ values above and below -13°/oo, the dividing line between people whose diet is said to have included a substantial marine element (i.e., >-13°/oo) and those whose diet included more of a terrestrial component (<-13°/oo), as discussed in Chapter 8, the coefficients show a moderate correlation between stature and a largely marine diet only among the females with $\delta^{13}C$ values >-13°/oo: $r = +0{,}768$, while for the males in this group $r = +0{,}212$. In the group with $\delta^{13}C$ values <-13°/oo the males show a weak correlation: $r = +0{,}573$, while for the females $r = -0{,}148$, which indicates very little correlation.

It was shown above that the mean stature of the skeletons of both sexes was less in the 2000-3000 B.P. period than in the preceding or succeeding periods. The isotope values for the skeletons in each of the three periods show, however, little correlation with stature, as was the case with the total sample for each gender. For the post-2000 B.P. period, $r = +0{,}329$ for males and +0,497 for females. For the 2000-3000 B.P. period, $r = +0{,}332$ for males and -0,401 for females. For the pre-3000 B.P. period, $r = +0{,}595$ for males and -0,636 for females, the highest correlations for the three periods, but still not strong, particularly in the case of the females, where the correlation is negative, indicating, as also in the case of the females from the 2000-3000 B.P. periods, that there is an inverse relationship in these samples between stature and isotope value.

Since, other than what can be conjectured from study of debris from archaeological sites, nothing is known of the life-styles of the San of the prehistoric period, discussion of physical stress can only be speculative. However, it may be assumed that people living in the coastal region would not have had to travel great distances in search of food and water, though this might have varied according to season and locality. For example, the Cape west coast region today has a harsher climatic regime than the south coast region and in places the distance between the coast and the hills or mountains of the interior is quite considerable. The incursion of the Khoikhoi into the territories of the San would have affected the latter's access to resources, with a probable increase in physical stress due to their having to travel greater distances to gain access to these resources so as to avoid the Khoikhoi, particularly if they lived in enmity with them, although this was not always the case.

Where the Khoikhoi are concerned, the same environmental constraints would have applied, except that in their case the primary motivation for travel would have been the need to provide grazing and water for their livestock. However, except in the more arid regions, the distance they had to travel daily might not have been great, and their speed would have been dictated by that of their stock. Periods of extended drought would have been more hazardous for the Khoikhoi herders than for the San, whose mobility was not restricted by livestock. As mentioned in Chapter 1, the Little Namaqua, whose territory was between the Orange and Olifants rivers, were reported as having been at Saldanha Bay, some 100 km south of their lower boundary. No reason was given for their having journeyed so far from their territory, although barter or war may be suggested.

Pathological conditions resulting from nutritional and physical stress, as well as from various types of disease, can be observed in human bone and teeth (Steele & Bramblett 1988 *passim*). Forensic anthropology is, however, a specialized field and diagnosis of pathological conditions and identification of their causes should be left to those with the proper training.

The crux of the problem, as far as the present study is concerned, is that the sample of skeletons is too small, and their chronological and spatial distribution too great, for any meaningful conclusions to be drawn. The three male skeletons from Oakhurst span a period of more than 3 500 years, or over 150 generations, and the two female skeletons from Byneskranskop 3 some 1 700 years, so that even discussion of samples from individual sites has little value. Moreover, too little is known about the *actual* life-styles of the individuals concerned. The most that can be said is that the calculated mean stature of the female skeletons is greater than the measured mean stature of their modern northern female counterparts, but that the male skeletons, while covering a wider range, have a calculated mean stature almost identical to that of the modern male sample; and with this must be repeated the caveat that some of the post-2000 B.P. skeletons might be those of Khoikhoi. If only the pre-2000 B.P. male skeletons are considered (ignoring the standard deviation of the dates), the range, 1416-1633 mm, is slightly lower than that of the San male sample, and the mean, 1530 mm, only 1,8 per cent lower. In the case of the female skeletons of the pre-2000 B.P. period, the range, 1404-1678 mm, is higher than that of the San females, and the mean, 1527 mm, 5,8 per cent higher. If the two 'over-sized' females, UCT 248 and UCT 200 (whose gender was confirmed by A. G. Morris (1990, pers. comm.) after personal examination), are excluded from the pre-2000 B.P. sample, the range, 1404-1577 mm, falls within that of the San females and the mean, 1501 mm, is only 2,8 per cent higher. That the mean stature of the male skeletons is less than that of the Khoikhoi male sample (and the same may be inferred for the female skeletons in relation to Khoikhoi females, even though measurements were not available for the latter) may be suggested as resulting from differences in diet and possibly also stress; but genetic factors are not to be overlooked, particularly in view of the evidence of possibly considerable hybridization of the modern Khoikhoi, although the statistics for the modern South African Negro male sample indicate no great difference in stature between it and the Khoikhoi sample (Table 4, Fig. 2).

Keen (1952: 224) commented

> 'Crania which in the past have been called *Strandloper*, on account of their being found in association with coastal middens and rock shelters, are proving to be Hottentots or Bush-Hottentot hybrid types . . . For this reason the term Strandloper should no longer be used in a racial sense, as denoting a distinct population group. The Strandlopers are a subdivision of the Hottentot group with a propensity for living along the seashore' (Keen's emphasis).

While I have no problem in accepting that the term should not be used 'as denoting a distinct population group', I cannot accept that, biologically, they 'are a subdivision of the Hottentot group' as distinct from the 'Bushman' group, or that, apart from the Goringhaicona - the 'Strandlopers' of the early historical period - they 'had a propensity for living along the seashore'. It is my opinion that all these people: the 'Hottentots', the 'Bushmen' and the Goringhaicona, were part of the same biological group, the Khoisan; that all three groups differed culturally; and that all three lived in the coastal region, making periodic visits to the seashore. Aspects of culture and subsistence are discussed in the following chapters.

CHAPTER SIX

ARCHAEOLOGY

SITES AND ARTEFACTS

Discussing midden sites, Goodwin (1929: 265-7) suggested that

> 'they seem to represent an inland people, who have taken to a coastal type of subsistence. The term "Strandloper" has been badly applied to these folk, and it would be better to use the term as a verb, implying a strandloping type of subsistence. . . . The midden sites often show a full range of Wilton implements . . . and a number of rough instruments, apparently water-worn boulders, hacked into shape by use (not *for* use, so far as can be judged) in removing shells and breaking them open for eating purposes. . . . flesh foods seem to have been replaced largely by fish [*sic*], and the bulkiness of the shells compared with their contents gives an immediate impression of a vast population or a long continued residence. The populational density most certainly was greater than that of the inland folk, owing to the higher rainfall of the southern and eastern coastal belts . . . and to the increased supply of vegetable foods directly resulting from this, together with the abundant shell-fish supplies along the coast. But even with this increase of density, only a comparatively small number of people could have subsisted in a given area'.

Goodwin then mentioned the evidence of Drury (the South African Museum's taxidermist) and Hewitt (Curator of the Albany Museum, Grahamstown and amateur archaeologist), who had found sea-shells and 'nacre pendants of marine origin' in cave deposits in the interior, suggesting that 'inland peoples trekked periodically to the coast'. He seems to have considered that these people from the interior were bearers of the Wilton artefactual tradition, for he continued

> 'There is, on the other hand, the fact that many of the midden deposits fail to reveal Wilton or Smithfield "C" implements, the only stone objects appearing being the bored stone and the formless unconventionalised stones typical of the midden folk. . . .

> Pottery of the usual type is often associated. . . . I have in several instances discovered Wilton implements in middens previously regarded as sterile, while, on the other hand, I have failed to obtain any small implements at all from other middens. It is thus possible that many middens are refuse heaps from an evolved or deteriorated Wilton, which has discarded the microlithic side of the industry as unnecessary.
>
> One further subject is of extreme importance, but has not hitherto been sufficiently studied: the possibility of the middens either being of different ages, or having been made in some instances by peoples of mixed cultures'.

Goodwin was perceptive, at that early stage of his archaeological career, in recognizing that the middens might be of different ages and that they might have resulted from the activities of different peoples. He did not, however, put his ethnological training to use, in that he seems to have failed to recognize that the peoples responsible for at least some of these middens could have been the Khoikhoi and, in the eastern regions, the Nguni: peoples known since the earliest European contacts to have occupied the coastal region. It is also not clear why he should have restricted what he saw as a population increase, relative to that in the interior, to the eastern and southern parts of the country. Based as he was in Cape Town, he must have been well aware of the abundance of shell middens, in caves and in the open, on the south-western and western seaboard. His comments on the variable nature of the artefactual content of middens are a part of a problem that has been addressed by a number of researchers but has not yet been satisfactorily resolved.

As mentioned in the section on the 'Vismans' (Ch. 2), Goodwin (1952: 142) suggested that the 'Strandlopers and Fishmen . . . were Hottentots and Bushmen, herding and hunting people respectively who had turned for part of the year at least to the abundant sea-food to augment their normal sources of subsistence'. This represents a certain change from his earlier view mentioned above, that the middens 'seem to represent an inland people who have taken to a coastal type of subsistence' (Goodwin 1929: 265), although he seems not to have taken cognizance of the elusive

'Vismans' who had cattle, yet were said to subsist by fishing (see Ch. 2). However, he still did not address the problem of whether sites with Wilton or 'non-Wilton' artefacts could be related to the two different cultural groups. He also did not discuss why people who had access to terrestrial resources on a year-round basis, and certainly during the period of their residence at the coast, as is evidenced by the presence of the bones of land animals in many middens, should have found it necessary, or desirable, to collect shellfish. This matter is discussed in the following chapter.

Rudner (1968) attributed to the 'Strandlopers' all the pottery he studied from the coastal region between southern Namibia and the south-eastern Cape. He conceded, however, that 'It has not been possible to differentiate between the pottery of the Strandlopers and the pastoral Hottentots, who also sometimes camped along the coast or were themselves forced to become Strandlopers' (Rudner 1968: 611). The reason for this attribution appears to have been that 'the vast majority of "Hottentot" pots have been found on Strandloper sites' (Rudner 1968: 594). These sites are chiefly shell midden deposits, in the open or in caves and shelters in the coastal region (Rudner 1968: 591-594). To my mind, the reason for Rudner's inability to distinguish between the pots of the Khoikhoi and those of the 'Strandlopers' is that there was no difference between them: the Khoikhoi were the potters.

Parkington (1989 pers. comm.) has drawn attention to the fact that even when every potsherd has been collected from a site, attempts to reconstruct the pots by joining as many sherds as possible almost invariably result only in larger sherds. This has led him to suggest that they might have been used as containers of some sort. Reasonable though this suggestion is, it does not assist in the identification of the users of these containers. They could have been Khoikhoi herders, who used whole and broken pots for whatever purpose these were needed; or they could have been San hunter-gatherers who had little or no access to whole pots but used sherds that they found that were of a size adequate for their purposes.

Sampson (1974, ch. 10) used the term 'Strandloper' to refer to the post- or non-Wilton sites on the South African littoral and commented that 'Only a few excavations reflect any awareness that the "Strandloper" concept is based on an ill-defined name that poses more problems than it answers'. He also pointed out that the term had been applied to Wilton (i.e., microlithic) as well as non-Wilton (non-microlithic) artefact assemblages from coastal sites. Sampson accordingly used it as a provisional term to cover the sites he discussed, observing that 'If there is a "Strandloper complex" with regional industries, insufficient data exist to support such a concept' (Sampson 1974: 404-405).

With regard to sites on the south-western Cape coast, Sampson (1974: 420) commented

> 'The presence of stock-herding "Hottentots" as revealed by Van Riebeeck and others further complicates the picture, and it is distressing to note that no definite "Hottentot" sites can be identified in the archaeological record, so that their origins, technology, and contribution to late coastal culture remain to be investigated. ... An archaeology of the "Hottentots" has not yet developed'.

A decade and a half later, in the western to southern coastal region only the Kasteelberg sites (see Ch. 7), can be accepted with reasonable certainty as resulting from herder occupation, although I have little doubt that many of the other sites in the Cape coastal region are attributable to the Khoikhoi. This is because it seems clear to me that people who inhabited the coastal region for well over 1 500 years before white settlement began must have left traces of their occupation. The problem is not so much of the existence of this evidence, but of finding and correctly identifying it.

Schrire & Deacon (1989: 112) concluded from an analysis of the indigenous artefacts at the former Dutch East India Company outpost, called Oudepost I by the authors, on the Churchhaven Peninsula near Saldanha Bay, that these artefacts were to be attributed to the Khoikhoi, who were historically recorded as having been in contact with the Dutch at the outpost. This attribution was questioned by Wilson *et al.* (*in press*), who, amongst other things, drew attention to the admission by Schrire & Deacon (1989: 110) that there was 'well-attested redistribution of material due to dune mole activity'. Wilson *et al.* pointed out that, apart from the potsherds, which they conceded were of Khoikhoi origin, none of the artefacts illustrated was any different from those recovered from deposits dated to before the advent of pastoralism in the western and southern Cape, and that there was no evidence to show that even the potsherds found at the site resulted from the activities of the Khoikhoi at that site. They therefore concluded that the evidence for the attribution of the indigenous artefacts to the Khoikhoi was circumstantial and could not be supported.

Sampson (1974: 437) considered that 'there is some reason to suspect that the Strandlopers represented [by cultural and human skeletal material from coastal sites] are an extremely ancient and isolated Stone Age group, briefly influenced by Wilton ideas (and the genes of the "Wilton population")'. He did not, however, explain from whom this Stone Age group had been isolated, nor why they should have been influenced only briefly by 'Wilton ideas' and genes. *Pace* Shrubsall and others, there is no skeletal evidence to support the existence of a biologically separate Stone Age population in the southern African coastal region prior to the advent of the Negro peoples (see Ch. 5); nor is there any evidence to support the suggestion that the culture of peoples in the coastal region was any different from that of peoples in the interior, making due allowance for temporal and regional differences such as, for example, between the pre-Wilton, Wilton and post-Wilton lithic industries on the temporal level, and the Wilton of the coastal region and the Smithfield C of the Central Plateau on the regional level. All the available evidence points to people with a common, hunting and gathering economy prior to about

2000 B.P. and to the addition after that time of pastoralism and agriculture, which were practised by only some of the population of the coastal region. As was shown in Chapter 5, the herders shared a broadly similar genetic relationship with the hunter-gatherers into whose territory they moved; and here it must be stressed that the assumption that pastoralism was introduced into the coastal region by people other than the aboriginal inhabitants of the area, and not as the result of acculturation, has yet to be verified (Wilson 1989*b*).

A suggestion similar to Sampson's was also made more recently by Inskeep (1987: 303) who, in an attempt to explain the apparently abrupt introduction of the Wilton industry at Nelson Bay Cave (NBC), proposed that

> 'The NBC Albany people were year-round residents at the coast, and the "Wilton" occupation (quartz and chalcedony, segments and small scrapers) represents inland dwellers induced by environmental constraints in the interior . . . to begin a pattern of seasonal movement to the coast around 6000 BP, where, for perhaps as little as two months a year, they shared the resources with indigenous "Albany" populations until 4500 BP when, as a result of environmental amelioration in the interior, the need for seasonal transhumance (to the coast, at least) was reduced and their visits became less frequent, eventually ceasing altogether at around 3300 BP, leaving the coast for the sole (year-round) occupancy of the descendants of the original "Albany" population'.

I cannot accept that the introduction of the Wilton at Nelson Bay Cave was as abrupt as Inskeep suggested. The retouched artefact or 'formal tool' component of the deposits underlying Inskeep's excavations (J. Deacon 1984, table 13) shows small but increasing numbers of 'backed microliths' as well as segments and borers to be present from at least layer RA onwards. Charcoal from this layer was dated to 6070 ± 125 B.P. (UW-222) and from the underlying RB to 8070 ± 240 B.P. (UW-181) (Klein 1972: 202), but Deacon (1984: 62, 68) suggested that, because the interface between RA and RB has not been dated, the 'break' between these layers 'should be considered more apparent than real'.

Similarly low but increasing frequencies of retouched artefacts were also recorded from layers 13-10 of Byneskranskop 1 (Schweitzer & Wilson 1982, fig. 14, table 7). Layer 13 was not dated but layer 14 was dated to 9760 ± 85 B.P. (Pta-1587) and layer 12 to 7750 ± 90 B.P. (Pta-2347) (Schweitzer & Wilson 1982, table 1). H.J. Deacon (1976, tables 3 & 10) recorded a similar transition at Melkhoutboom at much the same time as at Nelson Bay Cave and Byneskranskop 1. It is thus arguable that these deposits, like layers RA-IC of Nelson Bay Cave, contained what Sampson (1974: 298) called the 'Early Wilton', and that Inskeep was misled by the low frequency of retouched artefacts in the lower layers of Nelson Bay Cave into thinking that the Wilton 'arrived' at that site only at about 6000 B.P., when the 'Classic Wilton', as Sampson (1974: 298) called it, became evident.

Inskeep's suggestion that an independent Albany population co-existed in the cave with Wilton visitors appears to be based on the superficial resemblance between the pre-Wilton Albany/Oakhurst industries and those of the 'post-climax Wilton', both of which are notable, in contrast to the intervening Wilton, for their relative lack of 'formal' stone artefacts other than scrapers and, in some cases, the presence of a relative abundance of bone artefacts. Also common to the pre-Wilton assemblages and some of the post-Wilton ones is an apparent preference for quartzite in place of the more siliceous materials like silcrete and chalcedony (or chert). This non-microlithic quartzite element is also present in Wilton assemblages, which presumably gave rise to the suggestion by Inskeep that the resident 'Albany' population co-existed with the 'Wilton' people at the time that the latter were at the coast. It seems highly improbable, though, that two populations with distinct norms of artefact manufacture could co-exist so closely as to share the same cave for several thousand years without some degree of cross-culturation. It must also be asked why, if the Albany tool-kit was adequate for the tasks for which the tools were made, it was necessary for the more elaborate and extensive Wilton tool-kit to be manufactured while these people were at the coast.

An equally important question that must be answered before Sampson's and Inskeep's hypotheses can be accepted is from where in the interior the Wilton people came who lived at the coast between about 6000-3300 B.P. Sites in the eastern and southern Cape interior such as Wilton (J. Deacon 1972), Melkhoutboom (H. J. Deacon 1976), Boomplaas (H. J. Deacon *et al.* 1978; J. Deacon 1984) and Buffelskloof (Opperman 1978), to mention some of the better known, all have the same Albany-Wilton-'post-climax Wilton' sequence as Nelson Bay Cave; and the 'Smithfield C' industries that are characteristic of the Central Plateau differ from those on the coastward side of the Great Escarpment to such an extent that the Wilton cannot be seen as their expression in different raw materials.

While there may be no ready explanation for the development of the Wilton from the Albany/Oakhurst and its replacement by an Albany-like industry, it should be remembered that there was a similarly 'atypical' development in the Middle Stone Age: the Howiesons Poort industry (see Sampson 1974: 231-242), which saw the addition of a range of artefact types not presaged by anything in previous assemblages, and which subsequently disappeared. Certain of these types, such as segments, trapezoids and obliquely backed pieces, were only 'reinvented' in the Wilton, albeit in microlithic form.

Another question that needs to be answered is why, if conditions in the interior were so unfavourable as to constrain the populations of that region to move to the coast, they needed to do so for only as little as two months a year, as suggested by Inskeep. Such a scenario would help explain the continued occupation of the interior sites mentioned above, but evidence for a major *seasonal* deterioration in the environment, which is what Inskeep's scenario suggests, is wanting. Cockroft *et al.* (1987: 172; fig. 6) suggested that during the period 9000-4000 B.P. most of southern Africa was moist, that the southern Cape coastal region was wetter than at present, and that 'Regional variations in climate do not appear to have been marked', although the south-western Cape was drier than at present. The period mentioned covers the Albany-Wilton transition and most of the time of Inskeep's scenario. More specifically, Cockroft *et al.* (1987: 172) quoted the observation of Street & Grove (1976) that 'The climates of most of Africa at 6000 BP were a great deal moister than they are today'; and, according to Zubakov & Borzenkova (1990: 295), the period 6200-5300 B.P. was that of the 'most considerable Holocene warming'. Increased temperature and precipitation would have had an effect on the vegetation, both in the interior and at the coast; and changes in the vegetation would also have affected animal life and, consequently, the lives of the people who depended on these. Schweitzer & Wilson (1982: 175-183, table 26) showed that, in the case of Byneskranskop 1, 'technological change is not concomitant with economic change' and this must be true of other sites.

At Byneskranskop 1 the introduction of the Wilton, at about 8000 B.P., was accompanied by an increase in the use of marine resources, particularly shellfish, that was even more marked some 1 500 years later. During this period, but not simultaneously, there was a change in the overall size range of the land mammals brought back to the site, from a predominance of large medium animals over 100 kg adult weight to a range below this (Schweitzer & Wilson 1982, tables 17 & 26). The period from about 10000-6000 B.P. was one of rising sea-levels and a marine transgression (Flemming 1977: iii, 81, 143-145; Mörner 1978: 5-6; Yates *et al.* 1986) and to the reaching of what Mörner termed the 'climatic optimum'.

On the basis of micromammalian fauna recovered from the deposits, D.M. Avery (1982, table 45) interpreted the climate of the Byneskranskop area as becoming generally warmer during this period, though with an episode of harsh climatic conditions around 6000 B.P. The topography of the coast near Byneskranskop is such that a maximum rise of 3 m in sea level would have extended the area of the estuary of the Uilkraals River, but would not have brought the shore that much closer to the cave to allow for the suggestion that it was this factor that persuaded the cave's occupants to go to the trouble of bringing fairly considerable quantities of shellfish back to the cave.

Although the evidence provided by Inskeep's excavations at Nelson Bay Cave allowed him to suggest that the environment was suitable for year-round occupation from at least 10000 B.P., and shellfish were exploited well before that time (Klein 1972, fig. 5), this was not the case at all sites in the coastal region, as evidenced by the data from Byneskranskop 1 and the abandonment of Elands Bay Cave between about 7800-3800 B.P. (Parkington 1987*a*: 7) and again between about 2900-1500 B.P. (Parkington *et al.* 1988, fig. 3.5). While it is not necessary to invoke 'environmental determinism' as the sole reason for the differences mentioned above, environmental change certainly seems to have played a part in the changes observable in the hunter-gatherer life-style during the Holocene. It is to be hoped that future archaeological and palaeoclimatic research will help to fill the gaps in our knowledge that exist at present, and help solve the overall problem.

D. M. Avery (in prep) has suggested that 'basic' tool-kits, such as that of the Albany, may be indicative of periods of stress, while more elaborate ones, such as those of the Howiesons Poort and Wilton, may indicate periods of little or no stress. On the other hand, it has been suggested that elaboration of rock art was a response to stress (Van Rijssen 1980: 16, 99; Parkington *et al.* 1986: 314-315). Wilton artefact assemblages include a range of ornaments that are generally more elaborate than any of those from the Albany; and it can thus be argued equally well that the manufacture of these was a palliative for the stressful conditions in which the people found themselves, whatever these were.

Sampson (1974: 435) concluded that

> 'Because the term "Strandloper" refers to a highly developed set of littoral exploitation strategies, it obviously has too broad a meaning to be applied to a related group of lithic assemblages. Strandloper strategies were employed by the makers of the

> Oakhurst, Coastal Wilton, and later sites. Certainly there is no single "Strandloper industry" and a terminology for later non-Wilton assemblages is needed'.

Pointing out that 'a relatively simple Oakhurst-like flaking technology was practised on several middens during the Wilton period' Sampson (1974: 435-436) could not agree with the view of other researchers such as H. J. Deacon (1970, cited by Sampson as 1969*c*), Inskeep (1967), Maggs & Speed (1967) and others, that 'such sites must represent a specialized or seasonal activity of the local population'. According to this view 'adjacent middens and inland caves containing Wilton assemblages . . . would represent yet another aspect of the same population's activities'. Sampson disagreed, observing that

> 'The range of faunal material from Wilton and non-Wilton middens is identical: shellfish collecting is invariably supplemented by fishing and hunting of all available game animals. The same range of subsistence activities took place on both kinds of midden. Thus, the differences in artifact content are more likely to reflect two distinct stone-flaking traditions that represent two independent populations. Obviously, they exploited the littoral by using every available food source'.

While I lack Sampson's long experience in artefact analysis, I have been unable to find any evidence, in the Holocene assemblages from the southern and south-western Cape coastal region that I have studied, for the 'two distinct stone-flaking traditions' mentioned by Sampson and suggested by Inskeep (above). Clearly, different flaking *techniques* would be employed in the manufacture of the retouched microliths that are characteristic of the Wilton and the crude flakes and flaked cobbles or 'heavy edge-flaked pieces' (J. Deacon 1984: 378) that are a common feature of shell middens but are also found in Wilton deposits (e.g. Schweitzer & Wilson 1982, table 6; fig. 12; J. Deacon 1984, table 12). These need not, however, imply distinct flaking *traditions*, and certainly not 'independent populations'.

Another point to be borne in mind is that the tools needed for shellfish collecting and preparation need not be as complex as those required for other activities, such as hunting, the preparation of clothing, the manufacture of ornaments and implements and, perhaps, plant food collecting and processing. A cobble, or perhaps a bone spatula would suffice for the removal of limpets (*Patella* spp.), perlemoen and Venus ears or siffies (*Haliotis midae*, *H. spadicea*) and chitons (*Dinoplax* and other species of Polyplacophora) from the rocks. Mussels and other bivalves, as well as gastropods other than those mentioned above, can be removed individually by hand or, in the case of bivalves, in bulk by the use of an implement such as a digging stick. Cooking opens the shells of bivalves, or weakens the ligaments that hold the valves together, so that they are easily opened; and most gastropods are also easily removed from their shells after cooking. Perlemoen could be removed by using a sharp stone or bone flake to cut the adductor muscle and the flesh tenderised by pounding it on a grindstone with a muller. These, then, would represent the basic tools needed for shellfish collecting. Any other artefacts found in midden deposits would relate to the other activities mentioned above.

Rudner & Rudner (1954) described what they termed 'a local Late Stone Age development' in which the common factor, or 'type implement' was an artefact they said Goodwin called a 'slug' (Rudner & Rudner 1954: 103). Of the ten sites included in their paper only one, Het Kruis near Piketberg, is not coastal. Sampson (1974: 414), reviewing the 'Sandy Bay industry', described these artefacts as 'worked-out adzes' and commented 'Whereas these forms are certainly present in Wilton samples, they are not recorded as a dominant form'. However, as the inventories (Rudner & Rudner 1954: 106; Sampson 1974, table 69) confirm, adzes outnumber scrapers at seven of the ten sites, only Hawston, Arniston and Het Kruis having more scrapers than adzes.

At Byneskranskop 1, one of the few long-sequence Late Stone Age sites in the coastal region with adzes, these were rare in the Albany levels (layers 18/17-13:

1,2% of the site total), uncommon in the first half of the Wilton (layers 12-6,: 14,5%) but abundant thereafter. In layer 5, adzes accounted for 42,3 per cent of the layer total of retouched pieces, and in layer 4 for 38,1 per cent. In layers 3-1, when there was a marked decline in the frequencies of other retouched pieces relative to the underlying layers, adzes outnumbered scrapers by a factor of between 1,7:1 in layer 3 and 1,2:1 in layer 1 (Schweitzer & Wilson 1982, fig. 14, table 7). The indication is thus that adzes were in greater use from the 'climax Wilton' onwards, but do not appear to have been affected by the apparent general decline in the frequency of other retouched artefacts that occurred at the site in the post-Wilton period.

The nearby coastal cave site of Die Kelders yielded only 62 retouched pieces, of which only six were scrapers, and there were no adzes (Schweitzer 1979, table 15). Adzes were not found in Klein's excavation at Nelson Bay Cave (J. Deacon 1984, table 13), and Inskeep (1987: 140) recorded only two from his excavations, commenting that they did not look like those illustrated by other researchers and, 'in the sense that most workers currently use the term in South Africa, adzes may be said not to occur in Nelson Bay Cave in the deposits under discussion'.

None of the 'Sandy Bay industry' sites described by Rudner & Rudner and discussed by Sampson has been dated and the artefact inventories suggest that sampling was selective. The sites were not systematically excavated according to modern criteria: Rudner & Rudner (1954: 105) stated that they had been collecting material from the Gordon's Bay midden for years. What they do indicate, however, is the degree of inter-site variability and the need for caution in considering undated sites as comparable on the basis of shared artefact types, such as the adzes that were used to define the 'Sandy Bay industry'.

Jacobson (1987*a*) studied the sizes of ostrich eggshell beads from eighteen sites in Namibia. Only seven of these, Wortel and the Kuiseb (!Khuiseb) sites are in the coastal region, but since the rest are south of the areas occupied (at present) by the black farmers and pastoralists, all the sites may reasonably be attributed to the Khoisan, although the presence of the Dama should not be overlooked. Jacobson 1987*a*: 56) divided the assemblages into three types. Type I assemblages contain Wilton artefacts but no pottery. Type II have artefacts similar to those of Type I, but generally include small potsherds. Type III, which include all those from the coastal sites, and were attributed by Jacobson to herders, 'are characterized by a lack of formal stone artefacts except, perhaps, for scrapers, but with abundant pottery'. Jacobson (1987*a*, table 1) provided statistics for the diameters of the beads from these sites that show that, with one exception (Kuiseb site K24), the mean and maximum diameters of the beads from the Type III sites are greater than those from the Type I sites. The two Type II sites are problematic and Jacobson was unable to classify the Geduld site into any of his types, so that these are not included in the present discussion. Although Jacobson (1987*b*) later provided data for beads from 'the lower herder component' of the Geduld site, which contained potsherds, he still did not assign to any of his three assemblage types.

Using the analogy of modern Kalahari San women, who made larger beads for other people than they would make for themselves, Jacobson (1987*a*: 58) suggested that the similar pattern observed in Types I and III beads 'could provide a stylistic marker documenting the appearance of Khoe [Khoikhoi] pastoralist society as known historically'. The implication of this, that the Khoikhoi did not make beads for themselves but obtained them from the San, is one that, in my view, is going to be extremely difficult to substantiate. Even if beads that are larger than those in pre-pottery assemblages are found exclusively in assemblages with pottery, the only valid assumption that can be made is that the people responsible for the pottery assemblages had a preference for larger beads than the people of the pre-pottery assemblages; and here it must be borne in mind that assemblages containing potsherds are not *de facto* attributable to the Khoikhoi.

DISCUSSION

The foregoing serves to highlight the lack of consensus among archaeologists when dealing with sites in the coastal region. It seems that too little attention has been paid to temporal and regional variation; that the 'Strandloper concept' of people subsisting solely on marine resources dies hard in the minds of some researchers; also that too little consideration has been given to the probability that shell middens represent but one facet of the people's subsistence strategies, whether these people were hunter-gatherers, herders or farmers.

Where the artefact industries found in coastal sites are concerned, I consider that still too little is known about the precise nature and distribution of the pre- and post-Wilton industries for hypotheses such as those of Sampson and Inskeep to be acceptable. Moreover, the *differences* in these industries - between those of the pre-Wilton and the post-Wilton, as well as between those *within* each group - need to be examined in greater detail, since it seems that the 'continued existence' of an Albany tradition during and after the Wilton is based only on a number of *similarities*.

A question that needs to be addressed is whether, if the earliest pastoralists had only sheep, they had the mat-and-withy huts that were such a commonly-described part of their impedimenta in the historical period. According to the records (e.g., Kolb 1738: 181), the frames and mats of which these huts were made were transported, along with pots and other baggage, on the backs of oxen. Clearly, sheep would have been unsuitable for this purpose, and it must therefore be asked what sort of shelter the sheep-herders used prior to the introduction of cattle into the region. Although I tend to be sceptical about the use of caves and rock-shelters by pastoralists, the apparent lack of transport animals in the early period of pastoralism provides a good argument in favour of the use of such places.

Another problem is that, no matter how well archaeological deposits are excavated or how many radiocarbon dates are obtained, present techniques are incapable of detecting other than major breaks in occupation. Noli (1989: 96), discussing sites in the Koichab River region of Namibia, suggested that sporadic occupation of sites is not detectable and that 'The scenario which the archaeological record is likely to suggest is one of continuous occupation during an extended period of favourable conditions'. It seems that further refinements in excavation techniques are needed, as well as detailed analyses of greater numbers of dated artefact assemblages from sites in the coastal region. The greatest lack, however, is in sites that can unquestionably be ascribed to the Khoikhoi.

The early records are also lamentably inadequate as sources of information regarding the artefacts of the Khoikhoi and San. It is insufficient, for archaeological purposes, to know that they used bows and arrows, assegais and digging sticks and so forth. These were, as far as the evidence goes, common to both groups; and the same is probably true of all the artefacts found in midden deposits. Prior to the arrival of the voyagers and settlers from Europe, very little metal seems to have been available to the Khoikhoi and, presumably, even less to the San; and what there was was used for ornaments (Ch. 1). The Khoikhoi would thus have been obliged to use the same raw materials as the San: stone, bone and plant fibres, though they would possibly have had less use for ostrich egg-shells as containers since they were able to make, and transport, clay pots. Discussion of this matter will, however, have to wait until the artefacts from the Kasteelberg sites have been analysed and published. Then, too, perhaps the stone artefacts from Oudepost I can be assessed.

CHAPTER SEVEN

ARCHAEOLOGY (continued)

SUBSISTENCE IN THE COASTAL REGION

Introduction

According to Waselkov (1987, table 3.6), South Africa has the world's oldest record of shellfish collecting in any quantity, dating back some 130 000 years at Klasies River Mouth (see below). Yet, on 29 October 1653, some 18 months after the establishment of the Dutch settlement on the shore of Table Bay and at least 1 500 years after the introduction of pastoralism to the coastal region, a party of soldiers who had returned from Saldanha Bay reported that 'They had met many elephants, rhinoceros, elands, harts, hinds, and other game' (Moodie *ed.*1960: 39). It must be asked why, in a land until recently so abundant in large terrestrial mammals (Skead 1980), people chose to collect shellfish, an occupation that seems more labour-intensive than the hunting of, say, a large antelope. Even if it is assumed that the collecting was done by women, their energies would seem to have been better spent in collecting plant foods in order to offset the potential danger of protein poisoning (see Ch. 8). The archaeological evidence is, none the less, that people did collect and consume considerable quantities of shellfish, and it is thus pertinent to discuss the matter of subsistence needs and strategies.

Prior to the advent of the European settlers, the southern African coastal region was occupied by three groups with differing resource bases: hunter-gatherers, who subsisted, initially, at least, on indigenous resources; pastoralists who had access to imported domestic stock as well as to indigenous resources; and farmers who added the cultivation of exotic crops to the resource bases they shared with the other two groups. It is therefore appropriate to consider the archaeological evidence for the subsistence of each of these three groups.

Hunter-gatherers

In common with the rest of the world, hunting and gathering was the basic subsistence mode of the peoples of the southern African coastal region for most of the existence of humankind. Very little evidence survives for the subsistence activities of the people who lived in the region for most of the million years or so of its human prehistory: their existence is attested to chiefly by the stone tools they made and used.

Where the Early Stone Age is concerned, Sampson (1974: 127-134) observed

> 'All the evidence suggests that the Acheulian economy was based on the hunting of available game near permanent water supplies such as river banks, lake shore or swamp. . . . There is clearly no evidence to suggest specialized hunting of a few selected animals. It appears that any available meat supply was exploited, and the Acheulian hunting ability apparently extended to the largest and most dangerous animals among the African fauna. . . . Whereas the bones of lesser game could be obtained by scavenging from carnivores, the presence of the very large animals (presumed to be beyond the hunting capacity of carnivores) must reflect organized hunting and probably trapping by man. . . . If the faster antelope species were hunted rather than scavenged from carnivore kills, it must be assumed that stalking and the use of the throwing spear had become developed skills by this stage.'

Sampson also commented that there was 'some hint' that plant foods were also collected during this period, and that fire was used 'for warmth and possibly cooking meat'.

The scanty evidence on which Sampson based his observations comes mainly from the interior, with the coastal region represented only by the artefacts and fossil bone assemblages from Elandsfontein, south-east of Saldanha Bay. Sampson (1974: 127) remarked that, 'these were surface finds open to all the usual criticisms, including the possibility that they are artificial clusters caused by sand-dune deflation'. However, after studying the fauna from the so-called 'Cutting 10' at Elandsfontein, Klein (1978: 82) concluded that

> 'the sum of the evidence suggests that people were involved in the Cutting 10 occurrence as butchers of ungulates that died or were killed as they came to drink at a nearby waterhole. Some of the ungulates were perhaps killed by large carnivores, and people and other predators may well have scavenged each other's kills. The scatter of artifacts and bones probably reflects not only carcass dismemberment from hominid butchering and carnivore feeding, but also the kicking of objects across the surface of the site by later ungulates coming to the waterhole to drink.'

Klein's conclusions thus generally support Sampson's, although he was more sanguine than Sampson about the association of the artefacts and faunal remains. The faunal list (Klein 1978, table 1) does not include any marine species and it is not clear whether, at the time the material was deposited, Elandsfontein was within the coastal region as it exists today, or whether the sea was more distant. Klein (1978: 71) commented that the fauna indicated wetter conditions and 'a much larger grass component' than is present in the modern fynbos vegetation.

Hendey (1974, table 6) published a list of the mammalian fauna that includes remains from the whole site and is thus more comprehensive than Klein's, but it does not include any marine mammals, and there was no reference to any other marine fauna such as shellfish. Hendey did not comment on the relationship of the site to the present coastline (about 20 km), nor did Butzer (1973) in his geological re-evaluation of the site; but on the basis of Klein's comments given above, it is possible that the coast was further away during the Acheulean. Whatever the case, Elandsfontein is to be interpreted as relating to specialized activities on the coastal plain, namely the hunting and scavenging of terrestrial mammals, rather than as one or more campsites to which food was brought from elsewhere. Avery (1988) reported on other sites at Elandsfontein that can be identified as the remains of hyaena nursery dens, with circular concentrations of bone and numerous coprolites, as well as others that he considered to be places where large mammals had died, probably from natural causes rather than as a result of being killed by carnivores, including humans.

As mentioned above, The Middle Stone Age (MSA) deposits at Klasies River Mouth (Singer & Wymer 1982; Voigt 1982) yielded the oldest record of the exploitation of shellfish in any quantity. Waselkov (1987: 123-124) distinguished between the 'small quantities' of shell found at Terra Amata in France (ca. 300000 B.P.) and the 'shell middens' of the South African coastal region (ca. 130000-30000 B.P.). Unfortunately, however, marine shell from the Klasies River Mouth excavations was only selectively retained - G. Avery (1990 pers. comm.) has said that only whole shells were kept - which vitiates any discussion of the role of shellfish in the diet of the caves' occupants. Thackeray (1988) published details of the marine shell recovered from H. J. Deacon's 1984-1987 excavations at Klasies River Mouth, a sample total of 13 532 that is considerably higher than those published by Voigt (1982, tables 13.3-13.7), whose tables contain apparently conflicting information regarding frequencies (e.g., table 13.3 does not include MSA III and Howiesons Poort, for which details are given in table 13.5; it gives the total for MSA II as 96 whereas table 13.6 gives a total of 3 174). Thackeray did not provide numerical statistics for the various species, but it is evident from his figure 1 that the brown mussel *Perna perna* was dominant in all but the MSA III, with frequencies generally exceeding 50 per cent of the unit total, whereas in Voigt's table 13.4 this species never reached 50 per cent of the unit total. In passing, it should be pointed out that Thackeray's table 2, which lists the taxa 'in descending order of relative abundance', omitted both *P. perna* and *Turbo sarmaticus*, the latter generally being third in order of relative abundance. In the circumstances, discussion of the contribution of shellfish to the diet of the occupants of the Klasies River Mouth caves is better left until publication of full information on the fauna from Deacon's excavations.

Brink & Deacon (1982) published information on a MSA shell midden at Herold's Bay near George in the southern Cape. However, their list of mammal fauna (table 1) was small: 9 (or 11) individuals, of which 6 (or 7) were bovids and one Cape fur seal *Arctocephalus pusillus*; and the only information provided regarding shellfish was a comment (p. 32) that *P. perna* was the dominant species 'but a number of other taxa are represented'. The report on this salvage excavation thus contributes little to our knowledge of the subsistence activities of the 'Middle Stone Age strandlopers' (p. 38) who occupied this site.

Marine shell and fish remains were not present in the MSA deposits of Die Kelders cave (Schweitzer 1970: 138), but the mammalian fauna (Klein 1975, table 1) include 26 Cape fur seal *A. pusillus* and 4 Cetaceae (whales and/or dolphins), and Klein (1975: 266) mentioned that 'penguin bones occur throughout'. This shows that the cave was relatively close to the sea at some time(s) during the 30 000 years or so of its Pleistocene human occupation (Volman 1981: 199), and it is thus somewhat surprising that the more easily transportable shellfish were not also brought back to the cave. Klein (1975: 266) considered that the absence of the remains of fish and flying sea-birds from the Klasies River Mouth and Die Kelders MSA deposits and their presence in the Late Stone Age (LSA) deposits of these sites suggested that 'active fishing and fowling may have been beyond the technological capabilities of MSA peoples' and that 'MSA peoples exploited coastal resources less effectively than LSA peoples in the same habitat'.

Klein (1976) reported on the faunal remains from the Duinefontein 2, about 4 km north of the village of Melkbosstrand and now in the boundaries of the Koeberg nuclear power station. The fauna in Klein's table 1 is exclusively terrestrial, although G. Avery (1990 pers. comm.) has advised that there was at least one bone of a jackass penguin *Spheniscus demersus*. Surface-collected material from the vicinity of the site included the remains of a probable Cape fur seal *A. pusillus*. Klein (1976: 19) concluded that the fauna, which includes a number of extinct species, is 'probably earlier Upper Pleistocene at youngest (older than 40 000 years) and may even be later Middle Pleistocene (older than 125 000 years) in age'. It is thus possible that the Duinefontein site indicates some exploitation of marine resources by MSA people.

Volman (1978) reported on MSA shell midden deposits at the Hoedjies Punt and Sea Harvest sites at Saldanha Bay. The faunal sample from the latter (Volman 1978, table 2) was small, but included a Cape fur seal *A. pusillus*, the vertebra of an unidentified fish, a bank cormorant *Phalacrocorax neglectus* and shellfish, mostly *Patella* spp. (88,3% of the sample total of 212). Thus, despite Klein's comments cited above, the Sea Harvest site may provide the earliest evidence in southern Africa for fishing and fowling, some 60 000 to 70 000 years ago (Volman 1978: 911).

Evidence for the exploitation of the resources of the coastal region during the late Pleistocene and Holocene (LSA) is more abundant than for the preceding periods but, as far as I am aware, the shellfish from almost every site has been sampled in such a way that only a relatively small proportion of the total amount excavated was retained, and the sampling methods differed from excavator to excavator. For example, Schweitzer (1979: 187) reported that in the excavation of the LSA deposits at Die Kelders 'all the shell from parts of selected grid squares and natural stratigraphic units that remained after sieving on a 3-mm-mesh sieve' was retained, and that 'the ratio of shell recovered from individual stratigraphic units relative to the excavated volume is extrememly variable, from 85,7 per cent in layer 2 to 13,9 per cent in layer 12'. On the other hand, in his excavation of the Stofbergsfontein shell midden near Saldanha Bay, Robertshaw (1978: 143) passed all the excavated material through sieves with meshes of 12 mm and 3 mm 'and the shell from the 12mm mesh sieve [was retained] from approximately every third bucket. The shell in the 3mm mesh sieve was sampled only occasionally'.

Sampling shell, understandable though it may be because of its large numbers in shell middens, militates against any attempt to determine the relative contribution of shellfish to the diet of the people responsible for the deposits. At best, it serves only to *characterize* the shellfish component; at worst, particularly when the bulk of the shell is retained from only a large-mesh sieve, it introduces a bias in favour of the more robust shell types, such as those of the gastropods, and against those of the more fragile ones, particularly the black, ribbed, and brown mussels *Choromytilus meridionalis*, *Aulacomaya ater* and *Perna perna*. I tested two samples of excavated mussel hinges or umbones, the part used for counts, that had been kept separate after sorting, according to whether they were from the 12 mm or 3 mm mesh sieve, and found that close on 93 per cent of the *C. meridionalis* hinges passed through the 12 mm mesh sieve, and 84 per cent of the *P. perna* (see Appendix). The bias resulting from selective sampling is evident in the differences between Voigt's and Thackeray's shell samples from Klasies River Mouth discussed above. Klein (1972: 185-186) reported that shell samples from his excavation at Nelson Bay Cave were taken only from the fraction remaining on the half-inch (12,5 mm), or larger, mesh sieve, which led to 'systematic under-representation of molluscs with more friable shells, especially of the mussels', and that analysis of unsorted shell samples had shown 'a small, but significant' proportion of the mussel fragments with umbones were smaller than 12,5 mm. He considered, however, that although the resultant bias allowed the results of the shell analysis to be used only 'in a gross way, to establish the relative importance of mussels vs. other molluscs . . . since the same bias was introduced into samples from all the middens, comparisons of samples as among middens are still meaningful'. With respect, and given the high percentage of umbones that passed through the 12 mm sieve in the examples given above, I cannot agree with Klein's conclusion. While it is true that my two samples may not be representative of all middens, it is not necessarily true that preservation of shell in all the units of a deposit ('middens' in Klein's terminology) is identical, so that the bias resulting from excluding the fraction that passes through the 12 mm sieve is not a constant. Given these problems, I am reluctant to comment in any detail on the shellfish samples from the coastal sites, particularly where they appear to provide evidence of changes in the proportions of species represented.

Global warming in the late Pleistocene resulted in rising sea levels and the flooding of the previously-exposed continental shelf (Dingle & Rogers 1972). This must have resulted in the retreat of the terrestrial fauna, including people, and the drowning of the latter's living sites. It also resulted in changes to the environment of sites that were previously in the interior but, by about 11 000 years ago, were within reach of the coast and its resources. Klein (1989 pers. comm.) has advised that there was minimal marine shell in the Brown Shelly Loam (BSL) deposits at Nelson Bay Cave, which are dated to 11950 ± 110 B.P. (UW-177), but that from the Crushed Shell Midden layer, dated to 11540 ± 110 (UW-162), shell was relatively abundant (dates from Klein 1972: 202). Apart from a doubtfully identified individual in the basal LSA layer, Cape fur seals *Arctocephalus pusillus* made their first appearance in the Crushed Shell Midden layer (Klein 1972, table 1). At Elands Bay Cave, which was occupied for about 30 000 years, the first marine shell lenses appear at about 11000 B.P., with fish and *A. pusillus* remains somewhat earlier (Parkington 1981: 344; fig. 12.4; table 12.3). The evidence from these two sites suggests that the exploitation of marine resources was probably the continuation of a practice developed long before, and that the appearance of the remains of these resources in the cave deposits reflects their increased accessibilty resulting from the closer proximity of the sea. Occupation of Nelson Bay Cave continued into the Holocene (Klein 1972, Inskeep 1987), while Elands Bay Cave was abandoned between about 7800 B.P. and 3800 B.P. (Parkington 1987*a*: 7), probably as a result of the Holocene marine transgression (Flemming 1977; Yates *et al.* 1986) having made the cave, or the marine resources of the area, inaccessible; and the same happened at the nearby Tortoise Cave (Robey 1987: 310; table 1).

Byneskranskop 1, which is situated about halfway between Nelson Bay Cave and Elands Bay Cave and, unlike them, not directly on the coast (Schweitzer &

Wilson 1982, figs 1 & 5), provided evidence of a temporal pattern of marine resource exploitation different from that of the other two sites. Although the site was initially occupied during the terminal Pleistocene (12730 ± 185 B.P., I-7948), and was probably never more than 10 km from the sea (Schweitzer & Wilson 1982: 10; table 1), the evidence for the exploitation of marine resources before about 8000 B.P. is sparse. Less than 10 per cent of the marine shell (all of which was retained) came from the earlier deposits, only a third of the relatively few (15) seals, and 3 per cent of the fish (Schweitzer & Wilson 1982, tables 15, 18 & 20. See also summary in their table 26). Although the radiocarbon dates for layer 1 suggest that the lowest sub-unit should probably have been kept separate from those overlying, the wide range of dates, from 3220 ± 45 B.P. (Pta-1631) to 255 ± 50 B.P. (Pta-1864), indicates that the cave was occupied relatively little after about 3000 B.P. The period of major occupation of the cave, and of the exploitation of marine resources thus coincides more or less with the period that Elands Bay Cave and Tortoise Cave were abandoned. The topography of the coastal plain in the area of Byneskranskop is not such that the Holocene transgression would have brought the sea appreciably closer to the cave, although changes in the vegetation may have occurred (Avery 1982, table 45). The time when shellfish began to play a more important role in the lives of the people of Byneskranskop is roughly coincident with the transition from a predominance of large medium mammals (>100 kg live mass) being brought to the site to a predominance of very small (<10 kg) and small (10-25 kg) ones (Schweitzer & Wilson 1982, tables 17 & 26). This phenomenon, the evidence for which was summarized by Schweitzer & Wilson (1982: 153-163) and Klein (1983), appears to have been common to most sites in the south-western and southern coastal region and in the interior of the Cape Fold Mountains region, and to have been approximately coincident with the development of the Wilton technology. Schweitzer & Wilson (1982: 163) commented that

> 'The reason for the general similarity of procurement patterns in the later Holocene cannot, however, be ascribed to climatic change inducing a general homogeneity in the ecology of the regions in which the various sites are located. It seems that there must be an overriding factor that is probably cultural, possibly demographic, but such an assumption cannot be tested until a greater body of information is available, from more than single-site observations, on patterns of human distribution and land-use'.

The change in technology after about 3500 B.P., marked by a reduction in the number and range of retouched stone artefacts and the apparent reversion to an 'Albany-like' toolkit, which was discussed in the previous chapter, does not appear to have been accompanied by a change in the subsistence base: the evidence for this period is much the same as that for the preceding 'Wilton', as indicated by the faunal lists for sites like Nelson Bay Cave (Inskeep 1987, tables 56-58 & 61), Byneskranskop 1 (Schweitzer & Wilson 1982, tables 15, 18 & 20) and Elands Bay Cave (Klein & Cruz-Uribe 1987, table 2, fig. 2; Poggenpoel 1987, table 1). This is not to say that there was *no* change at all, but rather that there was no *major* change, such as that observable between the faunal lists of the early and later Holocene.

After about 2000 B.P., when pastoralism was introduced into the coastal region, there would have been increasing competition between the hunter-gatherers and the herders for the use of the land and its resources. The evidence for the early part of this period is by no means clear, but the indications are that the earliest herders had only sheep, cattle being introduced some centuries later (Schweitzer 1979: 202-203; Smith 1987: 393-394). It is also not clear whether this early evidence for pastoralism reflects the incursion into the coastal region of people along with the domestic stock, or whether it reflects the gradual adoption by local hunter-gatherers of the 'pastoralist package' (Wilson 1986*c*, 1989*b*). Whatever the case, by the early historical period, the hunter-gatherers appear to have been largely restricted to the mountainous areas bordering the coastal region while the pastoralists controlled the coastal plain. Parkington (1984: 167-168) concluded that

> 'the appearance of pastoralism, reflected archaeologically in the inclusion of ceramics and particularly sheep bones into prepastoral contexts, resulted in the reorganization of hunter gatherer life into a pattern that survived to be historically described as soaqua. The visible elements of this life-style were greater use of isolated and fairly rugged parts of the landscape and the broadening of the resource base (Smith 1983) to include, or perhaps to emphasize, reliable and widespread but small food parcels such as underground geophytic corms, caterpillars, locusts, termites, a variety of shellfish, tortoises and rock rabbits. In some parts of the landscape alliances, or arrangements, were made with local pastoralists, and a certain amount of stock raiding helped supplement protein inputs from game hunting'.

Parkington (1984: 164) used the term *soaqua* 'not as denoting a tribe but rather, in lower-case letters as denoting a behavioral [*sic*] complex'. While I would agree in general with Parkington's description of the *soaqua* life-style, the major change seems to me to have been the loss of access to the whole of their previous territories and their resources, the degree of which depended on their relationship with the pastoralists, which was sometimes hostile, sometimes not. The resource base changed from that of the pre-pastoralist period only insofar as access to the various resources was concerned, and by the probably occasional addition of domestic stock. In the historical period, the white settlers with their guns were able to act more effectively than the Khoikhoi against the poisoned arrows of the 'Bushman' stock thieves who, by the end of the first quarter of the eighteenth century, probably included increasing numbers of Khoikhoi detribalized and impoverished by the plagues that affected them and their livestock (Elphick 1985: 37-40; 229-234).

The seasonal mobility hypothesis

For many years, conventional archaeological thinking has been that the coast was visited seasonally as part of the annual round of hunter-gatherers who spent the other part of the year in the interior. Put at its simplest, the 'seasonal mobility hypothesis' (Parkington 1972) postulates that because of a shortage or even absence of plant foods in the interior during the winter, hunter-gatherers would have been constrained to spend the winter at the coast, where they could supplement their diet with marine foods, and where the climate was more tolerable. This hypothesis was derived from a number of lines of evidence (summarized by Inskeep 1978: 105-114): the absence from, or paucity in, coastal deposits of the remains of edible plants and their presence in deposits in the interior along with inflorescences of grasses used for bedding that indicated summer occupation; the age at death of pups of the Cape fur seal *A. pusillus* that indicated their dying in the winter and similar studies on juvenile rock hyrax *Procavia capensis* from sites in the interior that indicated their death during the summer (see also Parkington & Poggenpoel 1971; Parkington 1972; H. J. Deacon 1976; Schweitzer 1979, etc.)

More recently, however, as Parkington (1987*a*: 18-19) put it, 'The original model of seasonal movement has been modified and challenged, giving rise to a new generation of research projects with particular spatial, temporal or methodological emphases'. Basically, the change has been the recognition that the pattern of seasonal movement was not rigid - summer in the interior, winter at the coast - but that it was adapted to changes in climate and environment such as those, for example, resulting from changes in sea level that would have increased or reduced the distance of sites, particularly cave sites, from the sea. These changes would have affected the availability of resources and consequently the scheduling of visits to certain areas. As examples of these, Elands Bay Cave was abandoned for about 4 000 years during the mid-Holocene (Parkington 1987*a*: 7), probably as a result of the marine transgression that occurred during this period making the marine resources inaccessible, while Nelson Bay Cave appears to have been suitable for year-round occupation for at least the last 10 000 years (Inskeep 1987: 293).

Demographic changes would also have played their part, especially those resulting from the introduction of pastoralism to the coastal region during the last

2 000 years and the expansion of white settlement during the last 300 years (Parkington 1984). Another factor recognized has been that seasonal movement may not have been just between the coast and the interior, but that some groups may have moved between the winter rainfall region of the western and south-western Cape and the summer rainfall region to the east and north-east (Parkington 1987*a*: 7). Parkington *et al.* (1988: 28) drew attention to the higher frequencies of hornfels and altered dolerite (presumably the 'shale' and 'other' of their table 3.1) in the Pleistocene deposits of Elands Bay Cave. They suggested that these raw materials were brought in from east of the Cape Fold Mountain Belt and that 'the most frequent kind of stone tool made from hornfels, a knife-like tool or scraper, resembles the concavo-convex scrapers which are widespread in the interior'. An implication of this, they suggested, might be that 'Pleistocene groups ranged over larger areas than their Holocene descendants'.

An aspect of the seasonal mobility hypothesis that seems not to have received the attention it deserves is that, while many inland sites have yielded evidence that their occupants had some sort of contact with the coast, coastal sites do not appear to provide evidence that their occupants were people who also lived in the interior, the evidence from Elands Bay Cave cited above being the exception rather than the rule. Marine shell has been found at sites in the interior, such as Melkhoutboom (H. J. Deacon 1976: 51), Wilton (J. Deacon 1972: 31), Boomplaas (H. J. Deacon *et al.* 1978: 54), De Hangen (Parkington & Poggenpoel 1971: 19), Observatory Shelter (Lloyd Evans *et al.* 1985: 106), and even as far inland as Koffiefontein in the Orange Free State (Humphreys 1970: 108-110), which is some 500 km from the nearest point on the coast. While it is possible that the shell from some of these sites, which is often in the form of ornaments, could have resulted from trade or gift exchange with people living nearer the coast, there appears to be nothing in the Holocene deposits of coastal sites that could not have been obtained from the coast itself, or from a few kilometres inland.

Sealy & Van der Merwe (1986*a*: 142), whose isotopic analyses of human skeletons in the Cape coastal region are discussed in the next chapter, pointed out that the evidence for seasonal occupation is based on *positive* indicators and that it 'does not address the very difficult problem of showing that winter occupation of the mountains and summer occupation of the coast did *not* occur'. Where sites in the coastal region are concerned, they contain many food items that are not season-specific: shellfish, adult mammals, rock lobsters, and fish among them. While a case can be made for filter-feeding bivalves such as mussels having been avoided during the summer out of fear of poisoning by eating them when they had been toxified as a result of a 'red tide' (Buchanan 1988: 83-86), eating grazing gastropods (limpets, perlemoen, whelks and alikreukel) and fish would not have lethal results; and terrestrial mammals and reptiles would have presented no such problem. These are all found in coastal deposits and it needs to be demonstrated that these did not provide the resource base at times when mussels and the like may have been avoided. Although archaeologists refer to material in sites as being *in situ*, it has actually all been disturbed by the activities of the occupants of the site, both during and subsequent to the occasion when the material was deposited. No matter how meticulously sites are excavated, I do not believe that current methods are capable of demonstrating that parts of the deposit (e.g., layers) relate only to a specific season, for all that they may contain specific seasonal indicators.

Another factor in the seasonal mobility hypothesis that must be questioned is whether marine resources are an appropriate dietary replacement for plant foods. This is discussed in the following chapters.

Pastoralists

The earliest evidence for the presence of pastoralists in the coastal region comes from the cave site of Die Kelders (Schweitzer 1979) and an open station shell midden at the relatively nearby Hawston (Avery 1974). This evidence is in the form of potsherds and the bones of domesticated animals: sheep at both sites and cattle as well at Die Kelders. In the previous chapter, it was pointed out that the presence of potsherds is not *de facto* evidence that the site was occupied by herders, and the same applies to the remains of domestic stock. These items were available, by fair means or foul, to hunter-gatherers in the area and it is thus necessary to exercise caution in interpreting their presence in sites.

Whether the herders of the coastal region were immigrants, or whether they were 'acculturated' former hunter-gatherers was discussed briefly above, as well as elsewhere (Wilson 1986*c*, 1989*b*), and I think that at present there is insufficient information available for this matter to be resolved with any certainty. The indications from sites like Die Kelders (Schweitzer 1979: 202-203, table 27) and Kasteelberg (Smith 1987; Klein & Cruz-Uribe 1989) are that the early herders had only sheep and that cattle were introduced later. If this was the case, the early herders may not have had the mat-and-withy huts that were so typical of them in the historical period and which were transported on their pack-oxen, and they may therefore have lived in caves and rock-shelters for at least part of the time, although I do not believe that this was the case. The present topography around Die Kelders is such that access to the cave is difficult enough for humans and, in my view, virtually impossible for sheep, let alone cattle. It is, of course, possible that the topography has changed considerably since the cave was last occupied some 1 500 years ago (in fact, that may have been the reason for its abandonment) but caves are, in general, impractical places in which to kraal domestic stock for any length of time. Apart from difficulty of access, many of them are small (Die Kelders and Elands Bay Cave being notable exceptions), and there would have been problems with access to water and feed unless the animals were taken out daily.

Klein & Cruz-Uribe (1989: 90-92,) hypothesized that if the two Kasteelberg sites were 'specialized stockposts/ sealing stations', where the sheep were concerned they 'would expect the middens to contain mainly young males and postprime females'. The reason for this assumption was that if the herders were culling their flocks they would select young males since rams do not produce milk and only few are needed for breeding purposes, while ewes that were no longer fecund could also be used for food. Klein & Cruz-Uribe were, however, unable to determine the sex of most of their samples, so they analysed the mortality profiles of the sheep from Die Kelders and the two Kasteelberg sites, but found that their prediction regarding adult sheep was not borne out by the profiles for Kasteelberg B and Die Kelders, where adults are less common than at Kasteelberg A. They found that there is a statistically significant difference between the age profiles of the sheep from Kasteelberg A and Kasteelberg B and between those from Kasteelberg A and Die Kelders. In the Kasteelberg B and Die Kelders samples, the bulk of the sheep were in the first 20 per cent of life-span, while in the Kasteelberg A sample there was a wider distribution, up to about 70 per cent of life-span, although there were still more in the first 20 per cent group (Klein & Cruz-Uribe 1989, fig. 8). They concluded, however, that occupation of Kasteelberg A 'was less seasonally focused than at either KBB or Die Kelders, and it could have extended into a portion of the year when the people had to rely more on sheep and less on wild animals (especially seals)' (Klein & Cruz-Uribe 1989: 91).

While I am content, on the basis of the relatively large number of sheep and cattle remains (Klein & Cruz-Uribe 1989, table 1), to accept that the Kasteelberg sites were occupied by herders, there is another interpretation that can be made of the age profiles of the sheep from Kasteelberg B and Die Kelders. This is that, male lambs and post-prime ewes being the least necessary to the maintenance of the flocks, they are the most suitable for barter with, or payment for service by, hunter-gatherers. As mentioned above, neither of these sites has a large number of adults that could be identified as possibly being post-prime ewes (beyond 40-50% of life-span: Klein & Cruz-Uribe 1989: 90), but there are not that many at Kasteelberg A, either: the table suggests that about a quarter of the total of aged individuals is in this age-group, and their distribution is not suggestive of the killing of sheep of specific age-classes within this group.

Kolb (1738: 171) commented that the Khoikhoi 'let the Bulls run with the Cows, and the Rams with the Ewes the Year round. By which Means their Herds encrease [*sic*] apace, their Ewes yielding them constantly Two Lambs in the Year'. He also commented (Kolb 1738: 185-186) that, despite the great numbers of cattle slain by the Khoikhoi during the ceremonies attending their rites of passage (called by him *Andersmakens*, a corruption of the Dutch *anders maak(en)*, to make different, or to change) 'their Herds often encrease too fast, and are greater than they can conveniently tend or find Pasture for'. In such a case, they would dispose of their surplus to the Europeans, or else to other Khoikhoi, from whom they would obtain tobacco, dagga (*Cannabis sativa*) 'or some other wanted Commodities', even though the exchange rate was much lower than they would demand on other occasions; 'And if yet there remains an Excess, they charitably and very cheerfully distribute it among their own Poor'. Although it may be doubted that occasions such as the last arose more than rarely, particularly in the early period, it suggests a method of redistribution of wealth and a subsistence strategy that would be beneficial to both donor and recipient. In this regard, the evidence of the Little Chariguriqua, former herders for the Cochoqua may be mentioned, although they were accused of having stolen the livestock entrusted to their care (Ch. 2). Whether this practice was also extended to hunter-gatherers who had a clientary relationship with them cannot now be determined, but it suggests a method by which hunter-gatherers could have become herders in their own right.

Although the Khoikhoi were recorded in the early historical period as having killed seals (Thom *ed.* 1952: 176) and caught fish with spears (Moodie *ed.* 1960: 93), I have been unable to find a single reference to their having collected and eaten shellfish - excluding, that is, the 'Strandlopers'. However, although the full information regarding the fauna from the Kasteelberg sites is still to be published, Smith (1987: 393-395) reported that Kasteelberg A contained 'large quantities of shellfish', though there was no mention of any in Kasteelberg B. If these sites are accepted as being pastoralist sites, then at least one of them provides evidence that shellfish were part of the Khoikhoi diet; and it is thus reasonable to assume that at least some of the shell middens of the post-2000 B.P. period are also attributable to them. The problem here will be to identify them with any certainty: the mere presence of potsherds and/or the remains of domestic stock is no guarantee of a site being the result of herder subsistence activity and, as mentioned in the previous chapter, the artefacts themselves provide no indication of the identity of their users.

Parkington (1984: 159) commented that 'the impression is gained that stock owners seldom hunted bovids for food but most frequently hunted carnivores in organized drives, partly to obtain desirable skins and partly to rid their herds of troublesome predators'. Parkington's impression was presumably gained from the early records, but these are far from complete with regard to the information archaeologists and ethnographers might desire. Kolb (1738: 203), although not always the most reliable of recorders, observed that

> 'The Victuals of the *Hottentots* are the Flesh and Entrails of Cattle and certain Wild Beasts, with Fruits and Roots of Several Kinds. But setting aside the Sacrifices . . . and other *Andersmakens*, the *Hottentots* rarely kill Cattle for their own Eating but when they are at a Loss for other Sustenance. The Cattle they devour between the *Andersmakens*, are, for the most Part, such as die naturally . . ..
> When, betwe[e]n the *Andersmakens*, Death, by Disease or Old Age, does not furnish them with Carcasses from the Herds, and the Men are not contented with Fruits, Roots, and Milk, provided wholly by the Women, they go a hunting or (if they live near the Sea) a fishing. They always hunt in Troops. Sometimes they bring Home a great Deal of Venison and sometimes they return without'.

Johann Schreyer (1668 *in* Raven-Hart 1971: 122-123) described the hunting of elephants, and the trapping of rhinoceroses and hippopotami in pitfalls; also

'Elands, deer, steenbuck go there in great herds. To catch them the Hottentots surround them with many men and draw these together little by little so that they enclose them, and sometimes they have a good catch'. He also mentioned that in the summer the 'Hottentots' killed 'all sorts of wild beasts come to drink from the hollows in which the water has collected . . . since often the water in the hills is dried up'. Thus, although it is not clear whether the Khoikhoi hunted regularly or only occasionally, perhaps seasonally, there are records that they did hunt; and the evidence from the Kasteelberg sites (Klein & Cruz-Uribe 1989: 84-85, table 1) also provides support for the ethno-historical information. At Kasteelberg, the numerical emphasis is on small bovids the size of grysbok/steenbok *Raphicerus* spp., with few small medium ones if the sheep are subtracted, and more large medium bovids (Cape hartebeest *Alcelaphus buselaphus*) than large after deduction of the cattle from the totals for the latter size group. Wild bovids are, however, not as commonly represented as domestic ones, and the most numerous of all the mammals is the Cape fur seal *A. pusillus*, which indicates the importance of at least one marine resource to the occupants of these sites. It will be interesting to see the full faunal tables, including those for shellfish and fish, when these are published; but in the meanwhile, the evidence is that herders in the coastal region exploited the natural resources of both land and sea in addition to their domestic stock, though perhaps not to the same extent as did the hunter-gatherers.

Farmers

Compared with that available for the Late Stone Age of the western part of the southern African coastal region, relatively little archaeological information has been published on the Iron Age farmers of the eastern coastal region; and much of what has been published is subject to the same caveat as applies to the post-2000 B.P. Late Stone Age sites: that the presence of potsherds and the remains of domestic animals is no guarantee that the sites that contain these can be attributed indisputably to the people with whom they are more usually associated.

Hall (1981: 148) observed that

> 'In addition to domesticated cereal crops, other resources were clearly of importance to the Early Iron Age farmers of the coastal areas. Some of the Group 1 sites are marked by fragmentary molluscan remains as well as by pottery sherds, indicating that the nearby marine fauna was exploited. This may perhaps have been a further reason [in addition to the initially nutrient-rich soils] for the concentration of settlement along the immediate coastline. In addition, the coastal forests would have provided a variety of edible wild fruits as well as habitats for small animals which could have been either hunted or trapped. It would seem unlikely, however, that domestic livestock were important. ... there is no evidence for cattle in this phase of the Early Iron Age elsewhere in southern Africa. In addition, ecological considerations make it seem unlikely that cattle had much of an economic role. Little grazing would have been available until the process of shifting agriculture was well advanced and secondary grasslands had become established. Such a change in the biotic structure could only have taken place after a sustained period of Iron Age farming' (my interpolation, taken from Hall's comments in the preceding paragraph).

Faunal remains from these sites appear to be rare: Voigt (1980: 94-95) reported that only 27 pieces of bone from Maggs's excavation at Mjonzani near Durban (Maggs 1980) were submitted for identification and of these only two tooth fragments came from the Early Iron Age deposits, while Mackay (1980: 95-96) reported that most of the marine molluscs came from the Late Iron Age midden features, only six of the total of 388 shells or fragments of shells having come from the Early Iron Age deposits. Voigt (1980: 94) considered that the tooth fragments might have been those of domesticated cattle *Bos taurus* but that 'neither fragment was large enough to unequivocally assign it to this species, so that undeniable evidence for the presence of domesticated cattle in this early period of the Natal Iron Age is still lacking'. A radiocarbon date of 1670 ± 40 B.P. (Pta-1980) was obtained from charcoal in the Early Iron Age deposits (Maggs 1980: 75). The few bone fragments from the Late Iron Age deposits included *B. taurus*, the small blue duiker *Cephalophus monticola* and fragments of bovids the size of sheep or goat, and Voigt (1980: 94) commented that 'The presence of *Cephalophus monticola* indicates hunting or snaring activities in forested or thicket areas'.

Where the molluscs identified by MacKay are concerned, all that can be said of the Early Iron Age sample is that it includes the land snail *Metachatina kraussi*, the oyster *Crassostrea margaritacea* and the brown mussel *Perna perna*. The Late Iron Age sample was dominated by *P. perna* (82,7% of the sample total), but included a few individuals of other species. MacKay (1980: 95) commented that the Early Iron Age midden 2 was contaminated by Late Iron Age material 'and in both cases there were very few shells, therefore this cannot be taken as proof of shellfish exploitation during the EIA occupation. However, evidence from other, similar sites is positive in this respect and it is probable that most shell as well as bone has weathered away over the intervening 1 700 years'.

Cronin (1982) reported on an Iron Age midden site at Mpame in Transkei which is dated to between 1310 ± 60 B.P. (Pta-2019) and 540 ± 55 B.P. (Pta-2017) (Cronin 1982: 38), so that the site encompasses both the Early and the Late Iron Age. Cronin did not provide separate information for the two phases and commented that 'The Mpame midden represents the remains of hunting, fishing and shell collecting activities' (Cronin 1982: 38). The faunal sample was dominated by *P. perna*: 'which forms over 90% of the shell debris' and other animal remains, 'occurring especially in the lower layers', included fish and terrestrial wild animals (Cronin 1982: 38) but apparently no domestic ones. However, Cronin (1982: 38-39) suggested that 'Home bases or villages were probably not located along the shore but in the immediate hinterland close to arable soils', so that even if the evidence for pastoralism and agriculture is lacking, it may be assumed to have formed the major part of the subsistence base of the Late Iron Age people, with hunting, fishing and shellfish collecting providing the bulk of the protein during the Early Iron Age. Cronin (1982: 39) commented that 'Early Iron Age groups, it is thought, indulged in intermittent but regular shell collecting based on a tidal cycle and primarily aimed at the harvesting of the *Perna perna* mussel'.

The evidence for the Iron Age in the coastal region, sketchy though it may be, is that the farmers, whether engaged only in agriculture (Early Iron Age) or in pastoralism as well (Late Iron Age), made use of the natural resources, terrestrial as well as marine, of the coastal region.

DISCUSSION

If the Early Stone Age people collected shellfish, they and their Middle Stone Age descendants may have done so to supplement the protein they obtained by hunting, at which they may not have been very proficient, and scavenging. The Late Stone Age people were, it seems, proficient at hunting and trapping terrestrial mammals as well as seals, in addition to collecting tortoises, the remains of which are generally abundant in archaeological sites. They, at least, would thus appear to have had little or no *need* to collect shellfish, and it must therefore be presumed that they did so from choice.

Yesner (1980: 729-730) pointed out that shellfish 'exist as a highly concentrated resource, are easily collectable by all segments of the human population with a minimum of energy input, and often serve as an emergency buffer during times of relative food scarcity'; and he considered that shellfish collecting is not labour-intensive, but did not explain why. To my mind, considering yield in terms of effort, collecting shellfish *is* labour-intensive - perhaps more correctly, labour-*expensive* - though the degree of effort varies according to the type of mollusc being collected. Sessile and colonial bivalves such as mussels can be fairly easily removed in large numbers with the aid of an implememt such as a digging stick, but will then include numbers of small individuals with little or no food value. Gastropods, which are generally distributed over wider areas than mussels, must be collected individually and, although in this case the reward in terms of flesh mass is determined by the collector's selectivity and may thus be greater than the unselective removal of mussels, the effort involved is greater than that of collecting mussels. Osborn (1980: 740-741) pointed out that 'shellfish are small-body-sized food resources and exhibit high shell-to-meat ratios; protein and energy content is low, [and] processing time is high'. He mentioned that a white-tailed deer of 64 kg live weight contains more calories than a metric ton of *Mytilus* sp. shellfish; and that a llama of 90 kg live weight yields 12,58 kg of protein, or the protein equivalent of 135 269 mussels with a live weight of 4 329 kg. The mean of 31,25/kg for Osborn's mussels is high compared with about 300/kg for *Perna perna* from the Transkei coast (Bigalke 1973:173), which makes comparisons with southern African bovids and shellfish problematic, but the weight equivalent of the deer would be a male bontebok *Damaliscus dorcas dorcas* and of the llama eight male steenbok *Raphicerus campestris* (Smithers 1983: 613, 640), so that an 11-kg male steenbok would have a weight equivalent to about 3 300 brown mussel *P. perna*.

An experienced man would expend little effort in erecting a snare to trap a steenbok, but he might have to travel some distance from the camp to find a suitable place; and the rewards would not be immediate or guaranteed. On the other hand, the women and older children could have gone to the shore and in a relatively short time collected a quantity of mussels sufficient to provide a meal for the band. Griffiths (1981: 107) reported densities of black mussel *Choromytilus meridionalis* in the range of 5 500-6 000/m^2, and Zoudendyk (1989: 18) *P. perna* in excess of 5 000/m^2. These densities are for mussels of all sizes, and if they were collected *en masse* a number of juveniles of little or no food value would be included. None the less, and despite the fact that the steenbok's flesh might have been the *preferred* food, the mussels would provide the *certainty* of sustenance, so that even if their collection was more labour-expensive than the snaring of a steenbok, in terms of survival it would have been worth the effort.

Osborn's information supports my contention that the labour cost of shellfish collection is high in terms of flesh yield, but shellfish are generally a more reliable food source than antelope, regardless of cultural preferences. If the day's hunting was not successful, marine resources would provide an alternative source of protein that would justify the amount of time and energy spent obtaining it by collecting shellfish and rock lobsters or by fishing. These are, however, not always available - for example, during the high seas that result from storms and gales, when the intertidal and subtidal zones may be inaccessible for a week or more. The advantage of living in the coastal region is that there are two resource zones to exploit, one of which will always be open for exploitation.

The archaeological record does not provide any evidence for periods when terrestrial foods were scarce, although it is probable that during the course of the year, chiefly in the winter, carbohydrate-rich plant foods may have been. However, as mentioned above, marine fauna are not an appropriate dietary alternative to plant foods, so that the seasonal unavailability of the latter cannot be adduced as even a partial explanation for seasonal occupation of the coastal region or for the exploitation of marine resources; and here again it must be mentioned that, while there is evidence that coastal sites were occupied during certain times of the year, there is no evidence to show that they were *not* also occupied during the rest of the year. Goodwin (1946: 5) reported that he had 'watched a man collect, cook and devour a petrol-tin full of shell-fish'. On that basis, he calculated that in a year six people would accumulate over 50 m^3 of uncompressed shell or 5 m^3 of compressed shell; and he concluded that 'It is clear that a considerable bulk of fish shell [*sic*] does accumulate'. If Goodwin's calculations are anywhere near accurate, they provide good support for the suggestion that people did not eat shellfish - at least, not in those quantities - throughout the year. Were they to have done so, it is likely that the middens that have accumulated during the past 2 000 years or so since the sea reached its present level would have been considerably bigger than they are, and coastal caves would have been filled rapidly. This does not, of course, mean that Goodwin's suggestion can be used to support the seasonal mobility hypothesis, especially since we have no valid information regarding the size of the groups who occupied the coastal region during the course of its history of human occupation, or of the size of their territories.

In the Appendix, I have dealt at length with Buchanan's (1988) reconstruction of prehistoric human diet, concentrating on that for the Elands Bay area; but what may be said here is that I consider Buchanan's major error to have been one of interpretation, rather than merely mathematical. In the introduction to his study, Buchanan (1988: 10) said that he had chosen to 'focus on the period from 1800 to 300 BP characterized by the presence of pottery'; and in his concluding discussion (Buchanan 1988: 105) he observed that 'With the first appearance of pottery and domestic stock in the archaeological record about 1800 BP, signs suggestive of radical change in the economic and possibly social systems emerge', yet his reconstruction completely ignores the cumulative effect that this must have had on the hunter-gatherers and their way of life. In Buchanan's reconstruction, the total energy budget for the area was calculated, then divided by 1 500, the length of the period selected for the reconstruction, and allocated to a hypothetical hunter-gatherer group on the basis of their assumed minimum daily energy requirements. The result of this is that the diet of these people was taken to be completely unchanging, so that even sheep and cattle, subsumed under the general category of terrestrial mammals, would have been available at the same proportional annual rate as all the other dietary components throughout the whole of the 1 500 years.

While it is true that we know next to nothing about the impact of the introduction of pastoralism on the lives of the hunter-gatherers of the region, and very little about its development beyond the fact that cattle appear to have been introduced some centuries after sheep, its implications cannot simply be ignored, particularly in the light of the evidence that, by the time of the first European contacts, the pastoralist Khoikhoi controlled virtually the whole of the coastal region of the western half of the subcontinent. Buchanan's failure to take this into consideration in his reconstruction allows it to be assumed either that the introduction and development of pastoralism had no effect whatsoever on the lives of the aboriginal hunter-gatherers or, if the short period of the annual visit, an average of 10 days, by a small group of about 18 people (Buchanan 1988: 94-95) is accepted, that the initial impact was such that from that time onward the hunter-gatherers were able to spend only a short time at the coast each year, perhaps when the pastoralists were elsewhere, though Buchanan (1988: 99) suggested that 'pastoralist incursions may have restricted [hunter-gatherer] movement into the *interior*' (my emphasis) - the very part of the country in which the San were most commonly met during the early historical period. I believe, however, that the short period and small number of people is the result of a serious underestimate in Buchanan's calculations of the total energy budget contained in the sites he used for his reconstruction. Buchanan's reconstruction also does not allow for the possibility that some of the midden debris is the result of the activities of pastoralists.

The archaeological evidence for all the groups except those who lived during the Early Stone Age, for whom comprehensive information is lacking, is that they exploited the resources of the marine and terrestrial elements of the region, the hunter-gatherers most of all, the herders and early farmers perhaps less so, and the later pastoral-agricultural peoples probably least of all. The nature of this exploitation can be seen to have varied through time and in space, though the reality of the latter can be demonstrated only through intensive regional surveys. Single-site investigations have been, and will continue to be, useful but they cannot provide the necessary information on the changing patterns of land-use and resource exploitation. There appears to be less difficulty in distinguishing between sites in the eastern coastal region that were occupied by hunter-gatherers and those occupied by farmers than there is in distinguishing between hunter-gatherer and herder sites in the western part, largely because these two groups seem to have shared a common artefact technology, but perhaps also because 'typical' herder sites, like those at Kasteelberg, are still mostly undiscovered or have been destroyed by agricultural development.

Discussion of subsistence, as indicated by dietary inferences from stable isotope analyses of human bone, is continued in the following chapter.

CHAPTER EIGHT

ARCHAEOMETRY

ISOTOPES AND PROTEINS IN COASTAL DIET

Sealy (1986, 1989) and Sealy & Van der Merwe (1985, 1986*a*, 1987, 1988) studied prehistoric human diet by means of analyses of the ratios of stable isotopes of carbon ($\delta^{12}C$ and $\delta^{13}C$) found in human bone. They suggested that these ratios can be used to determine the proportion of marine versus terrestrial foods consumed by the individuals whose skeletal remains they studied.

The analytical processes and their rationale have been fully discussed by these authors, and for the purposes of this study it is only necessary to state they considered that, in a biome with a C_3 flora, a low negative $\delta^{13}C$ value of around -11‰ in human bone collagen is taken to be indicative of a marine diet, while a higher $\delta^{13}C$ value of around -21‰ is taken to be indicative of a terrestrial diet. Intermediate values are taken to indicate a mixed diet (Sealy & Van der Merwe 1988: 89).

Sealy's first monograph dealt with 19 human skeletons for which radiocarbon dates had been obtained, 12 of which came from coastal contexts and 7 from inland contexts. Those from coastal contexts had $\delta^{13}C$ values ranging from -11,2‰ to -17,4‰, while the values for those from inland contexts ranged from -16,0‰ to -19,0‰ (Sealy 1986, tables 20 & 21). This led to the conclusion that 'inland dwellers consumed very little marine-based food, whereas coastal dwellers ate considerable amounts' (Sealy 1986: 89). Sealy & Van der Merwe (1986*a*: 142), whose paper dealt with 14 of the same skeletons, concluded that

> 'the isotopic data clearly do not support the prevailing seasonal-mobility model . . .. Most of the indicators used to demonstrate summer occupation of the mountains and

> winter occupation of the coast are *positive* indicators. The case for both phenomena is fairly convincing but does not address the very difficult problem of showing that winter occupation of the mountains and summer occupation of the coast did *not* occur. This is the crux of the problem' (authors' emphases).

This interpretation was questioned by Parkington (1986: 145-146), who commented that, while Sealy and Van der Merwe had made 'a substantial contribution to our knowledge of Holocene subsistence patterns in the Cape', the seasonal-mobility model they had used had 'already been tested, found inadequate, and modified'. Parkington observed that 'An obvious limitation of carbon isotope readings is that they represent a distillation of several quite different constituents, an average of a set of averages, an answer for which there are numerous questions'; and that it had been shown 'that many stable carbon isotope readings are obtained from samples with C:N ratios quite different from those of fresh bone and that these are likely to give highly inaccurate results'. He concluded that 'the question of seasonality has to be considered independently of the isotope readings, if only because the carbon isotopes say nothing about the processes or timing of admixtures'. The 'admixtures' referred to are those of terrestrial foods or marine foods such as whales, which would deplete the reading, while seals, crayfish and birds would enrich it.

In reply to Parkington's comments, Sealy & Van der Merwe (1986*b*: 148), while conceding the validity of his point about the δ^{13}C readings representing an average of many possible diets, pointed out that

> 'All over the world, coastal peoples exhibit a range of δ^{13}C values (depending on how heavily they rely on marine foods), but there is, in areas with C_3-based terrestrial food webs, a fairly sharp cut-off point at the positive end of the range of about -11°/oo . . .. It therefore seems likely that this is the most positive δ^{13}C value possible for a marine diet'.

They commented further that

> 'We would agree that most broad archaeological reconstructions are site-based, whereas our technique is individual-based. This is obviously a problem if one is attempting to compare and contrast results obtained with the two approaches. We are not yet sure how to bridge this gap; ultimately the isotopic data ought to form just one part of the greater archaeological picture.'

However, in a subsequent paper, dealing with the same set of skeletons, Sealy & Van der Merwe (1987: 263) were able to assert that 'The seasonal mobility hypothesis is directly testable by means of stable carbon isotope ratio measurements'. They were satisfied that the isotopic evidence of skeletons with very positive $\delta^{13}C$ values (i.e., around -11‰) was proof that the economy of at least some of the people whose remains were studied had been based largely on marine foods. They saw this as tending to support the reports of the early European settlers of 'strandlopers' living in the way indicated by the isotopic data; and they commented that 'The isotope data greatly increases the time depth of this lifestyle, extending it back at least as far as the beginning of the Holocene' (Sealy & Van der Merwe 1987: 264, 266).

The most recent paper on this subject by Sealy & Van der Merwe (1988) dealt with 75 human skeletons, all from the south-western Cape coastal region (Elands Bay to Gordons Bay). Their table 1 includes details of the places of origin of the skeletons, their $\delta^{13}C$ bone collagen values, radiocarbon dates and the degree of dental wear.

Sixteen of the skeletons (21,3% of the total) have $\delta^{13}C$ values of -12‰ or less, while 12 (16,0%) have values between -12‰ and -13‰, 26 (34,7%) have values between -13‰ and -15‰ and 21 (28,0%) have values greater than -15‰. Sealy & Van der Merwe (1988: 92-93) considered that values below -13‰ indicated a predominantly marine diet, while values of -12‰ or less 'allow almost no terrestrial protein intake'. On this basis, therefore, almost two-thirds (62,7%) of the sample consumed more terrestrial than marine foods.

Radiocarbon dates were obtained for 50 of the skeletons, ranging in age from about 6000 B.P. to 440 B.P. Of these, the 8 skeletons that are older than 3000 B.P. have $\delta^{13}C$ values ranging from -11,2‰ to -17,6‰ (mean -14,2‰). The values for the 23 dated to between 3000 and 2000 B.P. range between -11,5‰ and -17,9‰ (mean -13,0‰); and those for the 19 dated to 2000 B.P. or younger are in the range of -12,9‰ to -17,5‰ (mean -15,0‰). Only 2 individuals in the pre-3000 B.P. group have values lower than -13‰ and only 1 in the post-2000 B.P. group, while in the 3000-2000 B.P. group 13 individuals, or just over half the total, fall into this category. This led Sealy & Van der Merwe (1988: 92-93) to conclude that 'These individuals clearly ate a predominantly marine diet', while the values for the post-2000 B.P. group reflect 'a more mixed diet with a greater terrestrial component than in the preceding millennium. . . . Such a pattern accords well with the greater variety of food waste found in sites dating to the last two thousand years'. That the greatest number of dated skeletons in the sample with low negative values falls into the 3000-2000 B.P. period is interesting, since this is the period into which the bulk of the west-coast 'megamiddens' fall (Parkington 1987: 11; fig. 2), and these certainly seem to be indicative of a rate of exploitation of shellfish unparallelled in this area in the preceding or succeeding periods.

Responding to the suggestion by Parkington (1987*b*: 93) that 'Plants with very high protein content could contribute more to collagen readings than sets of starchy but protein-poor plants', Sealy & Van der Merwe (1988: 93) commented that 'even if the most extreme scenario is true, and carbohydrate foods are not represented in collagen $\delta^{13}C$ values, readings of -11[‰] and -12‰ allow almost no terrestrial protein intake. Thus we believe that these individuals must have spent their lives very largely at the coast'. In this regard, it is worth noting that only 16 (21,3%) of the 75 individuals in Sealy & Van der Merwe's sample have $\delta^{13}C$ values of -12‰ or less. Of these, 7 are undated, 2 are older than 3000 B.P. and the remaining 7 are dated to between 3000 B.P. and 2000 B.P. (Sealy & Van der Merwe 1988, table 1). The conclusion to be drawn from this is that the bulk of the individuals in the sample ate some terrestrial foods.

Sealy & Van der Merwe's study also included examination of the implied dietary differences between the men and women represented in their sample. They found that although the women's diets appeared to have changed little through time, they were more variable than the men's. 'Men's diets, on the other hand, clearly shifted to include a greater terrestrial food intake in later time periods. Men's and women's diets were more similar in the post-2000 [B.P.] period than they were earlier in the Holocene' (Sealy & Van der Merwe 1988: 95). The difference in the women's diets was attributed to their possibly having eaten more plant foods while they were out collecting, but this habit of 'snacking' does not appear to have been applied to marine foods.

Sealy (1989, table 1) provided data for a total of 72 dated and two undated skeletons that includes all but one (SAM-AP3458) of those dealt with in the paper by Sealy & Van der Merwe (1988) discussed above. Sealy (1989, table 3), taking into account the standard deviation of each radiocarbon date, grouped the skeletons according to date. (In this regard, it should be noted that Sealy (1989 pers. comm.) advised that the date for SAM-AP4203b was incorrectly given by the dating laboratory as 4760 ± 50 B.P., whereas it is actually 2760 ± 50 B.P.)

Of the 11 individuals in the pre-3000 B.P. group only 2 (males) have $\delta^{13}C$ values of -12‰ or less and another 2 (also males) have values between this and -13‰. In the 3000-2000 B.P. group, 9 of the 28 individuals have $\delta^{13}C$ values of -12‰ or less (4 males, 2 females, 1 juvenile and 2 unidentified as to gender) and 8 (5 males, 2 females and 1 unidentified) have values between this and -13‰. In the ±2000 B.P. group, 2 of the 6 individuals (1 male, 1 juvenile) have $\delta^{13}C$ values of -12‰ or less but none have values between this and -13‰. In the post-2000 B.P. group 3 of the

27 individuals (2 males, 1 female) have $\delta^{13}C$ values of -12‰ or less and 2 (1 male, 1 female) have values between this and -13‰.

These data support those for the smaller dated sample in the joint paper discussed above (Sealy & Van der Merwe 1988). More males have lower negative $\delta^{13}C$ values than females: 8 have $\delta^{13}C$ values of -12‰ or less and another 8 have values between this and -13‰, while only 3 females have $\delta^{13}C$ values of -12‰ or less and another 3 have values between this and -13‰. The temporal trend is also confirmed, with 11 of the 39 individuals in the pre-2000 B.P. period having $\delta^{13}C$ values of -12‰ or less and 10 having values between this and -13‰ (53,8% of the total for both groups of isotope values), while in the ±2000 B.P. and post-2000 B.P. periods only 9 of the 33 individuals have $\delta^{13}C$ values of -12‰ or less and only 2 have values between this and -13‰ (33,3% of the total for both groups). It should be borne in mind, however, that the overall number of individuals who, according to Sealy's and Sealy & Van der Merwe's calculations, had a largely marine diet is small: 16 of the 72 (22,2%) have $\delta^{13}C$ values of -12‰ or less and 12 (16,7%) have values between this and -13‰. In other words, some 60 per cent of the sample had a higher terrestrial food intake than a marine one.

A further aspect of Sealy & Van der Merwe's research was a study of dental caries and tooth-wear patterns in the skeletons in their sample. Tooth wear was found to be slightly heavier in females than in males, and this was attributed to possible differences in diet or in occupational activities (Sealy & Van der Merwe 1988: 96). A very low incidence of dental caries was observed: 1,1 per cent in the males and 2,2 per cent in the females. Caries was totally absent from the teeth of individuals with $\delta^{13}C$ values of -13‰ or less and found in only 3,9 per cent of individuals with values greater than -15‰ (Sealy & Van der Merwe 1988, table 4). The higher incidence of caries in the teeth of women 'probably supports the suggestion of women's greater involvement with terrestrial plant foods' (Sealy & Van der Merwe 1988: 97).

That people lived exclusively, or more or less exclusively, on marine foods was questioned by Noli & Avery (1988) on physiological grounds. Discussing the role of the major dietary components - carbohydrates, fats and proteins - they pointed out that although there is no limit to the amount of carbohydrates and fats that can be consumed safely, this is not the case where proteins are concerned. Moreover, it was clear from the various case studies cited by Speth (1987) ‘that consumption of lean meat alone led to symptoms of starvation and protein poisoning within a week, diarrhoea within 7-10 days, severe debilitation within 12 days and the possibility of death within weeks’. Thus, despite uncertainty about the maximum amount of protein that can be consumed with safety, ‘it is clear that people cannot survive long on protein alone, and, furthermore, that there are severe limitations on the amount of protein-rich foods that can, in the short term, safely be consumed by humans’. They also considered that the amount of protein, about 400 g, estimated by Speth (1987) to be the maximum that could be consumed safely per day, to be ‘an overestimation of the ability of humans to utilize protein as a major source of energy and [which] would not have been possible for long’ due to the physiological and metabolic factors they mentioned (Noli & Avery 1988: 396-397).

Noli & Avery (1988: 399) further pointed out that Kruger & Sullivan (1984) had suggested ‘that carbon isotopes do not reflect the total diet in humans, but only the protein component. This would mean that the use of plant foods would not register isotopically. If this is indeed so, it is not surprising that the majority of values for coastal people reflect a very strong marine diet’.

Pointing out that ‘It would have been particularly dangerous on the coast to live on the flesh of shellfish, fish and marine mammals for longer than a few days without the inclusion of a carbohydrate- or fat-rich source of energy’, Noli & Avery (1988: 399) concluded that ‘in view of this, existing hypotheses and dietary reconstructions which assume that coastal hunter-gatherers were able to subsist entirely on protein-rich diets for protracted periods extending over months, need to be reassessed'.

Speth (1989: 330-331) cited a personal communication from G. F. Cahill Jr that the apparent upper limit of lean flesh mass that the body can safely handle 'is about 300 g or roughly 50 per cent of one's normal total daily caloric intake', although conceding that 'The precise nature and value of this limit, however, remain poorly documented and controversial'.

Perhaps the most important aspect of Speth's paper is the attention he drew to the need to offset the effects of potential protein poisoning by including, in particular, large amounts of fat in the diet. Proposing that about 300 g of protein, or 50 per cent of total per capita daily caloric intake under normal, non-stressful conditions is 'the approximate upper limit that can be consumed safely on a sustained basis', Speth (1989: 333-334) pointed out that 'The extent to which this threshold varies among foraging populations because of genetic factors, or is affected by the absolute amounts or relative proportions of fat and carbohydrate in the diet, remain[s] unknown' and that 'input from medical and nutritional specialists, as well as more detailed long-term studies of protein intakes among foragers in different ecological settings and under different dietary regimes are critically needed to clarify this issue'.

Speth (1989: 334) cited Stefansson (1944: 90; 1956: 31, 212-213), who had experimented on living on an all-protein diet and suffered ill-effects, but not when he lived on a diet of pemmican, as arguing that the ideal mixture for pemmican was 'about one pound of fat for every six or seven pounds of lean meat', which is a minimum of 12,5 per cent of fat relative to the total meat and fat mixture.

Buchanan (1988, tables 5.21 & 5.22) estimated the percentage contribution of the three major constituents of the reconstructed diet of the the Elands Bay sites: 55,4 per cent protein, 33,6 per cent fat and 11,0 per cent carbohydrate; and that of the south-western Cape sites: 58,5 per cent protein, 35,2 per cent fat and 6,3 per cent carbohydrate. If it is assumed that Speth's estimation of the maximum of 300 g of protein, or 50 per cent of the total daily caloric intake, is correct, then Buchanan's estimates appear to indicate a somewhat excessive intake of protein. On the other hand, if Speth's meat:fat ratio given above is taken as being accurate, then the percentage of fat and carbohydrate in Buchanan's estimates should have been more than adequate to offset the excess protein intake. However, as stated previously, I have reservations about the acceptability of the methods by which Buchanan made his dietary reconstruction. These are evaluated in the Appendix, and although I have not discussed the methods by which Buchanan arrived at the ratios for the proportions of protein, fat and carbohydrate in his dietary reconstruction, it follows that if the reconstruction is incorrect the ratios must also be incorrect.

It must be borne in mind that marine fauna, whether molluscs, fish or mammals, do not consist entirely of protein, and that the human digestive system is capable of adapting to different diets. Unfortunately, little has been published on the biochemical constituents of southern African molluscs, particularly with regard to those species that are the most common components of shell middens: limpets *Patella* spp., mussels *Choromytilus meridionalis*, *Aulacomaya ater* and *Perna perna*, also perlemoen or abalone *Haliotis midae* and alikreukel or top-shell *Turbo sarmaticus*. Lombard (1977, table 11) gave information for *T. sarmaticus* showing that in the samples of this gastropod he analysed the protein content was in the range 63,75-79,62 per cent (mean 71,07%), fat 3,9-6,15 per cent (mean 4,95%) and carbohydrate 2,88-17,68 per cent (mean 11,11%). Seasonal variation was observed, with protein values at their lowest in mid- to late summer (February-April), and at their highest in the late winter to early summer (September-November). Waselkov (1987, table 3.4) provided similar data for various species and in every case the protein content considerably exceeded the combined fat and carbohydrate content.

P. Zoutendyk (1990 pers. comm.) mentioned that during gametogenesis mollusc gonads produce relatively large amounts of lipids. The eating of sexually mature shellfish during this period would therefore help alleviate the debilitating effects of a high-protein intake. Unfortunately, however, little research has been done in this regard and, as far as is known, none with regard to human diet. A problem here is also that the period of gonad activity varies according to species and, in some cases, locality. Zoutendyk (1989: 4) observed that spawning of the brown mussel *Perna perna* 'is intermittent, taking place from autumn to spring' (approx. May-September). He cited Berry (1978) as reporting two main peaks in winter and spring in Natal, while Lasiak (1986) recorded spawning taking place between February and September (midsummer to spring) on the Transkei coast. Griffiths (1981: 114-115) reported of *C. meridionalis* that 'The spawning season differs in duration in males and females, in different sized individuals and in successive years'. Where limpets are concerned, Branch (1974: 121, 126-127) recorded that *Patella argenvillei, P. barbara, P. cochlear, P. granatina* and *P. granularis* spawn chiefly in June, with some extension from May and into July (midwinter), while in *P. oculus* spawning occurs irregularly from August onward (late winter/spring) and *P. longicosta* spawns sporadically from late September into December (spring-summer). Lombard (1977: 81) observed differences in maximum gonad activity of *T. sarmaticus* at two localities on either side of Cape Recife near Port Elizabeth. At Flat Rock, where the water is warmer than at Skoenmakerskop, gonad activity was highest between September and January (spring to midsummer), while the peak at Skoenmakerskop was in December. However, as shown above, the lipid content of *T. sarmaticus* (at Skoenmakerskop) only reached a maximum of 6,15 per cent. Moreover, as indicated in Lombard's table 37, the seasonal peak in lipid content was not the same in 1976 (January) as it had been in 1975 (April).

Whether these relatively low amounts of fat and carbohydrate would have been sufficient to mitigate the effects of a high intake of protein, especially over an extended period, is a matter for dieticians to determine; but a problem here is that a living population on which to test this might not have the same physiological ability to metabolize such foods as did the indigenous populations who may have lived in this way. It is, however, arguable that people who lived on a high-protein diet for part of the year and a high-carbohydrate diet, or even a 'balanced' one, for the rest of the year would probably have had problems in making the necessary physiological adjustment from season to season unless the transition was gradual.

In 1654 it was recorded that 'Herry's allies', presumably the Goringhaiqua, 'were busy melting oil from the blubber of the dead whale, (which they preserved in the dried *trombas* [the giant kelp *Ecklonia maxima*] . . .) with which they explained that they grease themselves, and if they get bread from us, dip it in and thus eat' (Moodie 1960: 46). Dapper (1668 *in* Schapera *ed.* 1933: 57) commented that 'The Saldanhars or Cochoquas and other neighbouring Hottentots live in the dry summer on their cattle, but in the winter on certain roots growing in the ground in the rainy season', i.e., the winter. Dapper was, however, never at the Cape (Schapera 1933: 2), and his comments should therefore be treated with caution. The indications are, however, that fats and carbohydrates were part of the diet of the Khoisan; and it is unlikely that the faunal remains from archaeological sites represent only the protein intake of the people responsible for the deposits.

Francalacci (1989: 109) observed that 'Trace element analysis is a useful tool for reconstructing the dietary habits of ancient human populations, but its relative reliability and the various technical aspects of the method are still debated'. The method would thus seem to be at much the same level of development as isotope analyses, but appears to have the same potential as these for elucidating prehistoric human diets, and it could be used as a control against which to check the results of isotope analyses. Discussing the potential of the method, Francalacci mentioned that high levels of zinc found in human bones indicated that marine molluscs were an important food source to the people whose remains he studied. It would therefore seem to be of interest, and probably of value to researchers such as Sealy and Van der Merwe, if trace element analysis were integrated into their palaeodietary research as a complement to their isotope analyses.

DISCUSSION

What is evident from the foregoing is that, whether or not isotopic studies such as those of Sealy and Van der Merwe do accurately reflect the type of protein, marine or terrestrial, or combinations of both, attempts at reconstructing the diets of the indigenous peoples must give consideration to the probability that fats and carbohydrates played an essential part in their diet. The fact that most coastal deposits, whether in caves or in the open, contain a considerable terrestrial component cannot be ignored; and if isotope values - δ^{13}C or δ^{15}N - for dated skeletons do not accord with the evidence from similarly dated archaeological deposits, then the interpretation needs to be re-evaluated. However, it must be borne in mind that there is not a one-to-one correlation between the dietary component of archaeological debris and the diet of a similarly-dated skeleton, which can only be *inferred*; also that the interpretation of the isotopic evidence is that most of the individuals studied by Sealy and Van der Merwe included terrestrial foods in their diet.

It is of some interest that only 5 of the 27 skeletons (3 male, 2 female) in the post-2000 B. P. group have δ^{13}C values below -13‰. The possibility must be considered that some of these skeletons are those of Khoikhoi, who had access to the flesh of their domestic stock and thus a lesser need to supplement their diet with marine foods than may have been the case with the San hunter-gatherers. It is doubtful, however, that isotope analyses such as these will, on their own, be able to provide a reliable means of distinguishing between the two groups. It is, moreover, open to question as to whether either the Khoikhoi or the San actually *needed* to supplement their diets with marine foods: choice, rather than need, is more likely to have been the determinant, given the archaeological evidence that there seems always to have been an adequate supply of terrestrial animals of one kind or another, although in the terminal Pleistocene and early Holocene the transition from extensive grasslands to more closed, shrubby or forest vegetation seems to have reduced the availability in the coastal region of gregarious bovids and equids (Klein 1980).

If problems and lacks, such as those mentioned by Speth (1989) exist with regard to extant populations, extrapolation of such data as are available and may be obtained in the future and their application to the extinct populations studied by archaeologists can produce results that, at best, will be only conjectural. Certainly, for example, it would be most unwise to attempt to apply information on the Kalahari San (Speth 1988: 335-336) to populations who lived in the southern African coastal region, whether San or Khoikhoi, since the environments and their resources are vastly different.

Isotopic studies such as those undertaken by Sealy and Van der Merwe offer a provocative challenge to conventional archaeological interpretation; however, I can only echo Speth's comment that 'input . . . [is] critically needed to clarify this issue' (Speth 1988: 334).

CHAPTER NINE

ARCHAEOBOTANY

PLANT REMAINS FROM COASTAL SITES

Plant foods are an obvious dietary resource to mitigate the effects of a high intake of protein, and there is abundant ethnobotanical evidence for the use of plant foods, both endemic and exotic, by the indigenous peoples of southern Africa (e.g., Watt & Breyer-Brandwijk 1962, Smith 1966, Fox & Norwood Young 1982). There is also a fair amount of archaeological evidence in this regard, but most of it comes from sites in the interior (e.g., Parkington & Poggenpoel 1971, H. J. Deacon 1972). Very little evidence has come from sites at the coast or in the coastal region, and this has provided one of the major arguments in support of the seasonal mobility hypothesis (Parkington 1972).

Liengme (1987, table 5) listed 25 plant species found in the deposits in Elands Bay Cave. Of these, 11 are edible, or could be, depending on their species, which is generally not given. Information as to the stratigraphic location of these residues was also not given, but they are presumed to date from after the second hiatus, or after about 3800 B.P. (Parkington 1987: 7).

There is no record of seaweeds (Algae) having been eaten by the Khoisan. The giant kelp *Ecklonia maxima* (Alariaceae) is recorded as having been used for the storage of train-oil (Moodie *ed.* 1960: 46). The ≠Aonin (see Ch. 10 below) used it similarly, also using it and another species, *Laminaria schintzei*, in the treatment of wounds (Budack 1977: 27, 37). The red alga *Suhria vittata* (Rhodophyta) grows on the stipes of *E. maxima* (Branch & Branch 1981: 67) and may thus have been brought in with the latter. Watt & Breyer-Brandwijk (1962: 1077-1078) reported that red seaweeds are sources of agar, and cited Pappe as having observed that

S. vittata when boiled has been used with advantage in the treatment of a number of complaints, but that its nutritive value is negligible.

The root of *Veltheimia glauca* (= *V. capensis*, Liliaceae), of which the Khoikhoi name *quaroube* was recorded in 1685, was used by them as a purgative (Smith 1966: 317, under kwarobe; 380, under quarobe; Wilson in prep.). Parkington & Poggenpoel (1971: 19) reported that, in the excavations at De Hangen, several valves of the black mussel *Choromytilus meridionalis* were found wrapped in the outer casing of a plant tentatively identified as being of this genus.

Boophane disticha (Amaryllidaceae), of which the Afrikaans common name is gifbol, poison bulb, (Smith 1966: 228) is toxic. In 1774 Thunberg (Forbes *ed.* 1986: 294) recorded its use by the 'Hottentots', 'chiefly for poisoning the arrows with which they shoot the smaller kind of game, such as springboks'.

Liengme listed five genera/species of Iridaceae: *Moraea*/ *M. fugax*, *Babiana*, *Gladiolus*, *Antholyza plicata* and *Watsonia*. Of these genera, only *Antholyza* has not been recorded as being edible, although not all species of the other genera are necessarily so, particularly those of *Moraea* (Watt & Breyer-Brandwijk 1962: 505-513; Fox & Norwood Young 1982: 240-243).

Thunberg wrote of the waxberry *Myrica cordifolia* (Myricaceae; not *cordifolium*, as given by Liengme) that the Khoikhoi ate the waxy substance derived from boiling the berries 'like so much cheese' (Forbes *ed.* 1986: 134).

Aizoon (Aizoaceae) is not recorded as having been eaten by the Khoisan. Fox & Norwood Young (1982: 68) recorded the use of *A. canariense* by the Zulu as a kind of spinach.

Of the three Mesembryanthemaceae listed by Liengme, *Carpobrotus*/ *C. edulis*, *Malephora* and *Ruschia*/*R. maxima*, only *C. edulis*, the so-called sour fig, is known to be edible by humans (Fox & Norwood Young 1982: 267).

The skilpadbessie (tortoise berry) or duinebessie (dune berry) *Nylandtia spinosa* (Polygalaceae) is edible (personal observation; Smith 1966: 419), and was said to have been eaten by the 'Hottentots' (Pappe 1857, cited by Fox & Norwood Young 1982: 303). It was called *cargoe* by the Khoikhoi of the western Cape (Smith 1966: 189; Wilson in prep.).

The fruit of various species of *Rhus* (Anacardiaceae), *Euclea* (Ebenaceae) and *Olea* (Oleaceae) are said to be edible (Fox & Norwood Young 1982: 75-79, 185-187, 282-3).

There is no information as to the edibility of *Cassine parvifolia* (Celastraceae; not *parviflora*, as given by Liengme).

Species of *Helichrysum* (Asteraceae) are recorded as being eaten, or an infusion drunk (Fox & Norwood Young 1982: 123).

The fruit of bietou or bosluisbessie (tick-berry) *Chrysanthemoides monilifera* (Asteraceae) is edible (personal observation; Fox & Norwood Young 1982: 128).

The only plant remains recorded from the deposits in Die Kelders Cave were small patches of the estuarine eel-grass *Zostera capensis* (Zosteraceae) (Schweitzer 1979: 206) which is not known to be edible.

Plant remains were recovered from throughout the deposit at Byneskranskop 1, except in the basal layer (Schweitzer & Wilson 1982, table 23). Possibly edible species include *Hexaglottis longifolia*, *Moraea* spp. and other Iridaceae that were not identified, while species known to be edible are Cape sumach or Hottentotskersie (Hottentots' cherry) *Colpoon compressum* (Santalaceae), *N. spinosa*, *Rhus glauca*, white milkwood *Sideroxylon inerme* (Sapotaceae), *Euclea* cf. *racemosa* and *C. monilifera*. This is much the same range of species as that recorded from Elands Bay Cave. However, although the bulk of the sample from Byneskranskop 1 was not analysed, the total amount recovered from the excavation was small relative to the amount of faunal and artefactual material (personal observation).

Inskeep (1987: 210-212, table 55) recorded the presence of only four edible plant species in the Holocene levels of Nelson Bay Cave on the southern Cape coast: *S. inerme*, *Diospyros* sp. (Ebenaceae), *E. racemosa*, *Watsonia* sp. and what are now regarded as subspecies of *Olea europaea: O. africana* and *O. capensis* (Oleaceae). The seeds represent only a proportion of the 114 that were recovered; and only 18 *Watsonia* corm bases were found. I have been unable to find any reference to the use of the fruit of *Olea* spp. as a foodstuff by the Khoisan, although Fox & Norwood Young (1982: 282-283) recorded its use by various black peoples from Zululand to Malawi. The Nelson Bay Cave evidence also suggests that there was not a great reliance on plant foods, although in all the cases cited preservation factors should not be overlooked. Inskeep's table 15 gives the seasonal availability of these plants, which covers all the months of the year according to species. As mentioned previously, Inskeep (1987: 293) considered that Nelson Bay was suitable for year-round occupation from at least 10000 B.P.

The most common plant remains in Inskeep's excavation were those of *Jatropha capensis* (Inskeep 1987, appendix 46). The part represented is not specified, but the number, 54 in two adjacent squares of Unit 131, with four more in other units, suggests that these were seeds. Watt & Breyer-Brandwijk (1962: 420-422) reported that the sap of *J. capensis* (Euphorbiaceae) contains hydrocyanic (prussic) acid and has been used as an internal remedy for tuberculosis and other respiratory ailments, also for the treatment of ringworm. However, the seeds of other *Jatropha* species, also poisonous if eaten raw or in quantity, after being lightly roasted and the pericarp removed, have been used as purgatives.

DISCUSSION

The sparse botanical evidence from these sites does little to counter the seasonal mobility hypothesis or to refute the claim that coastal peoples had a more or less exclusively marine diet, although, as mentioned above, protein-rich marine foods are not an appropriate dietary substitute for the vitamins, carbohydrates and roughage obtainable from plant foods. It seems that more research needs to be carried out with regard to the range and seasonal availability of the numerous edible plant species within the coastal region, including those that leave no residues. The problem here is that no matter what might be *assumed* to have been eaten, or for which there is ethnographic evidence, unless the remains of such plants are found in archaeological deposits the case for such use cannot be convincingly argued. For example, Grevenbroek (1695 *in* Schapera *ed.* 1933: 188) observed that it was the men's duty to prepare the winter's supply of food,

> 'to wit wild almonds [*Brabejum stellatifolium* (Proteaceae)] . . . roots of the larger arum [*Zantedeschia aethiopica* (Araceae)] . . . and various bulbs. [They], in their spare time, and as a hobby, expose these nuts and roots to the sun, and roast them with a little fire, and when they have thus become fit to stand the passage of time they bestow them in ditches and caves, as if in a storehouse' (my interpolations).

As far as I am aware, there is no evidence for the use of storage pits anywhere in the southern to western Cape coastal region, other than a number of possible ones found during recent excavations in a shelter in the Great Brak River valley in the southern Cape, some 4 km from the coast (W.J.J. van Rijssen 1988 pers. comm.; personal observation). It seems, though, that it would require more than a 'spare-time' occupation or 'hobby' to collect, prepare and store a quantity of these foodstuffs sufficient to last even a small, extended-family group for the four or five months of the Cape winter.

There is also the problem of how many plant species are capable of being stored. The fruit of *Carpobrotus edulis* can be dried and it is probable that the corms of the Iridaceae could be stored, as suggested by Grevenbroek's observation, but information is not available as to their palatability in such a condition. Several of the pits in the Great Brak River Valley shelter excavated by Van Rijssen appear to have been lined with the outer casing of *Boophane disticha* and contained bundles of the leaves of Iridaceae species as well as corm bases. Most of the trees and shrubs mentioned above bear fruit in the spring or summer, and it is doubtful that their berries could be stored for any length of time.

Archaeobotany is another field in which much work still needs to be done. In this regard, techniques for the recovery of microbotanical remains need to be applied more consistently than is the case at present. Research in this field should also include the study of plants that were recorded as having other than dietary uses.

CHAPTER TEN
ETHNOGRAPHY

THE ≠AONIN

In the context of the debate on whether people did, or could, live more or less exclusively on marine foods, it is perhaps pertinent to provide information on recent coastal-dwelling people in southern Africa. Budack (1977) studied the ≠Aonin or 'Topnaar', a small Nama Khoikhoi tribe living in Namibia in the vicinity of Walvis Bay. Among them also lived a few Dama, people originally of Negro stock who have integrated with the tribe physically as well as socially (Budack 1977: 2).

The ≠Aonin formerly consisted of two sections, the !Khuisenin, '!Khuiseb people', who lived further inland along the !Khuiseb River and only came to the coast seasonally, and the Hurìnin, 'sea people', who lived near the coast (Budack 1977: 12). The !Khuisenin had large and small livestock, and when they came to the coast did not fish, but concentrated on harvesting the *!nara* melons *Acanthosicyos horrida* (Cucurbitaceae) that grow wild among the dunes. The flesh of these can be eaten while fresh (November-April) and the seeds dried and stored for later use. The Hurìnin had no livestock and harvested not only the *!nara* but also the produce of the sea (Budack 1977: 7, 13-14).

The Hurìnin fished by means of traps made of baskets or weirs woven of rushes, by spearing, or by catching with the bare hands. The flesh of stranded whales and dolphins was also much appreciated, as were fish washed up after being killed by 'red tide'. However, Budack (1977: 37) reported that none of his ≠Aonin informants had ever observed or heard of any cases of such poisoning as can result from a toxic 'red tide'; and a study of 21 dinoflagellates from the area revealed none that appeared to be toxic. Seals were clubbed, turtles, sea-birds and flamingoes caught and their eggs also eaten, as were various species of shellfish, notably limpets

Patella spp., brown mussels *Perna perna* and white mussels *Donax serra*. Only a few shellfish were eaten at the beach, the bulk being taken to the dwellings, which were inland: 'Settlements next to the beach were unknown' (Budack 1977: 14-36).

DISCUSSION

The evidence is that the Hurìnin, the ≠Aonin who lived near the coast, had an economy that was largely marine-based but also included an important terrestrial component, the *!nara*, which provided a year-round supply of vegetable food. Watt & Breyer-Brandwijk (1962: 345) reported that *A. horrida* is rich in protein, the 'cake' yielding 61,3 per cent, while the seeds yielded 44,28-46,30 per cent of a light oil. Dentlinger (1977, unnumbered and unpaginated table) provided the following information

	Protein	fat	carbohydrate
flesh (fresh)	1,4%	1,9%	11,1%
flesh (cake)	11,2%	15,4%	2,3%
seeds	30,7%	57,0%	34,0%

the cake being the boiled and sun-dried flesh, from which much of the fat was allowed to drain off (Dentlinger 1977: 28). Fox & Norwood Young (1982: 165) stated that the seeds, which were dried and used for winter consumption, tend to become rancid after a time. However, the cake, rolled or cut into strips, lasts for years.

There is a great difference in the protein content of the cake given by the two sources, which makes it difficult to evaluate the role of the *!nara* in the diet of the Hurìnin. If Dentlinger, rather than Watt & Breyer-Brandwijk, provided the correct analysis, the amount of oil may have been adequate to compensate for the ingestion of plant and animal protein. It may also be that the relatively high oil or fat content of the seeds moderated the effects of the protein ingested in the pulp and the marine foods. However, oil and fat derived from fish and marine mammals were also important items in the diet of the Hur̃nin. Budack (1977: 20-21) reported that fish oil was extracted from the heads of 'salmon' (species not given: ?*Atractoscion aequidens*, the 'Cape salmon' (Van der Elst 1988: 290)) by boiling, but the mackerel *Scomber japonicus* was called *tana-tsũb* or 'headache', because it is very fat and it was believed that eating too much of it could cause a headache. Oil rendered from the blubber of whales 'was chiefly used for food', mixed with ground dried fish; and the fat of seals was also eaten (Budack 1977: 26,29).

In the light of the current debate regarding the effects of excessive protein intake, it is unfortunate that a more scientific study of the diet of the Hur̃nin was not undertaken. Appended to the information regarding the preparation and use of the sun-dried flesh of whales and dolphins is the comment that 'In earlier times it was also crushed in a mortar and mixed with vegetables' (Budack 1977: 26). Dentlinger (1977: 29-30), who studied the !Khuisenin of the interior, provided evidence that these people ate maize meal and bread as well as, occasionally, other wild plant foods. This suggests that the *!nara* may not have been the only plant food eaten by the Hur̃nin, but information in this regard was not provided.

Whatever the case, the Hur̃nin appear to provide evidence that it is possible to survive on a high-protein diet along with, apparently, a relatively high intake of animal and vegetable fats or oils, although a complete analysis of their total diet would be necessary to confirm this. It seems, though, that because the Hur̃nin lived permanently on the coast, and had probably done so for a considerable period, their bodies were adapted to the efficient metabolism of the foods they ate.

Budack (1977: 2) mentioned that other Khoikhoi tribes still refer to the ≠Aonin as !Naranin, '*!nara* people' and commented 'The term !Naranin has a slightly derogatory connotation. Other [Khoikhoi] look down on the ≠Aonin, because the latter, like the Bushmen, are dependent on *veldkos* [wild plant foods] for a considerable part of the year' (my interpolations). This is a rather surprising, and perhaps somewhat recent, attitude since there is abundant evidence in the early records that the Khoikhoi herders also ate wild plant foods (e.g., Kolb 1738: 162, 204, 207, 209; Moodie *ed.* 1960: 396, 404 etc., Raven-Hart 1967: 3 (see Ch. 1 above), 33, 100 128-129, 180 etc; Thunberg *in* Forbes *ed.* 1986 *passim*). On the other hand, Dentlinger (1977: 31) commented that 'Being dependent on nara implies being poor, and being poor implies having no cash to buy more desirable commodities . . . and dependence on nara has become an indication of low social status'.

CHAPTER ELEVEN

SUMMARY AND CONCLUSIONS

The aim of this study was to examine the evidence relating to the 'Strandloper concept': that there were people who lived on the southern African coastline by beachcombing and who were of a different 'race' from those also recorded as having lived there. Given the amount of information available, in both the early records and the anthropological literature of the past century, I was obliged to limit my survey to what was necessary to provide basic information or to elaborate specific points. The information provided by the early travellers and settlers was dealt with first because it antedated the development of anthropology in the subcontinent and thus set the scene for subsequent physical anthropological and archaeological interpretations.

The first indigenous inhabitants seen by the voyagers from Europe, at Mossel Bay in 1488, were clearly herders, but the identity of the second group, seen at St Helena Bay in 1497 is not clear (Ch. 1). On the basis of the information provided, some of which was evidently added later, they appear to have been hunter-gatherers: there was no evidence of domesticated animals, but the men were away from their home base, so that the absence of livestock cannot be accorded undue significance. That they wore copper beads in their ears is made more problematic in the light of the apparent lack of possession of metal by the herders in the Table Bay area until they started bartering their sheep and cattle for it, and by the later discovery by the European settlers that the territory of the Little Namaqua, to the north of most points of early contact, was evidently the source of all the local supply of copper. The men of St Helena Bay may thus have been Namaqua who left their beasts at their kraal, or they may have been hunter-gatherers who had, by whatever means, acquired copper ornaments from the Little Namaqua. The limited dietary evidence is that these people subsisted on both terrestrial and marine resources.

For more than a century after Da Gama's visit to St Helena Bay the descriptions of the indigenous people of the coastal region were exclusively of people who can be identified as herders; but from early in the seventeenth century the presence was recorded in the Table Bay area of a small group of people who had no domestic animals of their own and were said to subsist on what they could find along the shore, together with the roots of certain plants (Ch. 2). These were the people who became known to the first European settlers as 'Strandlopers', 'Watermen' and 'Vismans' or 'Fishermen', and whose Khoikhoi name was Goringhaicona. It is not clear whether these people were outcasts from Khoikhoi tribes or whether they were former hunter-gatherers who had established some sort of clientary relationship with the Goringhaiqua, who occupied the Cape Peninsula, or a mixture of both, but their name, meaning 'children of the Goringhaicona', clearly implies some sort of subordinate status. I pointed out in Chapter 3 that the observations on the 'Strandlopers' before 1652 were only partial, being based on what was seen when ships called at Table Bay; and I have suggested that the way of life of this group may have been a response to the perceived advantages of being the first to make contact with the voyagers and by acting as intermediaries in the bartering process, both then and after the establishment of the white settlement. Their way of life, as described, was typical of neither hunter-gatherers nor herders, and the later documentary information is that they were either absorbed into the service of the settlers or moved away from the area; certainly, by the early eighteenth century, when the Khoikhoi way of life was shattered by the plagues that affected them and their livestock, the 'Strandlopers' had ceased to exist as a visible entity. The only other records of 'Strandlopers', at Saldanha Bay during the latter part of the seventeenth century, are sparse and inconclusive except insofar as these men were recorded as mostly not having possessed domestic animals, although there were herders in the area.

The observations of Gordon and Paterson in the north-western Cape coastal region during the late eighteenth century (Ch. 4), although also only partial, provide evidence of people living there who subsisted on marine and terrestrial foods, but it is not clear whether they were permanently or only seasonally resident at the coast.

In Chapter 5 I suggested that the 'Strandloper concept' was probably revived by the publication of the first part of Moodie's *The Record*, at a time when people were beginning to interest themselves in the anthropology and prehistory of the area; and that, from this source, the name 'Strandloper' found its way into the registers of the South African Museum, whence it was adopted by Shrubsall in his studies of South African crania and Péringuey in the earliest local study of the South African Stone Age. I showed that, statistically at least, there is no virtually no difference between the male 'Strandloper' and 'Bushman' crania that Shrubsall used in his study, an observation supported by Shrubsall's own statistics. Because all the 'Strandloper' crania came from coastal contexts, I presume that this was Shrubsall's reason for assigning them to this category, but there is a lack of information regarding the provenance of the 'Bushman' and 'Hottentot' crania, which may well have been assigned to their categories on the basis of previous metrical studies, rather than on the basis of their having been individuals who had been known in their lifetimes. Shrubsall's summary statistics indicate that the range of the individual measurements for each group overlaps that of each of the other two groups and, if nothing else, provide support for Schultze's (1928) conclusion that the 'Hottentots' and 'Bushmen' form part of a group that is distinct from all the other peoples of Africa. I also showed that, on the basis of Keen's criteria for the identification of 'Hottentot' and 'Bushman' skulls using their 'typical' cranial indices, the 'Strandloper' males in Shrubsall's sample could be classified as 'Hottentot', 'Bushman' and, mostly, 'Bush-Hottentot' hybrids, while the females were 'Bushman' or also mostly hybrids.

My investigation of the stature of prehistoric peoples of the South African coastal region (Ch. 5) was limited by the small number of femora of dated skeletons that was available. Attempts at interpretation are also somewhat biased by the fact that the greatest number of skeletons is dated to the period 2000-3000 B.P., and it would thus be unwise to attach too much significance to the finding that the mean stature of the skeletons, both male and female, from this period is lower than that of the skeletons from either the preceding or succeeding periods, particularly when it has been shown that the difference is statistically significant only in the case of the males of this period and those of the post-2000 B.P. period, which possibly include some Khoikhoi. The study also showed that there is no correlation between stature and geographic latitude, as Tobias found in the case of the Kalahari San. Where inferred diet, as indicated by the δ^{13}C isotope values, is concerned, there is also little correlation with stature, although there appears to be a stronger, inverse, correlation between isotope value and stature among the female skeletons dated to before 2000 B.P. It must be stressed, however, that because of the small size of the skeletal samples, the results of all the statistical tests may be spurious, but it would need an experienced statistician to evaluate this.

In Chapter 6, information regarding cultural aspects of coastal populations, as indicated by artefact assemblages, was surveyed. I consider that there is still too little information regarding the nature and causes of the variability, both spatial and temporal, that can be observed in these assemblages, and that more work needs to be done, particularly with regard to those of the 'post-Wilton' of about the last 3 500 years, which appear to resemble those of the pre-Wilton 'Oakhurst/Albany' industries. There is no evidence to support the existence of a population in the coastal region that was technologically distinct from any other contemporary population in the region, even after the advent of pastoralism some 2 000 years ago, although evidence for the technology of the pastoralists, other than their pottery, is, for all practical purposes, nonexistent and must await publication of information from the Kasteelberg sites and others that may yet be discovered.

Since the major basis for the identification of the 'Strandlopers' in the early records, other than their not possessing domestic stock, was their supposedly miserable subsistence mode, it was appropriate to consider the archaeological evidence for subsistence, which was done to a limited extent in Chapter 7. To have examined all the published evidence would have been impossible in the present circumstances, but the overview showed that, almost without exception, sites in the coastal region, whether in caves or in the open, that contain food debris reveal that terrestrial foods were eaten as well as marine ones. The evidence for this dates from the Upper Pleistocene, but is sparse until the late Pleistocene, and is best in the Holocene. All sites in the coastal region that date to before 2000 B.P. are to be assigned to hunter-gatherers, but after that date there is evidence, in the western part, for the presence of pastoralists and, in the eastern part, for farmers. The evidence for pastoralist occupation is at present restricted to the Kasteelberg sites, full information from which is still to be published; but I do not doubt that many of the other open sites, including shell middens, should also be assigned to them, although the problem will be to identify them. Where the Iron Age farmers are concerned, the scanty evidence for the Early Iron Age suggests that these people were agriculturists who obtained their animal protein from shellfish as well as from terrestrial mammals; but in the Late Iron Age pastoralism was added to their subsistence base.

Attempts at dietary reconstruction, or even at assessing the proportionate contribution of marine and terrestrial foods, are hampered by the practice of retaining all bone but of only sampling marine shell, so that this important component is under-represented and the contribution of shellfish to the diet must be underestimated. The isotopic analyses of human bone by Sealy & Van der Merwe, discussed in Chapter 8, appear to provide evidence that the people on whose skeletons the analyses were carried out had diets that included marine foods in varying amounts, and that this dietary component was more common in the period 3000-2000 B.P. than in the preceding or succeeding periods. What is in question, however, is the accuracy of the method, and whether it is capable of showing the contribution of other dietary components besides protein. That people could live more or less exclusively on a high-protein diet derived from marine foods was questioned on the basis of evidence that excessive ingestion of protein can cause illness and even death within a short period.

To my mind, the isotope analyses do not effectively overturn the seasonal mobility hypothesis, which is itself in need of re-evaluation, since the evidence for this is based on positive seasonal identifiers and does not take into account the contemporaneous presence of non-seasonal components. That the analyses appear to show that some people at some times had a higher intake of marine foods than other people at other times is merely a reflection of the evidence provided by the archaeological fauna. They show, too, what the archaeological fauna also shows: that people, in general, did not live solely by 'strandloping', but rather that they exploited the resources of the *coastal region*, both terrestrial and marine. In this regard, future re-evaluation of the seasonal mobility hypothesis should allow for the testing of the hypothesis that territories differed according to the environment of the area and the resources it provided. The model of movement between the coast and the mountains is too simplistic and should encompass more complex and variable patterns of movement, both in time and space. Because of the general homogeneity of artefact types in the coastal region (taking this to include all that part of the country between the Central Plateau and the sea), I am not optimistic that we will be able to define with any reasonable certainty the territories of individual groups. If this is the case, we will also not be able to reconstruct prehistoric diet to the extent that we would wish - certainly not to the extent attempted by Buchanan (1988), which is discussed in the Appendix.

Plant foods are an appropriate dietary resource to mitigate the effects of a high protein intake, but the archaeological evidence, discussed in Chapter 9, is sparse, leaving the contribution of plant foods to prehistoric diet to be inferred rather than demonstrated. Modern ethnographic evidence, in the form of Budack's study of the Hurinin of Namibia (Ch. 10), is that this group was accustomed to live on a diet that consisted almost entirely of a variety of marine resources together with a single plant food species, the *!nara* melon. Although Budack did not carry out a comprehensive analysis of the contribution of the various dietary components, his information provides evidence that people can subsist on a largely marine diet as long as there is also input from plant foods; and it is probable that this was also possible in the past.

To conclude, the evidence of both the early records and archaeology is that the diet of people in the coastal region - hunter-gatherers, pastoralists and farmers - included terrestrial as well as marine foods, so that their subsistence mode cannot be described as 'strandloping', or the eking out of their existence by beachcombing. That this *appeared* to be the way of life of the Goringhaicona, the 'Strandlopers' of the seventeenth century, is a mistaken view based on partial evidence and coloured by the cultural prejudices of the observers.

There is also no evidence to support suggestions that there were people in the western half of the coastal region who were biologically or culturally different from the Khoisan, making due allowance for the fact that there may be evidence for spatial and temporal biological variation and that aspects of the culture of the Khoikhoi pastoralists differed from that of the San hunter-gatherers, also that the 'culture' of the Goringhaicona was typical of neither group. There is no evidence for the presence of the Khoikhoi in the eastern half of the coastal region, which was occupied initially by hunter-gatherers and later by black (Negro) farmers, who differed from the Khoisan both biologically and culturally.

There is therefore no justification for the general use of the name ‘Strandloper’, which incorrectly reflects the various ways of life of the peoples of the coastal region; and even its use as an informal sobriquet for the Goringhaicona should be accompanied by explanation that they were not mere beachcombers, but people who had adapted their life-style to accommodate their interaction between the European voyagers and settlers and the Khoikhoi herders.

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APPENDIX

AN EVALUATION OF THE RECONSTRUCTION OF PREHISTORIC COASTAL HUMAN DIET BY BUCHANAN (1988)

Buchanan (1988) reconstructed the diet of the people of the south-western Cape coastal region during 1 500 years of the present era, with specific reference to the Elands Bay area. He concluded (p. 95) that they spent only between ten days and a month at the coast each year. Since this conclusion has important implications for the ongoing debate as to whether people visited the coast only seasonally or whether they were permanently resident there, it is appropriate to evaluate Buchanan's reconstruction to see whether it supports his conclusion. Only the Elands Bay area is considered here, since it was for this that Buchanan provided most information; and it may be assumed that since common methods were used the results for other parts of the region will be similar to those for the Elands Bay area. To facilitate checking, the information given by Buchanan is dealt with seriatim as far as is possible.

Noli (1986) applied aspects of Buchanan's (1985) dietary reconstruction to the material from his re-excavation of the Hailstone midden at Elands Bay. He provided an extensive analysis of Buchanan's reconstruction, which was the basis for the publication discussed here and commented on a number of problems he encountered. Noli's approach differed somewhat from that adopted here, and he devoted more attention to the energy (kilojoule) content of Buchanan's reconstruction and the problems of protein poisoning (see also Noli & Avery 1988) than I have in Chapter 8 of my study. My evaluation is essentially a mathematical analysis of the major components of Buchanan's reconstruction, and I have here not considered the dietary implications in great detail.

PERIOD STUDIED

Buchanan (p. 10) chose for his study 'a focus on the period from 1800 to 300 BP characterised by the presence of pottery'. This was, to my mind, an unfortunate period to choose, since it allows for the possibility that the people whose diet Buchanan reconstructed could have been: (1) hunter-gatherers whose previous life-style had been disrupted by the incursion of pastoralists into their territory; (2) early pastoralists who had sheep but no cattle; (3) later, after about 1400 B.P., herders who had cattle and sheep; or a combination of hunter-gatherers and sheep-herders (1 + 2) in the early period and hunter-gatherers and sheep- and cattle-herders (1 + 3) in the later period. Buchanan seems, however, to have ignored the possibility that the pastoralists may have been responsible for at least some of the sites he included in his reconstruction and to have assumed (pp. 20, 72, 94) that the sites were the result only of the activities of hunter-gatherers.

SITES SAMPLED

Buchanan stated (p. 22) that he had obtained a total of 54 samples from 32 sites, and commented (p. 23) that multiple samples taken from different areas of the same site 'indicate a degree of homogeneity'. His table 3.2 (p. 118) provided percentage frequencies of limpets, mussels and whelks for 28 samples taken from 13 sites, including frequencies for three samples from each of two sites.

Inspection of these frequencies suggested that in some cases the differences between the samples were sufficiently great as to be statistically significant. All the samples in Buchanan's table were therefore subjected to the x^2 test for two independent samples (Siegel 1956: 104-111). Since this test is based on percentages (the sum of each row and of each column must be 100%) it was considered appropriate to use the percentages given by Buchanan. The results of these tests are given in Table A, from which it will be seen that, of the 17 pairs tested, 7 (41%) are

TABLE A. RESULTS OF CHI-SQUARED TESTS ON PAIRED SAMPLES FROM SITES LISTED IN BUCHANAN'S TABLE 3.2.

Site	sample	x^2	p	sig.
VV1		2,03	0,30	ns
VV4		8,41	0,02	s
SC1	1:2	0,85	0,95	ns
	1:3	14,05	0,001	vhs
	2:3	10,00	0,01	hs
DD1		0,03	0,98	ns
DD2	1:2	4,86	0,70	ns
	1:3	0,99	0,50	ns
	2:3	2,16	0,30	ns
BK4		10,57	0,01	hs
DP1		1,58	0,30	ns
BR6		0,14	0,90	ns
BR8		9,13	0,02	s
JT1		2,18	0,30	ns
JT2		13,97	0,001	vhs
EC1		15,03	0,001	vhs
VN1/2		1,39	0,50	ns

Notes. p = probability; sig. = significance: ns = not significant, s = significant, hs = highly significant, vhs = very highly significant.

statistically significantly different from each other. The indication is thus that the degree of homogeneity of the samples is lower than Buchanan considered. The evidence of heterogeneity in two of the pairs of samples from site SC1 and the lack of it in the three pairs from site DD2 shows that, overall, the content of the sites is extremely variable and that taking averages or means masks the realities of the situation. These factors make questionable the calculations Buchanan used in his dietary reconstruction.

Sampling such as carried out by Buchanan serves, at best, only to *characterize* a site, not to provide an accurate quantification of its contents. If this imprecise sampling, used as a basis for quantifying the dietary components of the site, is then multiplied by the equally imprecisely estimated volume of the deposit (p. 25: see below), the error is compounded. Moreover, reference to any excavation report will show that the distribution of components throughout the deposit, both horizontally and vertically, is *never* equal, so that homogenizing the distribution distorts the realities of the situation.

As a general comment, applicable to the foregoing as well as to the components discussed below, it must be pointed out that a mean is derived from a sample of a specific size with a specific distribution over a specific range. It is therefore incorrect to apply a mean derived from one sample to other samples in which any or all of the factors - number, range and distribution - differ. It is also incorrect to assume that a mean derived from combining a number of samples will be applicable to all samples drawn from a total population, for example, of a particular species of shellfish, or of terrestrial mammals.

SITE VOLUMES

Volumes of deposit in the sites were estimated by measuring the surface area 'by triangulation and other simple techniques' (p. 25), while the mean depth was estimated by 'a series of probes to bedrock at one metre intervals both across and into the cave' - and, presumably, open sites as well, since these were a component of Buchanan's dietary reconstruction (fig. 1.3). Since both the area and the depth of deposit in a site are irregular, and only the *mean* depth was calculated, these estimates of volume are imprecise, with the degree of imprecision probably varying from site to site. As mentioned above in connection with the calculation of the shellfish proportions in the sites, the error is compounded by the use of imprecisely measured volumes. Buchanan asserted (p. 27) that 'The spread of volume estimates over many sites and the general shallowness of deposits should tend to restrict the scope for error and give grounds for assuming that the collated data provide an adequate basis for analysis, inference and interpretation'. In my opinion, the contrary is true: since the *estimated* volume of each site already contains an inherent error factor, multiplying this factor (different for each site) by the number of sites included in the study *increases* the overall error, rather than reducing it. As Buchanan (p. 28) commented, 'Any error in the initial quantification is likely to be progressively magnified through subsequent calculations'. In his Appendix A (pp. 227-229) Buchanan gave the total (estimated) volume of the deposits of the sites in the Elands Bay area as 2 823 m^3, but different volumes were used in the calculations for the various components of his dietary reconstruction. This matter is discussed further below.

SHELLFISH

Discussing the effects of sieve mesh size on shell counts, Buchanan (p. 24) stated that 'All samples were sieved through a 3 mm mesh prior to sorting; but shellfish data from excavated sites reported occasional sieving through 12 mm mesh only when some hinges and apices would have been lost through the larger mesh'. Since a 12 mm mesh is larger than a 3 mm mesh, Buchanan appears to have transposed the mesh sizes, and the samples were probably sieved through a 12 mm mesh, with the portion that remained on the 3 mm mesh sieve perhaps being checked only occasionally.

In order to test the loss through using only a 12 mm mesh sieve, Buchanan (table 3.3, p. 119) passed four samples of *Patella* spp. through both mesh sizes and found that 3,1-6,9 per cent of the shells passed through the larger mesh. *Patella* shells are more robust than those of *Choromytilus meridionalis*, for example, and Buchanan's percentages may thus be a reasonably accurate reflection of reality, though this probably varies from site to site, depending on factors such as age, degree of exposure, post-depositional actions (e.g., parking of beach buggies) and so forth.

In the case of the three samples of *C. meridionalis* for which Buchanan gave percentages (in the other three samples n = <20 and percentages were not given), 22,0-24,8 per cent of the hinges passed through the 12 mm mesh. This seemed considerably lower than had been my experience, so I counted samples of *C. meridionalis* from one of the layers of my excavation at Hout Bay, and of *Perna perna* from a layer in my Koppie Alleen excavation (Wilson in prep.). In the case of the Hout Bay sample (n = 4 701), 92,9 per cent of the hinges passed through the 12 mm sieve and 84,1 per cent in the case of the Koppie Alleen sample (n = 2 149). These results suggest that Buchanan's samples were from sites with remarkably little breakage, and that they are atypical of most sites containing mussels. G. Avery (1990 pers. comm.) has confirmed that in his experience the bulk of mussel hinges are to be found on the 3 mm mesh sieve. The results thus suggest that mussels are likely to be under-represented in Buchanan's dietary reconstruction. Buchanan (p. 23) stated that since he had allocated similar kilojoule values (24 for limpets and 28 for mussels), 'errors in the limpet/ mussel ratios and in energy calculations would be of limited effect'. However, if mussels are as severely under-represented as the indications from the Hout Bay and Koppie Alleen samples suggest, the effect of this would be considerable.

Buchanan (Appendix B, pp. 230-232) compiled a table 'To enable calculation of energy yields from limpet shell remains . . .. Average shell weights were later added, based on weighing individual shells' (p. 30). In his table 4.3.1. (pp. 124-125) he detailed the methods of sample analysis and determination of total kilojoules for each sample. The first stage, after sorting the identifiable shell into species, was to 'record the maximum width across the base of each shell and the overall weight of the shell' (p. 230, 1(b)(i)). For these measured shells, 'The kJ yield for each mm in size for each species is first determined (Appendix B)' (p. 230, 2 (a)).

The increments of kilojoules and shell weight per millimetre of shell width given in this appendix seemed too orderly to represent a real-life situation, so I weighed samples of unbroken *Patella granularis* and *P. argenvillei* from my excavation at Hout Bay. The shells were weighed on a Mettler P1200 electronic balance calibrated to 0,01 g. I chose the smallest and largest of the four species in Buchanan's table on the assumption that any differences observed in these two would be intermediate for the other two species, *P. granatina* and *P. barbara*. As I was not sure why Buchanan chose to measure shell width (generally accepted as the lesser plane dimension) when others (e.g., Branch 1974, Robertshaw 1977, Schweitzer & Wilson 1982) measured shell length, I measured the length and width of each shell that was weighed. The details for each species are given in Tables B and C, together with shell weights for the relevant shell width given by Buchanan in his appendix. The Hout Bay midden (HBM) shells were, like the shell from most archaeological sites, not washed, but I consider that the increased weight due to dirt adhering to the shell would, in part, compensate for loss of weight due to decalcification and dehydration, and that the resultant errors would probably only be of the order of one or two decimal places.

Before discussing the differences in weight between the HBM sample and Buchanan's data, it should be noted that, even when the plane dimensions are the same, there are differences in weight. For example, in the case of the five

TABLE 8. COMPARISON OF WIDTHS AND WEIGHTS OF PATELLA GRANULARIS GIVEN BY BUCHANAN (1988, APPENDIX B) WITH THOSE FROM A SAMPLE FROM HOUT BAY MIDDEN.

width (mm)	weight (g)		% diff	length (mm)	wt (g)	% diff
	Buch	HBM			Buch	
23	-	1,22	-	29	1,92	-36,5
25	1,20	1,59	+32,5	32	2,46	-35,4
25	1,20	2,80	+133,3	33	2,64	+6,1
25	1,20	2,13	+77,5	34	2,82	-24,5
27	1,56	2,54	+62,8	35	3,00	-15,3
28	1,74	3,70	+112,6	36	3,30	+12,1
28	1,74	3,37	+93,7	37	3,60	-6,4
30	2,10	3,33	+58,6	37	3,60	-7,5
30	2,10	3,56	+69,5	39	4,20	-15,2
30	2,10	4,30	+104,8	39	4,20	+2,4
30	2,10	2,75	+30,9	39	4,20	-36,5
30	2,10	2,61	+24,3	39	4,20	-37,9
30	2,10	2,67	+27,1	39	4,20	-36,4
31	2,28	2,50	+9,6	38	3,90	-35,1
31	2,28	3,46	+51,7	40	4,50	-23,1
32	2,46	3,38	+37,4	40	4,50	-24,9
33	2,64	3,84	+45,4	39	4,20	-14,7
33	2,64	3,56	+34,8	42	5,10	-30,2
34	2,82	4,62	+63,8	41	4,80	-3,8
34	2,82	3,48	+23,4	42	5,10	-31,8
34	2,82	3,51	+24,5	44	5,70	-38,4
35	3,00	4,37	+45,7	41	4,80	-9,0
35	3,00	4,35	+45,0	44	5,70	-23,7
35	3,00	5,67	+89,7	44	5,70	-0,2
35	3,00	4,34	+44,7	45	6,00	-27,7
35	3,00	4,76	+58,7	45	6,00	-20,7
36	3,30	5,84	+77,0	47	6,90	-15,4
37	3,60	5,84	+62,2	47	6,90	-15,4
37	3,60	5,08	+41,1	47	6,90	-26,4
39	4,20	8,28	+97,1	51	8,70	-4,8
40	4,50	6,68	+48,4	49	7,80	-14,4
40	4,50	6,01	+33,5	50	8,25	-27,2
40	4,50	7,15	+58,9	51	8,70	-17,8

Notes. The widths and lengths in each row are for each of the Hout Bay midden (HBM) shells measured, as is the weight in col. HBM. The weights in column Buch are those given by Buchanan for the respective shell width.

TABLE C. COMPARISON OF WIDTHS AND WEIGHTS OF PATELLA ARGENVILLEI GIVEN BY BUCHANAN (1988, APPENDIX B) WITH THOSE FROM A SAMPLE FROM HOUT BAY MIDDEN.

width (mm)	weight (g) Buch	HBM	% diff	length (mm)	wt (g) Buch	% diff
42	-	16,49	-	59	13,20	+24,9
45	5,00	14,81	+196,2	60	14,00	+5,8
54	9,50	24,51	+158,0	72	27,80	-11,8
54	9,50	27,02	+184,4	73	29,20	-7,5
54	9,50	32,57	+242,8	75	32,00	+1,8
55	10,00	23,56	+135,6	67	20,80	+13,3
57	11,60	27,40	+136,2	76	33,00	-17,0
57	11,60	33,35	+187,5	77	34,00	-1,9
57	11,60	29,79	+156,8	77	34,00	-18,4
57	11,60	36,53	+214,9	81	38,00	-3,9
58	12,40	38,06	+206,9	74	30,60	+24,4
58	12,40	43,18	+248,2	75	32,00	+34,9
58	12,40	33,72	+171,9	77	34,00	-0,8
58	12,40	31,10	+150,8	77	34,00	-8,5
58	12,40	38,39	+209,6	77	34,00	+12,9
59	13,20	28,34	+114,7	80	37,00	-23,4
60	14,00	39,53	+182,4	75	32,00	+23,5
60	14,00	32,53	+132,4	77	34,00	-3,3
60	14,00	55,26	+294,9	83	40,00	+38,1
64	17,20	54,16	+214,9	85	42,00	+28,9
71	26,40	87,00	+229,5	93	-	-

Notes. The widths and lengths in each row are for each of the Hout Bay midden (HBM) shells measured, as is the weight in column HBM. The weights in column Buch are those given by Buchanan for the respective shell width.

P. granularis measuring 30 x 39 mm, their weights are in the range 2,61-4,30 g (mean 3,18 ± 1,47 g). In the case of the three *P. argenvillei* measuring 58 x 77 mm, the range is 31,10-38,39 g (mean 34,40 ± 3,01 g). These differences are probably due to differences in height and shell thickness and indicate the probability of an error when a single weight is given for a size. In the case of the *P. granularis* sample the difference from the mean is large, from -18 per cent to +35 per cent. The difference is not as great in the *P. argenvillei* sample: from -10 per cent to +12 per cent.

Table B indicates that there are considerable differences between the weights given by Buchanan for specified *P. granularis* shell widths (col. 2) and those obtained from the HBM sample (col. 3), with differences in the range of +9,6 per cent to +133,3 per cent. To test the possibility that Buchanan's 'width' was, in fact, the greater dimension rather than the lesser, I used his weights per measurement (col. 6) for the lengths of the HBM sample (col. 3). In this case, the percentage differences are in the range of -7,9 to +12,1. The total weight of the 32 HBM shells (col. 3, excluding the 23 mm shell, for which Buchanan did not provide a weight), 132,07 g is 55,0 per cent higher than the total of 85,2 g obtainable from the weights given by Buchanan (col. 2). If the weight of the 33 HBM shells, 133,29 g is compared with the total of 164,49 g for what I have taken to be length (col. 6), the latter weight is 23,4 per cent higher than that of the HBM sample.

Where the *P. argenvillei* weights are concerned, the differences shown in Table C are even more striking: the HBM weights are in every instance 2-3 times greater than those given by Buchanan. This suggested very strongly that Buchanan's 'width' was the greater dimension (length), but application of his weights to the lengths of the HBM sample still showed a wide range of differences, from -23,4 per cent to +38,1 per cent. The weight of the 20 HBM shells (col. 3 excluding that of the 42 mm shell), 730,81 g, is 202,4 per cent greater than the total of 241,7 g for the weights given by Buchanan (col. 2), while the total of 625,6 g for what I have taken to be length (col. 6) is only 2,9 per cent less than the total for the HBM shell less the weight of the 71 x 93 mm shell.

The HBM samples thus provide no clear indication as to whether Buchanan's 'width' actually was the greater plane dimension. The mean width of the *P. granularis* sample is 32,3 mm and the mean length 39,7 mm, while the 'mean size' for this species given by Buchanan (table 4.3.1, p. 125) is 36,5 mm, which is intermediate between the two means given above. In the case of the *P. argenvillei* sample, the mean width of the HBM sample is 54,4 mm and the mean length 75,7 mm, the latter being closer to Buchanan's mean of 70,4 mm than that of the width. The lower percentage differences in the length:weight values for the two samples, compared with those for the width:weight values, tend to suggest that length was the dimension Buchanan measured, but in each case they show that there is a potential error that may be great according to the species involved.

While the evidence of two externally-obtained samples cannot be taken as conclusive proof that Buchanan's shell weights are incorrect, it shows that these weights are not applicable to all archaeological shell, and that his data should thus be treated with caution. The HBM data also show that there is not a clear correlation between shell length and shell weight, or even that larger shells are necessarily heavier than smaller ones. I do not have access to independent kilojoule data, but consider that the caveat applied to the shell weights also applies to the kilojoule values Buchanan gave in his appendix.

The next step in Buchanan's formula (table 4.3.1, 1(b) (ii)) is to obtain the MNI and overall shell weight for each species. In my experience, there is generally little difficulty in identifying the apices of broken shells of Buchanan's four main species, but it is difficult to identify *all* the fragments as being of a particular species - in the present case, chiefly the fragments of *P. granatina* and *P. barbara*, especially when the shell fragments are abraded.

The third step is to obtain the MNI of all apices that cannot be identified as to species and the overall weight of their fragments. Since the species cannot be identified, it seems unlikely that it will be possible to identify their fragments, unless by this Buchanan meant the residue that could not be assigned to any of the four main species, but which, in my view, is likely to include some of these.

The MNI of the unidentified species is then to be assigned to the four main species in the percentages listed below. These percentages 'have been derived from analyses of samples . . . from west coast middens'. Buchanan did not state from which, or how many, sites these percentages were derived, but they appear to be averages. Comparison with published frequencies for Tortoise Cave (Robey 1987, table 4) and Hailstone Midden (Noli 1988, table 3) suggests that Buchanan's allocations (Buch) are not generally applicable:

	Buch	TC	HSM
P. granatina	64,0%	72,5%	86,0%
P. granularis	30,0%	20,5%	12,8%
P. argenvillei	2,0%	1,8%	1,0%
P. barbara	1,8%	2,3%	0,2%
P. cochlear	-	2,8%	-

The next stage is the conversion of the shell weight to kilojoules by division of the total shell weight for the residues, identifiable as well as unidentifiable, by the MNI, which produces 'a mean weight per shell, which can be translated into a mean kilojoule yield per shell (Appendix C)' (*sic*: actually Appendix B, p. 125). However, since these residues, as well as the apices on which the MNI are based, must consist of shells of different sizes, and thus weights, the use of a *mean* weight is bound to add yet another error factor to the calculation.

As an example of the problems inherent in this method, I obtained the total weight of the 33 *P. granularis* shells from the Hout Bay midden for which details are given in Table B. I divided this total, 133,29 g, by the number of shells, arriving at a mean weight of 4,04 g. This is intermediate between the weights given in Buchanan's Appendix B for shells of 38 and 39 mm width, so I took a mean value of 15,55 kJ and multiplied it by the number of shells, which gave a total of 513,15 kJ. I then obtained the total of the kilojoule values for the shells on the basis of those given for the various shell widths in Buchanan's table. This total, 243,8 kJ, is less than half (47,5%) the total arrived at by the first method. The same method applied to the 21 *P. argenvillei* shells for which details are given in Table C showed that the second total is 44,4 per cent of the first. These results suggest that the method Buchanan recommended for obtaining the kilojoule values of broken shell has an inherent error factor that appears to be considerable. Added to the error factor in the derivation of kilojoule values from the widths of whole shells, this makes the whole method questionable.

Buchanan (p. 32 & Table 4.4.1 (p. 127)) provided data for calculation of the energy yield of *C. meridionalis* on the basis of information derived from Griffiths (1981: 104). It should be noted that in his table, Buchanan gave the composition of 'dry somatic flesh' as 82,12 per cent water and 16,88 per cent dry flesh, whereas in Griffiths's table 1 these are the values for *wet* somatic flesh. The 'parameters' derived from Griffiths's data appear to have no relevance to Buchanan's calculations, since the kilojoule values given in his Appendix C (pp. 233-234) are based on those for the *dry* flesh weight resulting from multiplying the product of Griffiths's formula for calculating dry flesh weight from shell length and multiplying this by 19,5 kJ/g (e.g., a shell of 20 mm length has a dry flesh weight of 0,035 g which, multiplied by 19,5 kJ, gives 0,68 kJ). I do not know how Buchanan arrived at his shell weight. It was not obtained by subtracting the dry flesh weight from the total dry weight (e.g., for 20 mm length, total dry weight 0,661 g minus dry flesh weight 0,035 g = 0,626 g, whereas Buchanan has shell weight as 0,50 g), which would have seemed the simplest method, particularly as the byssus, the only other component in Griffiths's calculations, accounts for only 0,17 per cent of the dry whole mussel, or 0,001 g in a shell of 20 mm length.

As was the case with Buchanan's table for the calculation of kilojoule yield and shell weight for limpets, the similar information he provided for *C. meridionalis* looked too orderly to represent a real-life situation, so I checked the shell weights given for shell lengths in his Appendix C against two samples of non-archaeological shell collected for other purposes. One sample was from the beach wash at the informally-named Shelly Cove near Sandy Bay on the Cape Peninsula, the other from the beach at Yzerfontein on the Cape west coast south of Saldanha Bay. The Shelly Cove sample consists of single valves that are variably water-worn, thus more like shell from middens than those in the Yzerfontein sample, which consisted of whole shells (both valves were joined, but have since been separated) that are relatively fresh and still have most of the periostracum present, which is not the case with the Shelly Cove valves.

The length of all the valves was measured, and they were weighed on the same Mettler P1200 electronic balance used for weighing the *Patella* shells. In the case of the Yzerfontein sample, one valve of each pair was weighed, then both. These measurements and weights are given in Table D together with weights for the appropriate valve length taken from Buchanan's Appendix C. In only one case, the 40 mm valves, did the weight of both valves equal exactly twice that of a single valve. In the other cases, the weight of both valves ranged from 0,10 g less than twice that of a single valve (84 mm) to 0,54 g more (80 mm). Although this latter difference is only 3,6 per cent of the total weight of the shell, it can be considered possibly important when valve weights are given to one-hundredths of a gram, as Buchanan did - an unnecessary refinement, in my view, since archaeological shell is not in a pristine condition.

TABLE D. COMPARISON OF SHELL LENGTHS AND WEIGHTS OF CHOROMYTILUS MERIDIONALIS AS GIVEN BY BUCHANAN (1988, APPENDIX C) AND SAMPLES FROM SHELLY COVE AND YZERFONTEIN.

Length (mm)	weight (g) Buch	SC	% diff	wt (g) Yfn:1	% diff	wt (g) Yfn:2	% diff
37	1,50			0,58	-61,3	1,17	-22,0
38	1,62			0,63	-61,1	1,27	-21,6
40	1,87			0,63	-66,9	1,26	-32,6
41	2,00	1,94	-3,0				
41	2,00	2,01	+0,5				
42	2,12	1,85	-12,7				
43	2,25			0,88	-60,9	1,82	-19,1
44	2,37	1,97	-16,9				
47	3,00	3,54	+18,0	1,13	-62,3	2,32	-22,7
54	4,75	2,07	-56,4				
54	4,75	2,31	-51,4				
56	5,30	4,45	-16,0	2,64	-50,2	5,36	+1,1
56	5,30	4,56	-14,0				
58	5,90	4,32	-24,8				
60	6,50			2,71	-58,3	5,37	-17,4
61	6,80	5,74	-15,6				
62	7,10	5,64	-20,6				
63	7,40			2,75	-62,8	5,68	-23,2
64	7,70	7,29	-5,3				
66	8,45			3,60	-57,4	7,22	-14,6
67	8,90	7,93	-9,9				
71	10,70	6,85	-36,0	5,16	-51,8	10,17	-5,0
78	14,75			6,52	-55,8	13,13	-11,0
79	15,50			7,83	-49,5	15,83	+2,1
80	16,25			7,27	-55,3	15,08	-7,2
84	19,25			7,56	-60,7	15,02	-22,0
90	23,50			8,97	-71,8	17,78	-24,3
101	33,90			12,70	-62,5	25,34	-25,3
103	36,20			15,57	-57,0	31,46	-17,1

Notes. Buch = Buchanan (1988, Appendix C); SC = Shelly Cove; Yfn:1 = Yzerfontein, 1 valve; Yfn:2 = Yzerfontein, both valves. % diff = the precentage difference between the weight given by Buchanan and that obtained from the shell(s) in the SC and Yfn samples.

It should be noted that an increase in length is not necessarily accompanied by an increase in weight, nor is the increase as constant as suggested by Buchanan's data. In the case of the Shelly Cove (SC) valves the 42 mm valve weighs less than either of the two 41 mm valves and the weight of the first of the two 56 mm valves is more than twice that of the first of the two 54 mm valves, although the difference in length is only 2 mm. In the case of the Yzerfontein sample, the length:weight increase is more regular, with only the 40 mm and 79 mm valves (single and double) being 'anomalous' in this regard.

In only two cases in the Shelly Cove sample does the weight exceed that given by Buchanan for the same shell length, while for the others the weight is 3,0-56,4 per cent less. In the case of the Yzerfontein sample, the weight of the single valves (Yfn:1) is 50,2-66,9 per cent less than the weight given by Buchanan for the same valve length. Where the weights of both valves (Yfn:2) are concerned, the differences range from +2,1 per cent to -32,5 per cent. Although MNI of bivalves are based on the greater number of either the left or right valve, I considered the possibility that the weights Buchanan gave were for both valves, i.e., the whole shell, since these are related to the determination of kilojoule values (see his table 4.4.1), but the discrepancies between his weights and those for the Yzerfontein pairs are mostly in the same range of magnitude as those for the Shelly Cove sample of single valves.

The total weight of the 15 Shelly Cove valves, 62,47 g, is only 79,4 per cent of the total of 78,69 g that is obtained by using Buchanan's weights for the same shell lengths. In the case of the Yzerfontein sample, the total weight of the single valves, 87,13 g, is only 41,9 per cent of the total of 207,94 g obtained using Buchanan's weights, while that for both valves, 175,28 g, is 84,3 per cent of the total obtained by Buchanan's method.

Even though the shells used in this test were not archaeologically-derived, the results indicate that the data in Buchanan's Appendix C are not as meaningful as they might appear to be, and that there is a greater degree of variation between shell length and shell weight than his figures indicate. They also indicate the probability that a number of calculation errors will occur even before the kilojoule values are calculated.

Buchanan's method for calculating the total kilojoules (table 4.4.2, p. 128) is the same as his first method for *Patella*, which was discussed above. I therefore tested it against the shells from Shelly Cove and Yzerfontein, for which lengths and weights are given in Table D. The mean weight of the 15 Shelly Cove valves, 4,16 g, is intermediate between the weights given by Buchanan for shell lengths of 51 and 52 mm, so I calculated the percentage difference for the kilojoule values of each of these and took an average of 8,32 kJ. Multiplied by the number of valves, this gives a total of 128,4 kJ. I then obtained the total kilojoules on the basis of the values given by Buchanan for shell length. The total, 157,24 kJ, is 26 per cent higher than that obtained by Buchanan's method. In the case of the 17 Yzerfontein shells, the total for the single valves, calculated by Buchanan's first method, is 170,00 kJ and that obtained by the second method described above is 338,54 kJ, 99,1 per cent higher than the total obtained by Buchanan's method. In the case of the pairs of valves, with the values halved, the second value is 97,9 per cent higher than the first.

The results of these tests contrast with those carried out on *Patella* shells in that the results of using Buchanan's method are higher for *Patella* but lower for *C. meridionalis*. In each case, however, they indicate that there is an error factor that may be considerable. Moreover, since shell from archaeological sites is not in its pristine condition, doubts may be expressed as to the validity of applying data obtained under strict laboratory conditions, such as those given by Griffiths for *C. meridionalis*.

Buchanan's table 4.4.2 gives a mean weight per shell of 18,3 g, from which an equivalent mean length of 82,7 mm is calculated from his Appendix C. This weight is 3,1 per cent higher than the weight for a shell of 82 mm length, but increasing the latter by this percentage gives a length of 84,5 mm. The mean length of 82,7 mm is 0,8 per cent higher than 82 mm, but increasing the energy value of 28,24 kJ for a shell of this length (Appendix C) by this percentage yields a value of 28,5 kJ, while the value in table 4.4.2 is given as 28,9 kJ.

In his table 4.4.3 (p. 129), Buchanan gave the mean weight of shell from Tortoise Cave as 28,8 g, the mean length as 95,3 mm and the total kilojoules as 4 156 kJ. Using the same method as above, I obtained a total of 4 078 kJ based on mean weight, and 3 889 kJ based on mean length. It is thus not clear from both these examples how, with reference to Appendix C, Buchanan arrived at the mean length or kilojoule value.

Buchanan (p. 128) mentioned that seven samples from layers 1-6 of Elands Bay Cave had a mean yield ranging from 16 to 35 kJ: a difference of 119 per cent between the lowest and the highest. In his table 4.4.3 he commented on the sample from Tortoise Cave 'Although very different in mean weights and mean length, kJ per 100 grams are the same as for the other samples'. Since the kilojoule value was assigned by Buchanan in the first place and is the one constant by which all the shell MNI or gross weights were multiplied, this observation has little value.

In his Appendix D11 (pp. 246-247) Buchanan provided details of the percentage differences in kilojoule yields when these were calculated on MNI and on shell weight for samples from 30 sites (excluding sites VV1 and VV4, which he said (p. 247) were incomplete). These differences range from +49,2 per cent (site DD2) to -74,6 per cent (site VN1), indicating considerable potential for error according to the method used. The revised mean difference is only +0,3 per cent but the very wide range is a more accurate indicator of the unreliability of the method. This is emphasized by the fact that mean weight per shell ranges from 4,7 g (site VN1) to 36,8 g (site DD2): a difference of 683 per cent between the lowest and the highest, which obviously must have an effect on the conversions to kilojoules.

Buchanan (p. 247) commented that 'For individual sites the kilojoule estimates by the two methods vary appreciably, but tend to merge as sample size increases', and he provided data for sites with MNI of less than, and more than, 100, for each of which the difference was again only 0,3 per cent. In fact, however, this can be ascribed to nothing more than a remarkable series of coincidences in which the plus and minus percentage differences happen to cancel each other out, whether in the case of the whole sample or in the case of the samples with MNI below or above 100. Using the same data as in Buchanan's appendix, the difference between the two methods for the first ten sites, excluding VV1 and VV1, (n = 1 264) is +105 per cent. The difference for the second ten sites (n = 670) is +9,2 per cent, and that for the last ten sites (n = 749) is +11,9 per cent. If the number of sites is doubled, the difference for the first 20 (n = 1 934) is +52,5 per cent, that for the last 20 (n = 1 419) is +10,6 per cent, and that for the first ten plus the last ten (n = 2 013) is +51,4 per cent. This shows that merely increasing the sample size does not reduce the degree of difference: this depends on the sample *content*. This is a good example of the danger of using means without any indication of the range or the degree of variation of the samples.

According to Buchanan (p.33), whelks from his study area consisted chiefly of *Burnupena* spp. and *Argobuccinum argus* and he assumed a mean size of 45 mm and a mean energy value of 7,5 kJ. The four *Burnupena* species mentioned by Buchanan have maximum lengths of 50-70 mm (Kilburn & Rippey 1982: 94), while *A. argus* is considerably larger, with a maximum length of 115 mm (Kilburn & Rippey 1982: 75, under *A. pustulosum*). A mean size of 45 mm for both genera is thus likely to result in an underestimate of the contribution of the larger *A. argus*.

Buchanan provided no information as to how he arrived at his total of 276 150 kJ/m^3 for shellfish in his table 5.6. This figure was derived from his table 5.13 (p. 166) which is based on a sample total of 1,8003 m^3 and n/m^3 of 14 000. His Appendix D15 (p. 251) gives a mean of 340 626 kJ/m^3 for samples from 32 sites with a sample volume of 0,6003 m^3. If this mean is multiplied by the 'sample volume' of 39,14 m^3 used in some of Buchanan's calculations (e.g., tables 4.8.3 & 4.11.2), the total is in excess of 13 million kilojoules. Reference to his Appendix D16 (pp. 252-254) shows that five sites with a sample total of 1,3282 m^3 had a total of 1 504 740 kJ/m^3, or a mean of 300 948 kJ/m^3. Since the reconstruction was for the whole Elands Bay area, Buchanan's kJ/m^3 total appears to be a considerable underestimate unless he arrived at this by some method that cannot be determined from his published data.

The problems inherent in using mean values should be evident from the foregoing; and their use for this component adds yet another error factor to the calculations involved in Buchanan's dietary reconstruction. Since shellfish are, numerically at least, the major component of shell middens, the cumulative errors in Buchanan's method are likely to be considerable.

ROCK LOBSTERS

Buchanan used data given in his table 4.6.1. (p. 131) to suggest 400 kj/100 g 'as an appropriate value' (p. 34) for the energy yield of rock lobsters *Jasus lalandii*. Reference to the table shows this value to be a rounding-off of the mean of nine values that range from 352-498 kJ/100 g. Of the nine, only three are from South African sources, the others being from British, American and Australian sources and therefore presumably for other species, so that their use in calculating the kilojoule value for the South African species is questionable. The South African values have a mean of 405 kJ/100 g that is not too different from the value Buchanan adopted.

On the basis of data provided for the Bonteberg Shelter (Grindley 1967), Buchanan assumed an edible mass per rock lobster of 150 g, but considered that those in the Elands Bay area would have had an edible weight of 200 g, this being based, it seems, on the measurement of 19 or 319 mandibles from sites in that area and calculation of the mean carapace length. (The information on p. 34 is not clear as to whether by 'Eland's Bay lobsters' Buchanan meant those from Elands Bay 'sampled sites' (n = 19) or to both these and 'EBO, HSM & CLB' (n = 290), giving a total of 319). He stated that when the kilojoule values given above were applied 'to the assumed weights of edible flesh' they gave a value of 800 kJ per lobster for all the Elands Bay sites and two others (DE & PN) and 600 kJ for those from other sites. Noli (1986: 41) questioned the accuracy of Buchanan's (1985) calculations, pointing out that the mean carapace length of 104 mm Buchanan used in his calculations yielded a (mean) left mandible length of 14,0 mm, whereas the information provided by Horwitz (1979) for rock lobster from sites in the Elands Bay area yielded a mean mandible length of only about 10,2 mm, with a consequently lower carapace length and a flesh mass about half that used by Buchanan, whose kilojoule conversions 'could thus be overestimated by as much as 100%'.

Buchanan (table 4.6.2, p. 132) calculated the energy values for all the Elands Bay sites, as well as for other sites, by dividing the MNI by the (estimated) volume of the site to obtain the MNI/m^3. This figure was then multiplied by the kilojoule value appropriate to the site. The values are thus based on *estimated* volume, *mean* carapace length and *estimated* kilojoule value, all of which contain an inherent error factor. Moreover, the conversion of the MNI for the sample to MNI/m^3 for the whole site assumes that the distribution for the whole site is identical to that of the sample. This is almost certainly *never* the case (see, for example, Noli 1988, table 3), not only for rock lobster but for all components of the deposit.

FISH

Buchanan (p. 39) computed the MNI/m^3 for fish in the same way as he did for rock lobster. The actual MNI were increased by 15 per cent to compensate for a potential loss 'as a result of depositional and post-depositional processes, loss through sieves and other factors' (p. 40).

In his table 4.7.1 (p. 134) Buchanan provided information on the mean kilojoule yield per fish (four of which were estimates) for a total of 13 fish species and 2 families (Mugilidae and Clinidae). These values are based on mean length and mean live weight, 70 per cent of the latter being taken as the edible portion. The mean kilojoule values range from 277 to 21 100 with a mean of 3 350 ± 5 660 (*sic*), which is thus not the source of the value by which Buchanan arrived at the total values in his table 4.7.2 (p. 135). On p. 39 he stated that 'The mean yield over all species from west coast sites emerges at 494 ± 105 kJ 100 g^{-1}'. This value was also given his table 4.7.1. but it, too, is not the value he used for his calculations.

In table 4.7.2. the MNI/m^3 (Nm^{-3} in Buchanan's terminology) for the Vlei area is incorrectly given as 51 instead of 55. Since the mean kilojoule yield per fish (per m^3 if Buchanan's table is correctly set out) appears to have been obtained by dividing the kJ/m^3 by the MNI/m^3, the mean yield should be 1 092, not 1 177. If, on the other hand, the table is incorrectly set out and the kJ/m^3 were obtained by multiplying the MNI/m^3 by the kJ yield (in this case 1 177 as given by Buchanan), the kJ/m^3 should be 64 735.

It is not clear why Buchanan included the sample volume of the 'Other' sites in his calculations, since these sites yielded no fish remains. The volume represents only 0,12 per cent of the total and thus has no significant effect on the results.

The figure in the last row of each of the first three columns in this table is the total for that column. The last figure in each of the last three rows, however, has no discernible connection with the figures above it, being neither the total nor the mean. Reference to table 4.7.3 (p. 136) shows that here Buchanan provided a figure for the total kilojoules that yields the kJ/m^3, as well as the MNI/m^3 and the mean kJ yield per fish, given in the last row of the previous table. It is unusual, to say the least, to use data in a table that are obtainable only from a subsequent one and without reference to it; and the use of these data in table 4.7.2 shows that the figures given in the first three rows of the last three columns of this table are totally irrelevant to Buchanan's calculations.

In table 4.7.3 Buchanan provided the MNI, volume of sample, total kilojoules, n/m^3, kJ/m^3 and mean kJ/fish. If the total kilojoules are divided by the MNI they yield the same mean kJ/fish as when the kJ/m^3 are divided by the n/m^3, indicating that the latter computation is unnecessary.

In his table 5.15 (p. 168) Buchanan gave the same sample volume, MNI/m^3 and kJ/m^3 as in tables 4.7.2 and 4.7.3, but a mean kilojoule yield per 'animal' of 1 484. It is thus not clear what the correct figure should be.

I am at a loss to understand why Buchanan did not follow his usual procedure of using a mean kJ/g value, for example the 494 kJ/100 g given in table 4.7.1; nor am I able to understand why his values were not derived from the sum of the figures given in the first three rows of the last three columns of his table 4.7.2. Since I do not know by what means Buchanan obtained his basic kilojoule values, I cannot comment on the overall results of his calculations. The use of estimated volumes of deposits and means for MNI/m^3 and kilojoules, however, all introduce their own error factors.

TERRESTRIAL ANIMALS

In addition to a number of error factors that he mentioned, Buchanan (p. 41) commented that 'the only safeguard against gross errors lies in very large samples from excavations. For EBC, with a total sample of 39,14m^3 and DK1 of 37,3m^3, the assemblages may be reasonably representative, but for all the other samples analysed, there are potentially gross errors'. From this it will be clear that, by 'samples', Buchanan meant excavated volumes rather than MNI of animals. Despite the potential for 'gross errors', and the fact that site EBO yielded a total of 33 terrestrial mammals from a volume of only 7,8 m^3 as against 58 from Elands Bay Cave (EBC) with a volume 3,6 times that of EBO, Buchanan provided data in his table 4.8.3 (p. 140) for four sites in the Elands Bay area. In this connection, it should be noted that the 'EBC' for which the volume was given on p. 41 includes *all four* of the sites in his table, whereas in the table it refers only to Elands Bay Cave.

Buchanan (p. 42) stated that 'The energy calculations in this study have been based on a mean yield for all terrestrial mammals of 700 kJ $100g^{-1}$'. His table 4.8.1 (p. 137) gives the mean value for 18 terrestrial mammals ranging in size from water buffalo to monkey - none, apparently, southern African species of the kind likely to be found in archaeological deposits in the coastal region - as 646 ± 152 kJ/100g, and the value of 700 kJ/100 g is presumably a rounding-off of the former figure. The range is 343-859 kJ/100 g, so that the lowest value is some 47 per cent below the mean and the highest about 33 per cent above it, indicating the potential range of error if only the mean is used. At the foot of this table, Buchanan stated that 'The energy yield for mammals has been calculated on 3000 kcals or 1259 kJ 100 g^{-1}', an amount some 95 per cent higher than the mean value given above. Buchanan's Table 4.8.2 (pp. 138-139) gives the kilojoule yield per animal for southern African species. The value is generally 7 000 kJ/kg, or the 700kJ/100 g quoted above, but in some cases it is higher, for example, *Genetta genetta* (7 143 kJ/kg), or lower, for example, *Alcelaphus caama* (6 860 kJ/kg). In the case of domesticated cattle *Bos* sp., only the kilojoule yield is given, for small and large animals. Using the various factors in Buchanan's calculations, it is possible to calculate the mean live mass of a small *Bos* sp. as 567 kg and that of a large one as 711 kg. Comparison of these values with those given for animals of similar size suggests that Buchanan's kilojoule allocations were arbitrary. The given live mass of an eland *Taurotragus oryx*, 580 kg, is only 2,3 per cent more than that of a small *Bos* sp., yet the 2 436 000 kJ Buchanan allocated for the eland is 21,8 per cent more than that allocated for the *Bos* sp. The given live mass of a buffalo *Syncerus caffer* is 5,5 per cent more than that of the large *Bos* sp., and the kilojoule allocation is 5,0 per cent more. This seems more consistent than the increase of almost one order of magnitude in the case of the smaller *Bos* sp. relative to the eland. Table 4.8.3 (p. 140) gives no indication of how the total kilojoules for the Elands Bay excavated sites were obtained, nor is this information obtainable from any of Buchanan's other tables or appendices. It must be presumed that he calculated these from the faunal lists for each site (not included in his study) and calculated the kilojoule values on the basis of the yields per animal contained in his table 4.8.2 (pp. 138-139), or on the mean of 700 kJ/100 g. Buchanan's calculations are therefore not testable, but may be presumed to contain error factors derived from basing the total kilojoules on the numbers of individuals in each species, assuming a constant weight for each individual, and multiplying the number of individuals in each species by the yield given in his table 4.8.1, another unverifiable 'constant'.

Buchanan (p. 43) alleged that 'there are no historical records of elephants or rhino within about 100 km of Eland's Bay'. Piketberg is some 60 km SE of Elands Bay and, to quote from but a single source, on 5 September 1685, just north of Piketberg, Simon van der Stel narrowly escaped being killed by a charging rhinoceros. Six days later, thus even closer to Elands Bay, the expedition had separate encounters with two elephants (Moodie *ed.* 1960: 401). The Olifants River, which passes within 60 km of Elands Bay, was not given its name without good reason: 'it is so called from the great number of elephants often found on its banks' (Moodie *ed.* 1960: 402). Moreover, as W. J. J. van Rijssen (1990 pers. comm.) has pointed out, there is also indirect evidence, in the many rock paintings of elephants in sites in the western Cape Sandveld region.

CETACEANS

According to Buchanan (p. 44), 'Skead (1980: 690-709) quotes *in extenso* the historical references to the exploitation of whales in Cape waters from the first sighting by Vasco da Gama's sailors in 1497 of Hottentots eating whalemeat at St Helena Bay'. As mentioned in Chapter 1 of my study, there is no evidence as to the identity of the men seen by Da Gama *et al.*, although they were 'Hottentots' *sensu lato* (i.e., Khoisan); and the information about their eating whales is probably the interpolation of a later writer. All that the people at St Helena Bay were *observed* to have eaten were a seal and some 'roots'.

Conceding that the evidence for cetaceans from archaeological deposits is minimal, Buchanan remedied this by taking a value of 350 kJ/100 g for the edible portion of toothed whales and 650 kJ for baleen whales (p. 45). Then (p. 47) he calculated the total annual kilojoule yield for toothed whales for the period 1963-1981 'based on strandings for the whole coastline from Namibia to west of Mossel Bay'. The total of close on 4,8 million kJ was divided equally between the 2 500 km of coastline 'and thus, on a proportionate allocation, the annual yield for the 100 km range round Eland's Bay would be about 200 000 kJ, assuming an even distribution' - an assumption that is surely questionable. It should be noted that the total number of 'events' (strandings) in Buchanan's table is actually 119, not 109 (see also p. 46) and that the kilojoule values are based on the *mean* live mass of the three families of cetaceans included in the calculations, 'calculated on the recorded species frequencies'. Information was not provided as to whether or not all the stranded individuals were fully-grown adults. The allocation of a 100 km range around Elands Bay, or an average of 50 km in each direction along the coast, seems remarkably high for hunter-gatherer activities and appears to imply that these people travelled considerable distances in order to avail themselves of whale flesh. How they would have known it would be there was not discussed. Moreover, if the range is taken at 100 km in order to accommodate the hypothetical contribution of whale meat to the diet of the Elands Bay people, the dietary reconstruction should also include the contents of all the middens and other sites in that range.

On p. 48 Buchanan commented that 'Any attempt to estimate the prehistoric energy yield from the few heavily exploited species (Mysticeti [*sic*] - Baleen whales and the Sperm whale Physeter catodon) would be too speculative'. None the less, he applied 'an arbitrary maximum of 1,5 million kilojoules from each animal', with the proviso that 'if the historic [*sic*] record is accurate in suggesting that hunter-gatherers ate nothing but whale for weeks or even months this estimate is understated'. It would be interesting to know to which historical record Buchanan was referring: he cited (p. 45) Paterson (1790: 107) as saying that a beached whale 'affords them sustenance for half a year', which I have questioned in my study (Ch. 4) and Backhouse (1844: 33) as saying that people 'live *principally* on it for many weeks together' (my emphasis), which is not the same thing as eating 'nothing but whale'.

According to Smithers (1983: 301-344), sperm whales are members of the Family Physeteridae in the Suborder Odontoceti, toothed whales (which also includes the Family Delphinidae, none of which appear to have been included in Buchanan's calculations). It is thus not clear why Buchanan excluded sperm whales from the kilojoule values he gave on p. 48 for toothed whales and included them instead with baleen whales (Suborder Mysticeti), although a possible reason is that these are the two largest types of whale (P. Best 1990 pers. comm.).

Buchanan (p. 48), having estimated that 'the prehistoric incidence of strandings . . . at one every 10 years', calculated that 'the annual yield would be in the region of 150 000 kJ', and that for all species of toothed, baleen and sperm whales (*sic*) as 350 000 kJ. On this basis, and taking the aggregate (and approximate) volume of all the Elands Bay sites as 2 500 m^3 and a period of 1 500 years (1800-300 B.P.), he arrived at a total of 210 000 kJ/m^3. This amount was included in his dietary reconstruction, and is discussed below.

SEALS

On the basis of data provided on p. 52, Buchanan (p. 53) assumed a mean live mass of 25 kg per seal, the weight of a seal in its second year. This was because Parkington (1976) had ascribed 'virtually all the seal bone in the EBC sequence to yearlings and the 21 seals identified by Horwitz (1979: 74 [from her excavation of Hailstone Midden]) were also related to the same restricted age group' (p. 52). Since yearlings have a mean live mass of 18,8-21,5 kg (p. 52), I assume that the increased weight Buchanan allocated was to cover the possibility that some of the seals were older than yearlings. He also assumed that 55 per cent of the live mass was edible, with a value of 750 kJ/100 g of edible flesh 'including viscera and some blubber'.

Buchanan's table 4.10.2 (p. 146) shows that he followed much the same procedure for calculating the kJ/m^3 as for fish and terrestrial mammals. In the present case, the MNI, 50, was multiplied by 55 per cent of the live flesh weight, 25 kg, and the total multiplied by 750 kJ/100 g of edible flesh. The total was then divided by the total volume of the sample to yield the kJ/m^3. In this regard, it is surprising to note that 32 sites had a volume of only 0,6003 m^3. This figure provides a mean of approximately 0,02 m^3 per site, about equivalent to the contents of two ten-litre buckets. If this figure is incorrect, as it certainly seems to be (see Site Volumes above), then the kJ/m^3 applied to tables 5.6 (p. 121), 5.18 (p. 171) and 5.21 (p. 174) are also incorrect.

In his table 5.18 Buchanan provided summary data for sample volume, n/m^3, kJ/m^3 and percentage of total diet. Where the percentage of total diet for the 'Eland's Bay Research Area' is concerned, reference to Buchanan's table 5.6 indicates that the value of 359 748 kJ/m^3 is 28,1 per cent of the total of 1 277 706 kJ/m^3, not 16,6 per cent as given in table 5.18. Also, for some reason not explained, the figure of 129 748 kJ/m^3 given in table 4.10.2 was increased in table 5.18 to 359 748 kJ/m^3. Reference to table 5.21 shows this additional amount of 230 000 kJ/m^3 to be an estimate based on 'no evidence'.

BIRDS

Buchanan (p. 53) assumed a value of 600 kJ/100 g for birds of all species and an edible percentage of live mass of 60 for penguins and 75 for all other species. The mean kilojoule yield for each species was given in table 4.11.1 (p. 147), without any indication of the number or species of individuals on which this value was based. The kilojoule values were then applied to the numbers of birds per species in the Elands Bay site samples in table 4.11.2 (p. 148), apparently on the assumption that all bird remains found in the sites represent food remains: something that could be questioned, especially with regard to the passerine from EBO, although the contribution of this bird was negligible. The kJ/m^3 value was then used in the reconstruction of the prehistoric diet in table 5.6 (p. 159), which is discussed below.

TORTOISES

Buchanan (p. 55) used a value of 557 kJ/100 g derived from the Australian turtle *Chelodina rugosa* and an average edible content of 340 g per tortoise to calculate the energy yield of these reptiles. These factors give a total of 1 893,8 kJ per tortoise, which Buchanan increased to 2 000 kJ, a difference of 5,6 per cent that increases the total kilojoules in his table 4.12.1 (p. 152) by 15 293 kJ, or 391 kJ/m^3. This component is discussed further below.

DIETARY RECONSTRUCTION

In his table 5.6 (p. 159), Buchanan provided kJ/m^3 for the various dietary components discussed above, together with their percentage contribution. Included in the table are values for plant foods and honey that, as indicated, are estimates based on ‘no evidence’. The same also probably applies to the value for rodents, snakes and microfauna since information on these was not provided elsewhere. These account for some 10 per cent of the total dietary budget, or 39,4 per cent of the terrestrial component. As mentioned previously, the contribution of cetaceans

(16,4% of the overall total) is speculative, and the addition of an extra 230 000 kJ (18,0% of the overall total) to the contribution of seals is unexplained and thus probably also speculative. This means that 44,6 per cent of the total kilojoules cannot be accounted for on the basis of the *archaeological* data.

At the foot of this table Buchanan gave the contribution of the marine and terrestrial components. From this, it is evident that he included all the birds in the marine component, whereas his table 4.11.2 (p. 148) shows that some 7,2 per cent of the total (pelicans to passerines) are not marine birds. If the dubious contributions mentioned above are excluded and 7,2 per cent of the contribution of birds allocated to the terrestrial component, the revised percentages are not that different from those given by Buchanan: 71,2 per cent marine (74,2% in Buchanan) and 28,9 per cent terrestrial (25,8%).

What is more in question than the relative contribution of the terrestrial and marine components, however, is the manner in which the values were reached on which these allocations were based. The accuracy of the calculation of the kJ/m^3 values for every component is open to question, as is the use of mean weights and kilojoule values. The estimates of volume are also questionable, and in this regard it must be pointed out again that the number of sites used in calculating the energy values for the Elands Bay area varies from 4 to 36 according to the component, with consequent changes in the total estimated volume on which the kJ/m^3 values were calculated, and thus in the kJ/m^3 themselves.

On the basis of information relating to five sites, *excluding Elands Bay Cave*, Buchanan (p. 91) calculated an annual rate of accumulation of deposit of 1,33 m^3. Of Elands Bay Cave he said (p. 92) that since the occupation period extended over some 5 000-6 000 years, the total deposit of some 150 m^3 indicated that 'Annual rates of accumulation would thus be very small which implies very short or very irregular visits or both'. It is not clear why Buchanan was unable to calculate the volume of deposit for the 'pottery period' of this site when he was able to do so for

Tortoise Cave, to which the comment about Elands Bay Cave also applied. It is arguable that at least some of the open sites in the vicinity are attributable to the cave-dwellers, which would have an effect on the rate of deposition in the caves. What is important here, however, is the assumption of a constant rate of deposition, something unlikely to have happened in reality.

Buchanan then used this annual rate of accumulation as a factor in the calculation of the number of 'person/days' spent in the area. The total food intake (table 5.6) was multiplied by the annual rate of accumulation and divided by the daily kilojoule requirements per person (table 5.3, p. 155) to give a total of 192 person/days a year. Buchanan considered that 'Band numbers below 10 would not be viable for long, and the time spent in the area is unlikely to have been less than 7 days per visit' (p. 93). Given that, according to Buchanan's calculations, 'the potential range emerges at 10 to 25 in band numbers and from 7 to 20 days in time spent in the area', the question of the viability of the group during such a short period is irrelevant; and the suggestion that a visit of less than 7 days was 'unlikely' is speculative: people would have spent as much time in the area as was necessary or desirable - more likely the latter than the former, since there is no evidence that people were *obliged* to spend time at the coast.

Using a formula (p. 95) to calculate the amount of time spent in the area, Buchanan concluded that the duration was 'about 10/11 days per visit if visited regularly once a year and a maximum duration to any annual visit of about one month'. This was based on an estimated average of 18 persons in the group spending 192 person/days per year; and the same approximate period was reached after decreasing or increasing four of the five 'parameters' used in the calculations in order to allow for a margin of error in these four 'parameters', the only constant being the number of years. Buchanan did not provide any information as to how, on the same total energy budget that he calculated for the area, people could have spent as much as a month at the coast, a period that is about three times as long as his average of 10/11 days or half as much again as the 20 days arrived at in his first calculation. It seems that, in order to do so, they would have had to consume less than their 'daily need'. The possible permutations of the 'parameters' used in these calculations are endless: it could, for example, be postulated that the energy budget available from Buchanan's calculations would have enabled the band to spend the three months of the Cape midwinter at the coast only once in each decade or so, or the whole winter once in each fifteen years. Similar variations can be made by increasing or decreasing the number of people.

SUMMARY AND CONCLUSIONS

In this evaluation I have concentrated on the major mathematical aspects of Buchanan's reconstruction since these are the basis for his conclusions.

Summary

Sample volumes. As mentioned in the section on site volumes above, the accuracy of the method by which Buchanan arrived at these is questionable. Moreover, although in his Appendix A he gave a total volume of 2 283,2 m^3 for the deposit of 'all sites', he used different volumes for different components: 39,6615 m^3 for rock lobsters (table 4.6.2., p. 132), 39,7403 m^3 for fish (table 4.7.1., p. 134) and seals (table 4.10.2, p. 146, but see my comments above, under shellfish and seals, re the 'sample volume' of the 32 sites), 39,14 m^3 for terrestrial mammals (table 4.8.3, p. 140), birds (table 4.11.2, p. 148) and tortoises (table 4.12.1, p. 152), while for shellfish the volume is a mere 1,8003 m^3 (table 5.13, p. 166 - but see Appendix D16, pp. 252-254, where he gave volumes of samples and of total deposits for five sites in the Elands Bay area. The total 'sample' volume is 1,3282 m^3, while the volume of 'total deposit' is 324,65 m^3). Although there is a difference of only 1,5 per cent between the highest and lowest of the volumes mentioned here (excluding that/those for shellfish), the inconsistent use of different volumes affects the reliability of the total calculations, and makes light of the calculation of these volumes to four decimal places.

Shellfish. As shown above, the accuracy of the methods for calculating the energy values of this component is questionable, and it appears that the MNI for mussels may have been underestimated considerably. It was also shown that it is not possible to determine from Buchanan's tables how he reached the kJ/m^3 value he used in table 5.6, so that the accuracy of this contribution cannot be determined.

Rock lobsters. The contribution of this component is derived from mean carapace length, mean kilojoule value and estimated sample/site volume. Each of these has an inherent error factor that compounds the total error when the kJ/m^3 are calculated. Also, as mentioned above, Noli pointed out that the calculation of the carapace length, and thus the kJ/m^3, may also be incorrect.

Fish. The problem with the acceptability of the value for this component is much the same as that for rock lobsters, except that it was not possible to determine how the kilojoule value was obtained that was used in arriving at the kJ/m^3 in table 5.6 since a mean kilojoule value could not be ascertained from the information Buchanan provided in his various tables. However, the basic method appears to be the same as that for rock lobsters and thus to contain the usual cumulative errors.

Terrestrial mammals. As was the case with the fish, it was not possible to determine the correctness of the kJ/100 g or the kJ/m^3 values Buchanan attributed to this component, and the cumulative errors also apply here.

Cetaceans. Although I agree with Buchanan (p. 71) that 'It would be unrealistic to disregard the edible resources of the Eland's Bay vicinity which although leaving no trace in the deposits would merit exploitation', I cannot accept the method by which he arrived at the contribution of this component, which can best be described as pure speculation. Buchanan's calculations, based as they are on strandings that have occurred since commercial whaling severely depleted whale populations worldwide, probably also have little relevance to the incidence of strandings in the period of his dietary reconstruction, which is well before the beginning of commercial whaling.

Seals. The method by which Buchanan arrived at the contribution of this component is also much the same as that for rock lobsters and other components, thus containing cumulative errors. In addition to these, Buchanan, for a reason not explained, increased the amount of this contribution by another 177 per cent: presumably another speculative addition for which there is no evidence.

Birds. The method for calculating the contribution of this component contains the same cumulative errors as for the other components.

Tortoises. The application of a kilojoule value derived from an Australian turtle to this component is questionable; and the method of calculation contains the usual cumulative errors.

Minor components (honey, plant foods, reptiles, etc.). The contribution of these is, like that of the cetaceans and part of the value for seals, based entirely on speculation.

Conclusions

Although this evaluation has not been completely exhaustive, in that I have not dealt with *every* aspect of Buchanan's dietary reconstruction, it should be clear from the foregoing that every stage of his calculations is open to question. Each stage contains at least one error factor, the magnitude of which and its direction (i.e., above or below the computed value) can only be guessed at. Furthermore, almost half of his computed energy values are, on Buchanan's own admission, based on 'no evidence' and are thus mere speculation. The consequence of this is that the energy values and their conversion into 'person/days' and thence into time spent annually at the coast cannot be accepted.

The question must also be asked as to whether all the animal and plant remains found in archaeological deposits can be taken to be dietary components. Noli

(1986: 38) questioned the validity of adding the kilojoule contribution of a whole hippopotamus - according to his table 9, close on 6 million kJ and 54,7 per cent of the total energy budget for his excavated sample from Hailstone Midden - on the basis of the presence of a single sesamoid; and in Chapter 9 of my study I have shown that not all the plant remains from excavated sites have dietary value.

The problem of how long people spent at the coast is also aggravated by Buchanan's assumption that the deposits in the coastal area resulted only from the activities of hunter-gatherers, despite the evidence (potsherds and the remains of domestic stock) that pastoralists were in the area at the time and thus may also have been responsible for at least some of the deposits. In this assumption, Buchanan ignored what was arguably the most significant factor for humanly-induced social and environmental change in the region since the appearance of mankind itself: the introduction of exotic, domesticated animals. As far as the evidence from Kasteelberg goes, domesticated sheep were in the region by about 1800 B.P., and this appears to be supported by the presence of potsherds in the Elands Bay area from about this time. It is possible that, initially, the numbers of herders and their stock were small, so that their impact on the lives of the hunter-gatherers and their environment was not great. By about 400 years later, however, when cattle were introduced, it is probable that numbers, of both people and livestock, had increased substantially, to the detriment of the hunter-gatherers, who found themselves in conflict with the herders in respect of rights to territory and the resources therein. These aspects are dealt with in greater detail in my study but are mentioned here to support my contention that Buchanan's model, of a pristine and totally unchanging world in which resources were evenly distributed, both in time and space, and constantly available to the aboriginal hunter-gatherers of the area, is archaeologically untenable.

Much as archaeologists would wish to extend the interpretability of the data they derive from their excavations, to do so in the manner employed by Buchanan is more likely to add to the problems of interpretation than to aid in their solution.

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