

Chapter Three

The Beginnings of Research

In the study of all things there are peaks and troughs of interest and research. So too with the study of southern Cape Later Stone Age burials (Fig. 3.1). As is to be expected, each peak of interest brings with it a different set of theoretical, methodological, technical and, indeed, historical influences. The earliest peak of interest in the burials, as measured by numbers of publications, was the greatest, though not the most theoretically and methodologically aware.

Most of this chapter therefore deals with the theoretical, methodological and historical factors inherent in previous studies of southern Cape Later Stone Age burials and that led to the present state of research. When considering earlier research of this sort, it is important to take into account the period in which it was done and the then current standards of practice and knowledge: it is all too easy to criticize earlier workers for not having done things that were conceived only decades later. On the other hand, some destructive excavations were, as we shall see, continued during a period when a much higher standard had been well established.

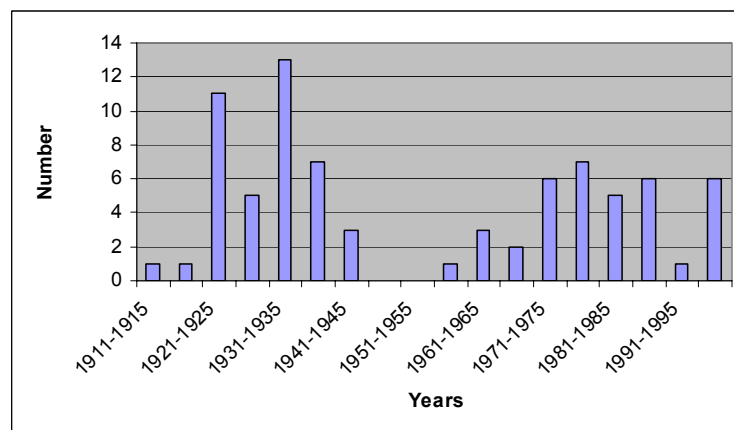


Figure 3.1: Bar graph of number of publications on southern Cape burials by year.

Changing concepts

It seems that any discussion of change in science cannot now proceed without some mention of Thomas Kuhn's (1970) influential scheme of the progress of science, although, many of his arguments have now been contested. Both what he said and his critics' responses help to conceptualize the historical trajectory of the study of southern Cape burials.

Kuhn entitled his book *The Structure of Scientific Revolutions*. It is the revolutionary aspect that is most often emphasized. In brief, he proposed that the progress of science is not a continuous, incremental movement towards some ultimate Truth. Instead, it is a disjunct series of paradigms, each successively replacing the previous by a process of scientific revolution. The process can be summarized thus (Chalmers 1999:108):

pre-science—normal science—crisis—revolution—new normal science—new crisis and so forth, in a continuous, open-ended scheme. Paradigm succeeds paradigm, concept replaces concept.

Each paradigm of normal science is made up of the general theory, laws, methods and techniques for their application that are adopted by members of the particular scientific community. Within normal science, practitioners engage in what Kuhn (1970) characterized as 'puzzle solving' governed by the theory and methods of the paradigm. Unsolvable problems, or anomalies, develop within paradigms; some may be accommodated by adjustment of the paradigm and are to be expected, others are more serious. Eventually, too many contradictions and anomalies develop: they strike at the very heart of the paradigm. There is then a crisis in the normal science. A scientific revolution may follow: the old paradigm is replaced by a new one consisting of a new set of theories and methods, and, importantly, is incompatible with the previous one.

This brief sketch of Kuhn's scheme of scientific revolutions could be taken as a prelude to the description of research into southern Cape burials in terms of a series of paradigms. But, that would be an oversimplification.

Two large-scale changes have occurred in Anglo-American archaeology over the last century. 'Old archaeology' was aggressively supplanted by the New Archaeology which, has in turn, been challenged by the post-processual movement. These changes are often characterized as paradigmatic. They are not, however, paradigmatic changes of the sort Kuhn described: there was not a complete change of theories and methods, on the contrary, certain aspects of previous 'paradigms' continued into newer ones. Indeed, current archaeological practice encompasses researchers undertaking studies characteristic of all three 'paradigms'. A mix of 'paradigms' exists rather than a series of paradigmatic changes.

In the case of research into the southern Cape burials, a series of changes certainly occurred over the century or so under review, but they cannot be characterized as paradigmatic. For a (Kuhnian) scientific revolution to occur, a *complete* change of theories and methods needs to take place. No such complete change has occurred in studies of the burials, besides, possibly, in the field of physical anthropology. The changes that are described are of a more incremental nature. No major crisis developed; no complete revolution occurred.

Although the story of the development of the study of the southern Cape burials is not a perfect example of Kuhnian revolution, his scheme nevertheless contains some interesting concepts that prove useful in discussing the progress of ideas, set, as always, within a social matrix. Although Kuhn was a pioneer of the social constructivist view of science, he did not allow sufficiently for the influence of social contexts on the formation of pivotal ideas. It is with such ideas that this chapter is largely concerned. True, much of the empirical data unearthed by earlier workers and mentioned in this chapter is of material significance in the analytical chapters of this thesis. These data are, however, presented in detail in

the relevant places and are summarized in Appendix A. This chapter deals instead primarily with the theoretical and methodological (and inevitably historical) issues that inform the work conducted by various researchers. In this way, the current state of knowledge can in part be explained: why have certain studies been conducted and others neglected?

The big men in the Cape

The first peak of interest in the southern Cape burials, that between the 1920s and 1940s, was particularly characterized by the work of four especially influential men: Louis Péringuey, Frederick FitzSimons, John Hewitt and John Goodwin. Although they were not the only people active at the time, they were the ones who were the most important in the development of interest in the southern Cape burials, excavation of skeletons and professionalization of the work. This period was the era of ‘big men’, influential individuals who, through their professional positions and networks with other intellectuals of the day, were able to control the development of the nascent disciplines of archaeology and physical anthropology. It was also the time of the amateurs. Of the four, only Goodwin was a trained, professional archaeologist. Indeed, at that time, he was the only professionally trained archaeologist in South Africa.

Much of the earlier, pre-Goodwin exploration work was done by educated gentlemen working and living in the areas they explored. For instance, Charles James Whitcher, whose name is now indelibly associated with an important rock shelter, was a farmer and timber merchant at Coldstream whose explorations along the Tsitsikamma coast were of considerable significance. Like him, the other men I discuss in this chapter, besides Goodwin, were also amateurs in that none was trained in archaeology. They were, however, in very different positions from other interested amateurs: they were in senior positions in three newly established museums. They were thus able to link into networks of educated amateurs ‘in the field’ and influence their work and gain collections of the materials recovered by them. They were central and influential nodes within the

developing world of ‘anthropology’ (including archaeology, physical anthropology, ethnology, and so forth). Their interests and choices—and indeed competition between them—played a major role in the development of the study of Later Stone Age (a term coined by Goodwin) burial practices in the southern Cape, and indeed, archaeology as a whole in South Africa.

The British Association meeting

Alan Morris (2002) has persuasively argued that the 1905 meeting of the British Association for the Advancement of Science (the British Association), held in several parts of colonial southern Africa, played a major role in the development of anthropology (including archaeology), particularly physical anthropology, in the nascent South Africa (see also Dubow 2000). Phillip Tobias (1985) has suggested that physical anthropology as a discipline began in 1877 in South Africa. He links this beginning to the founding of the South African Philosophical Society in Cape Town. Much of the physical anthropological research that was conducted in the years following 1877 was, however, undertaken by foreign-based researchers and published in foreign journals (e.g., Shrubbsall 1898, 1922). It was, however, the 1905 British Association meeting that proved to be the real catalyst that initiated intensive anthropological research.

Whilst a number of presentations at the meeting focused on some aspect of anthropology, two men were of wider influence: Alfred C. Haddon, the chairman of the anthropology section, and Felix von Luschan, the director of the Museum für Völkerkunde in Berlin and a prominent German ethnologist. Both gave presentations imploring the scientific community, both British and colonial, urgently to undertake studies of ‘the native peoples’ (Morris 2002).

Of the people who later came to dominate the field of physical anthropology, only Robert Broom and Thomas Dreyer were in a position to be directly influenced by the British Association meeting: Broom was certainly at the meeting, and Dreyer probably also attended. Broom’s later contribution to physical anthropology was,

however, in what are now the Northern Cape and Gauteng Provinces (at the time of the meeting he was more interested in mammal-like reptiles). His work was of only minor importance in the development of the study of the southern Cape material (besides one particularly influential synthetic paper entitled *A contribution to the craniology of the yellow-skinned races of South Africa* [Broom 1923]). Dreyer, on the other hand, does not seem to have been immediately influenced by the meeting: he continued his doctoral work in zoology (Morris 2002:339). He did, however, later become important when he initiated a major programme of excavation in the southern Cape whilst based at Grey University College in Bloemfontein.

Louis Péringuey

The most profound influence of the meeting was less direct. Sir David Gill, Her Majesty's Astronomer at the Cape and one of the trustees of the South African Museum (he was also the man who had formally suggested that the British Association meeting be held in South Africa), asked Haddon to give his opinion of the state of the study of anthropology in South African museums (Shaw 1978:6). Haddon was scathing: "I have visited the museums of Cape Town, Durban, Pietermaritzburg, Bloemfontein and Bulawayo and have been greatly disappointed to find that ethnology is on the whole very poorly represented in all of them and in scarcely any of them is there an officer who has any special knowledge of or a predilection for that science" (Shaw 1978:6). He continued, "Usually no pretence is made of keeping the objects of one tribe distinct from those of another and often the specimens, even if labelled at all, are labelled in such general terms that the information is of little value and the interest of the specimen is reduced to a minimum" (Shaw 1978:6). Haddon's concern with the 'tribal' affinities of objects presages later obsessions with racial typologies.

The trustees of the South African Museum were stung into action. Louis A. Péringuey (1855–1924), the deputy director of the South African Museum, was

put in charge of its anthropology collections (there was not yet a formal anthropology department). In 1906 he became director of the museum. As director he continued to oversee the anthropology collections. Péringuey was a good choice. His interest in archaeology pre-dated the British Association visit. In 1885 he had discovered handaxes whilst working in vineyards in Stellenbosch and Paarl (Péringuey 1899; Summers 1975:116). He later described these as forming the Stellenbosch Industry. This material, and other collections of stone tools from around the Cape, led him to publish, in 1911, the first substantial work on South African archaeology. He entitled it *The Stone Ages of South Africa as Represented in the Collection of the South African Museum* and published it as part of the *Annals of the South African Museum* (Péringuey 1911). He evidently intended his publication to establish the pre-eminence of his museum in the study of archaeology.

His initial interest in archaeology, spurred on by the British Association meeting, led Péringuey to make his greatest (and now most controversial) contributions to South African anthropology and prehistory: the collection of human skeletons and the production of life-casts of San and Khoe people. Both of these endeavours were carried out largely through the agency of one remarkable man, James Drury (1875–1962).

Drury, a Scot, joined the museum in 1902 as a taxidermist (Booth 1988), but it was under Péringuey's directorship that he did his most famous work. The most memorable, and in recent years most controversial (Davison 1993, 1998; Skotnes 1996; Lane 1996), component of his work was the production of life-casts of San and Khoe people. These were a direct result of the British Association meeting: von Luschan had tried to make a life-cast whilst in Cape Town (the attempt was nearly fatal since the technique for allowing the subject to continue breathing had not yet been mastered [Shaw 1978:6]). Von Luschan and Haddon were convinced that the San and Khoe were both dying 'races' and that anthropological records needed to be made of them. Upon assuming directorship in 1906 Péringuey pressed Drury to try his hand at casting. He proved adept at it and eventually

undertook nine major expeditions to various parts of South Africa and South West Africa (now Namibia) between 1907 and 1923 (Booth 1988:22).

Less famous than the life-casts, but more relevant for this study, is Drury's excavational work. It was also done at the director's request. Péringuey had many correspondents. One of these was J. S. Henkel, the conservator of forests at Knysna. In 1909 he informed Péringuey about a cave at Coldstream, near the mouth of the Lottering River (Fig. 3.2). The owner of the farm, Witcher, the timber merchant, had been digging in the cave for guano to use as agricultural fertilizer for some time. In the process of his digging he uncovered about seventeen burials, all lying in a contracted position, their knees drawn up to their chests, and many covered with flat slabs of rock (Péringuey 1911:153, 156). Henkel informed Péringuey, who hastily dispatched Drury to continue digging in the site. He arrived on 15 February 1911 and continued working there, with the help of some of Witcher's labourers, until the beginning of March (Fig. 3.3; Summers 1975:118). During that time, he uncovered an astounding twenty-eight burials, many of which were accompanied by grave goods; at least eight were the graves of children. The most famous of the burials was accompanied by—astonishingly—a painted stone:

I started another section down and when nearly the end of the layer I found the second full skeleton and the most interesting find so far. I was having layers carefully removed when I saw back part of skull protruding out of ground so started to carefully work earth away all round with trowels and hands and in so doing found a large flat stone lying on shoulder, so with great care removed the earth from on top, and thus exposed was the finest Painting I have so far seen anywhere, and with great care got it cleaned of all dirt adhering and so removed to Mr Witcher's house where I finished cleaning, there is three figures in colours and realy marvellous it is. The position of stone was flat on shoulder with feet end of figures next to the jaw of skeleton on painting side up . . . there was a flat stone lying on hips of skeleton also but there were no tracings of Paintings (Drury's field notes cited in Wilson *et al.* 1990:192–194, spelling and grammar as in original notes).

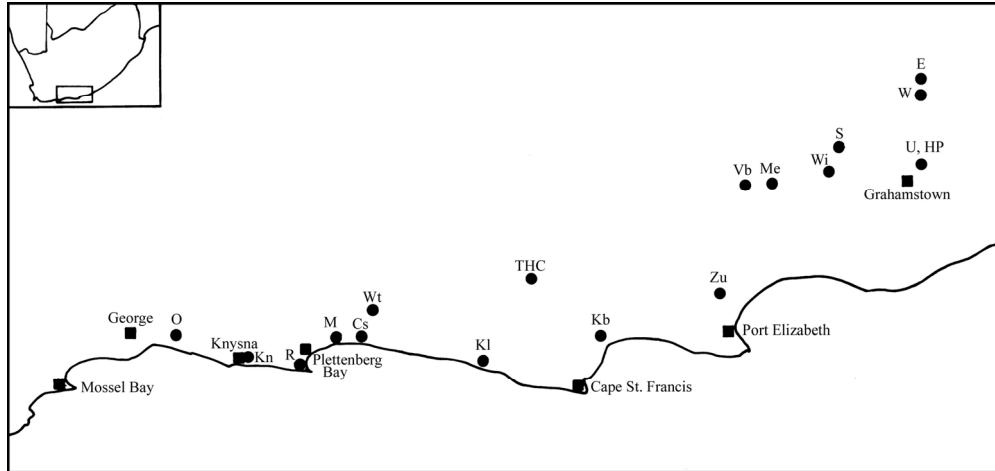


Figure 3.2: Locations of sites mentioned in the text. Cs Coldstream; E Edgehill; HP Howieson's Poort; Kb Kabeljous River sites; Kl Klasies River Mouth Cave 5; Kn Knysna open site; M Matjes River Rock Shelter; Me Melkhoutboom; O Oakhurst Shelter; R Robberg sites; S Spitzkop; THC The Havens Cave; U Uniondale Rock Shelter; Vb Vygeboom; W Welgeluk; Wi Wilton Cave and Large Rock Shelter; Wt Witcher's Cave; Zu Zuurberg.

Roger Summers (1975:118), who wrote a history of the South African Museum, reported that Drury proved to be a “competent excavator who recorded exactly where this or that ornament lay”. Unfortunately, Drury's notes, housed at the South African Museum, have never been adequately published. So exciting were Drury's finds that Péringuey added a hasty additional chapter ‘The Coldstream Cave’ to his 1911 monograph.

In addition to Coldstream, Péringuey arranged the investigation of eleven other archaeological sites between 1913 and 1922. Most of this work remains unpublished. Besides the archaeological excavation to recover skeletons, Péringuey also used his network of correspondents to acquire skeletons from more recently living people, mostly from what are now the Northern Cape Province of South Africa and Namibia. Unfortunately, some of the skeletons turned out to be those of the very recently living. Some of the horrors of this period of collecting human skeletons have recently been exposed (Legassick & Rassool 2000). It was largely through Péringuey's energy that large-scale collection of skeletons was begun and the South African Museum built up its anthropology collection.

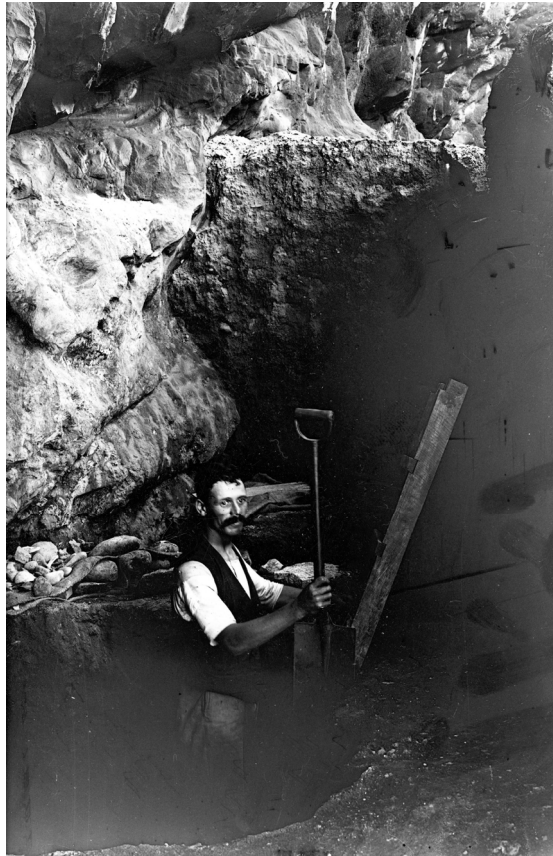


Figure 3.3: James Drury excavating at Coldstream Shelter in 1911 (photo: Iziko South African Museum).

Frederick FitzSimons

What started with Péringuey in Cape Town soon spread to other museums. Perhaps the most famous of all the early skeleton-hunters was Frederick W. FitzSimons (1870–1951), director of the Port Elizabeth Museum between 1906 and his retirement in 1937. Although better known for his work on snakes, FitzSimons undertook a large number of excavations of rock shelters along the Eastern Cape coast with the explicit aim of uncovering human skeletons. In the process he did, indeed, unearth a great many skeletons.

Most of his work was undertaken along what he referred to as the Tsitsikamma coast (he used the variant, and now obsolete, spellings ‘T’zitzikama’ and

‘Zitzikama’), roughly the coast between Knysna and Cape St. Francis (Fig. 3.2). He was aided in his undertaking by Whitcher (FitzSimons 1921:880, 1923a:541), the same man who had helped Péringuey and Drury. FitzSimons published descriptions of his work in a series of papers between 1921 and 1928 (FitzSimons 1921, 1923a, 1923b, 1926, 1928). Despite his prolific publication, the quality of FitzSimons’s work was seriously lacking, both in the field and in his publications.

He was primarily interested in skeletons and collected all of those that he found that were robust enough to move. Those that were too fragile were abandoned. He appears to have had limited interest in the cultural material and associations of the burials. He does not seem to have recorded specific details of the majority of the burials he exposed. But, he did keep some notes and take a number of photographs during the process of excavating the various sites (FitzSimons 1921:880, 1923a:541; Schauder 1963:59; Turner 1970:67). The majority of these have not been published. His collection of cultural material was similarly haphazard. He described that he “secured *specimens* of all objects obtainable in these cliff dwellings” (FitzSimons 1923a:541, emphasis added). FitzSimons was first and foremost a herpetologist and his collection of specimens of different types of material is typical of a natural history approach to collection: a representative sample of a supposedly homogenous population needs to be obtained. It is in stark contrast to more recent approaches in which attempts are made to collect all cultural material from a given context.

It is important to note that this is not a retrospective criticism: his work was lacking according to the standards of the day, and he did receive some criticism, albeit mild, from contemporaries. Lawrence H. Wells (1908–1980) and John H. Gear (1908–1974), were the two anatomists who examined the skeletal material from the site they suggested should be called ‘Whitcher’s Cave’ (FitzSimons [1926:813] had referred to it only as located “far up in the Outeniqua range”) (Wells & Gear 1931:444). They commented that “After much time had been spent in attempts to sort out skeletons and reconstruct shattered skulls, owing to unsystematic packing, it was decided, as the time available for the investigation

was limited, to confine attention to the better preserved material” (Wells & Gear 1931:445). It would have been difficult for them to have been more harshly critical than they were: FitzSimons had sent most of the skeletal material he had excavated to Raymond Dart, head of the Department of Anatomy, University of the Witwatersrand, in which Wells and Gear were based.

FitzSimons’s publications were equally unsatisfactory. He did not give detailed descriptions of most of the burials he exhumed. This, however, may have been more the general state of excavational work in South Africa at the time rather than a personal fault. More serious, though, was his failure to record where exactly the sites were, or, indeed how many he had excavated (Turner [1970:67] suggested that nine sites were excavated along the coast, excluding the open sites). The most famous of the sites he excavated are Tsitsikamma Cave (FitzSimons 1921, 1923a), Whitcher’s Cave (FitzSimons 1926), Zuurberg open site (FitzSimons 1923b) and Knysna open site (FitzSimons 1928) (Fig. 3.2). Little is known about the other sites he excavated. His vagueness has spawned a small secondary literature trying to relocate FitzSimons’s sites (Schauder 1963; Woodhouse 1966; Turner 1970; Robinson 1977).

The most intriguing case is that of the site known as Tsitsikamma Cave (FitzSimons 1921, 1923a). FitzSimons (1921:880, see also 1923a:542) described the site only as being “a huge rock shelter a third of the way up the cliff from the seashore in Plettenberg’s Bay. . . . It was beautifully sheltered from the prevailing winds and rains, and a mountain stream of crystal-clear water gushed musically down the cliff near by”. As a result of this rather vague description of the site’s location, Donald Schauder (1963:52–54) and M. Turner (1970:69) both tried to relocate the Tsitsikamma Cave without much success. The most plausible, and startling, contender for the location of the site has been suggested by G. A. Robinson (1977). He argued that the mysterious Tsitsikamma Cave may be none other than the well-known Coldstream Cave (Robinson 1977:98–99). He bases his conclusion on a photograph published by FitzSimons (1921:881) and on a comparison of the physical dimensions of the cave published by FitzSimons and

those he recorded for Coldstream Cave. That FitzSimons did visit Coldstream Cave is suggested by the presence of a large pot from that site amongst his collections in the Port Elizabeth Museum (PEM 125; Turner 1970:70). This item suggests that FitzSimons must have done some excavation work in Coldstream Cave: it seems unlikely that Witcher and Drury would not have collected such a piece if it had been on the surface. The mystery surrounding the site is deepened by another intriguing point: nowhere in his writings does FitzSimons mention either the Coldstream Cave or Drury and Péringuey. He must certainly have been aware of their work and discoveries, especially since the same man, Witcher, who had helped Drury also helped FitzSimons by showing him to sites. Perhaps he did not wish his work to play second fiddle to that of Péringuey.

Despite his lack of rigour in recording his excavations, FitzSimons did make some perceptive observations of the burials he uncovered. He was, for instance, the first to note that graves were dug into the floors of shelters in which people continued to live, although, he phrased it in somewhat more derogatory terms, “these primitive cliff dwellers buried their dead and simply went on living on top of the graves” (FitzSimons 1921:880). He also noted the general form of the burials: corpses were buried in a flexed position sometimes accompanied by a variety of grave goods and often covered with flat stones:

When a cliff dweller died, a shallow hole was scraped in the débris, usually on the inner side of the shelter near the rocky base. The body was doubled up in as small a space as possible, with the knees drawn up to the chest. It was then laid in the hole on its side; a flat slab of stone was placed on the head, and another on the body. Sometimes there was a third on the pelvis. With but few exceptions, the faces of the dead were turned to the east. (FitzSimons 1921:880, see also 1923a:542).

These observations tend to be confirmed by more recent research but have not yet been fully investigated.

The skeletal material that FitzSimons excavated came to play an important role in the development of South African physical anthropology. The reason for this was that he sent much of the skeletal material he collected to Dart, the head of the

newly formed Department of Anatomy at the University of the Witwatersrand, Johannesburg. Dart, and a number of his colleagues and students (most notably, Gear, Laing and Wells), described and published the material (Dart 1923; Laing 1924, 1925; Gear 1925, 1926; Laing & Gear 1929; Wells 1929; Wells & Gear 1931) (Fig. 3.4). The terms of their descriptions, though much criticized now, are important in that they reveal the intellectual climate that gave impetus to the continuing search for skeletons. Their work was embedded in the social circumstances of the time.



Figure 3.4: FitzSimons's skeletal material unpacked at the University of the Witwatersrand Anatomy Department (photo: University of the Witwatersrand Central Archives).

Dart and his students described the skeletons FitzSimons and others sent them in terms of racial types. Most of these descriptions were based on skulls. Each 'race' was seen as a pure and homogenous type. Skulls that did not conform to the racial types were considered to be hybrids between two or more races. This approach was consistent with global trends at the time and was closely linked to Dart's diffusionist views. In this view, race and culture (including material culture) were

inseparably linked. In Dart's scheme, a succession of races had moved down from the north, replacing or interbreeding with previous races. Hybridization between races led to the morphological and cultural differences seen in the skeletal and material remains.

It is easy to see how this 'scientific racism' could lead to intensified collection of skeletons. Large series of skulls (particularly, though not exclusively; other skeletal elements were also used) were needed to determine the basic types of race. Each skeleton could then be confidently ascribed to one or other racial type, or described as a product of hybridization between certain, specified, races. From these racial classifications much could be said about the movements of races and cultures in terms of the diffusionist view. Individual and group history could be inferred from the varied racial influences seen in the bones without recourse to cultural materials.

In retrospect, the flaws in this approach are easy to see. They are both theoretical and methodological (cf. Dubow 1995, 1996). Theoretically, the lack of appreciation of the concept of variation within populations led to the most serious errors. As I have pointed out, Dart and colleagues explained variation as the product of the intermixing of hypothetical racial types. This position assumes that populations of the 'pure stock' are morphologically homogenous (Hall & Morris 1983; Morris 1986:3). They are not. Most populations, including humans, encompass substantial internal variability: populations are heterogeneous, not homogenous (indeed, this is a fundamental tenet of Darwinian natural selection). That human populations do vary internally has now been empirically demonstrated beyond any doubt (in southern Africa, e.g., Tobias 1966; De Villiers 1968; cf. Hall & Morris 1983). There is no such thing as a 'pure stock'.

These theoretical misconceptions were exacerbated by further methodological problems. The specimens used to define particular types were chosen to fit preconceptions of what those types should be like; those that did not fit the preconception were excluded on the grounds that they were the products of

hybridization. “Typological sample selection has resulted in data being representative only of the pre-conceived morphological patterns that the investigator had intended to describe at the outset” (Morris 1987:12). It is, therefore, easy to see why racial types appeared to form such coherent and tightly defined categories. A form of reinforcement occurred between preconceptions and selection of specimens.

These theoretical and methodological flaws were realized early on. By the late 1950s concern over the techniques was growing. In 1958 Ronald Singer published the first critique of the use of racial typologies in southern Africa (Singer 1958). He wrote specifically about what he referred to as the Boskop ‘race’ problem and demonstrated that there are no grounds for retaining the concept of the ‘Boskop race’ (Singer 1958). This fictional race was based on the original discovery of part of a calvarium on a farm near Boskop in the Potchefstroom District, Northwest Province. It too had been found and described by FitzSimons (1915). In his general criticism, Singer (1958:174) pointed out that the “‘races’, species and subspecies are based on what are, relatively speaking, infinitesimally small collections of skeletal material, unearthed in scanty groups or singly over thousands of miles of territory”. He added that,

It is a failing among not a few anthropologists that they feel it incumbent upon themselves sooner or later to plant an evolutionary tree, to construct ancestral branches, to designate apparently unusual features in a skull as ‘primitive’ or pre-this or pre-that, and to plan vast migratory routes of so-called prehistoric ‘races’ which are represented only by odd skulls (Singer 1958:174).

As a result of this and other critiques, researchers no longer seek to describe idealized racial ‘types’, but rather to describe the variation present in the skeletal populations (e.g., Stern & Singer 1967; Rightmire 1970; Howells 1973; Hausman 1980, 1982; Morris 1986, 1987; AAPA 1996). This change in physical anthropology is probably the only example of what may be called a scientific revolution in the study of the southern Cape burials. The other changes have tended to be incremental rather than revolutionary.

The problem of racial typology was further compounded by the fact that morphology seemed to reign supreme. Specimens were grouped together (or excluded from groups) on the basis of morphology irrespective of from where the material came. Whilst on the one hand this approach grouped together skeletons from different sites (there is nothing intrinsically wrong with doing so), it also separated skeletons from the same layers of the same site. This second situation of privileging morphology over context led to the ridiculous state in which individuals who were likely contemporaries were ascribed vastly different racial and cultural histories. It may, however, explain the lack of concern of many early workers for the contexts from which the skeletons were removed. They were secondary to the intrinsic morphological data.

John Hewitt

At about the same time as FitzSimons began his investigations of Eastern Cape Province rock shelters, another museum director started a similar programme. John Hewitt (1880–1961) took up his post as director of the Albany Museum, Grahamstown in 1910. Like FitzSimons and Péringuey, his formal training was in zoology, yet, he too had an interest in prehistory. Hewitt, along with various friends and colleagues, excavated a great many sites, most of them rock shelters. By 1921 he had excavated, amongst others, the sites at Glen Craig, Bergplaats, Spitzkop, Wilton Large Rock Shelter and Wilton Cave (Hewitt 1922; Binneman 1990:13) (Fig. 3.2).

His excavations at Wilton Large Rock Shelter were particularly important. Based on the materials that he recovered from this site, he described a ‘pygmy industry’ that he called the ‘Wilton Culture’ (Hewitt 1922; see J. Deacon 1972 for a re-assessment of the site). The name ‘Wilton’ was formally adopted by Miles Burkitt, Goodwin, and Clarence van Riet Lowe at a meeting of the British Association in Pretoria in 1926 (Sampson 1974:292). The Wilton Industry (formally named thus by Goodwin & van Riet Lowe [1929]) has proved to be

particularly significant in Later Stone Age research. It is now considered to be an industrial complex with Coastal and Interior regional variations, and Early, Classic, Post-classic and Ceramic temporal variations recognized (Sampson 1974; see Mitchell 1997, 2002; Wadley 2001 for recent reviews). Unlike the original definition based on a *fossile directeur* approach (the backed crescent, now usually referred to as a segment, was considered as the distinctive marker of the Wilton) the industry is now understood in terms of quantitative variations (Parkington 1984b). The Wilton Industrial Complex is characterized as a microlithic assemblage containing many standardized small scrapers and backed microliths with an emphasis on the use of fine-grained raw materials (Mitchell 1997).

A clear difference between the work of Hewitt and FitzSimons is evident. Whilst FitzSimons was interested, primarily, in human skeletons and paid scant attention to material culture, Hewitt paid a great deal of attention to the material culture. Although he uncovered a number of human burials in the course of his excavations, the skeletons themselves were not his primary interest. He provided some description of the burials he uncovered (more, certainly, than FitzSimons) but devoted most attention to description and analysis of the material culture he discovered.

Hewitt continued his archaeological work in the Eastern Cape until 1934 (he published two, late, brief notes on archaeology in the 1950s; Hewitt 1954, 1955), excavating a series of important sites, mostly to investigate variations in stone tool industries. The more famous of the sites he investigated include Kabeljous River Mouth (Hewitt 1925), Howieson's Poort (Stapelton & Hewitt 1927, 1928; see Deacon 1995 for a re-assessment of this site; Lombard 2005 for a review of the industry), Melkhoutboom Cave (Hewitt 1931; more recently see Deacon 1969), Vygeboom and Uniondale Rock Shelter (Binneman 1990; Uniondale Rock Shelter has been re-excavated by Leslie-Brooker [1987]) (Fig 3.2). He excavated more than twenty rock shelters in the two decades he worked in the Eastern Cape (Binneman 1990). Several of these sites, particularly Wilton Large Rock Shelter

and Howieson's Poort, proved to be critically important in defining lithic industries. Other sites, unfortunately, were never published.

John Goodwin

The last of the men to play a particularly important role in the early development of the study of Later Stone Age burial practices was A. John H. Goodwin (1900–1959). He was in a quite different position to the pioneers so far discussed. For one thing, he was based at a university (the University of Cape Town) rather than at a museum. He was also somewhat younger than the others. Most importantly, though, he was the first, and at that time only, professionally trained archaeologist in the country (although he was initially employed as a research assistant in ethnology in the Department of Social Anthropology under the famous social anthropologist A. R. Radcliffe-Brown). Though a South African by birth, he read his degree in archaeology at the University of Cambridge under Burkitt and Haddon (of British Association fame). His training in archaeology gave him a very different perspective in approaching the problem of investigating the prehistory of a largely unknown region.

Goodwin's contributions to South African archaeology were many, and these have been well chronicled elsewhere (e.g., Deacon 1989, 1990; Shepherd 2003). Of particular importance for this study was his general contribution to the professionalization of the discipline. He, along with van Riet Lowe, published *The Stone Age Cultures of South Africa* in 1929. In it they propose a three-stage division of the southern African Stone Age into Earlier, Middle and Later phases (Goodwin had presented an earlier two-phase model in 1925 [Goodwin 1926]). These divisions still stand (despite some challenges, e.g., Sampson 1974). An important aspect of their classificatory scheme, besides clarifying the South African lithic sequence, is that it made a terminological break from the dominant European scheme of Lower, Middle and Upper Palaeolithics which had, until that time, been erratically employed in South Africa (Deacon 1990; Schlanger 2003;

Shepherd 2003). Another important aspect of *Stone Age Cultures* is that it employed terms such as ‘cultures’ and ‘industries’, rather than, as we have seen, the then more common term ‘race’. Saul Dubow (2003) suggested that it was the influence of social anthropology that caused Goodwin to take this stance. Goodwin also promoted more rigorous field techniques, particularly in his two handbooks *The Loom of Prehistory* (1946) and *Method in Prehistory* (1953).

He excavated a number of sites throughout the Western and Eastern Cape Provinces. The most significant, for the purposes of this study, was Oakhurst Shelter near George (Fig. 3.2) which he excavated between 1932 and 1935 (Goodwin 1938a, 1938b, 1938c, 1938d). What he found in this site forms the subject of the detailed case study in Chapter 8. That his material can today still be used for a detailed study of this sort is a tribute to his field practices. Indeed, Mary Leakey, who visited Goodwin at Oakhurst Shelter, later said that his excavation techniques were “way ahead of his time, and I was deeply impressed by his care and minute attention to detail in everything” (Leakey 1984:51). Unlike others excavating in the 1930s and 1940s, Goodwin worked with extreme care, taking note of stratigraphy and spatial relationships (Goodwin 1938c) and recording the details of all the individual graves he found (Goodwin 1938b) (Fig. 3.5). An indication of his care can be gained by comparing the four years it took him to excavate Oakhurst Shelter with the few days taken by most of his contemporaries in other sites of equal complexity. Most of the burials he excavated were individually described and photographed; some of the better preserved examples were filmed in time-delay throughout the process of excavation (Goodwin 1938a:232). Such care is in stark contrast to the cavalier manner of excavation employed by FitzSimons and others and provided richer rewards:

The ample scientific return for very careful brushwork in excavating is seen in the fact that for the first time the exact disposition of grave furniture could be observed, and even the arrangement of eggshell beads on the body could be seen *in situ*. In one instance a complicated woven bead bracelet was photographed in position and the whole wrist preserved in a wax bed. It was also possible to observe the distribution of red ochre on the bones and surrounding earth (Goodwin 1938a:232).



Figure 3.5: Goodwin's excavation at Oakhurst. Note the brush and the manner in which the burial is exposed (photo: University of Cape Town Libraries).

Goodwin was intentionally setting-up his careful, professional excavation work in contrast to the amateur work of others. He established his lithic sequence at Oakhurst Shelter in direct opposition to the other long sequence in the area, that at Matjes River Rock Shelter. His work at Oakhurst Shelter, besides demonstrating the marvellous details of the burials, reiterated his call (Goodwin 1935) upon his contemporaries to take a more rigorous approach to their excavations:

The habitual use of more and more exact methods in excavation will mean that the maximum of knowledge will be acquired with a minimum of disturbance to sites. The common "rooting over" of large numbers of deposits by students inevitably results in the destruction of evidence on a large scale. It is essential that those deposits which have remained untouched should be excavated with the intention of extracting every possible piece of evidence, or should be left utterly alone until such time as reasonable excavation can be undertaken (Goodwin 1938a:232).

Despite Goodwin's care, there is always room for retrospective criticism. Brian Fagan (1960), after examining the excavated material from Glentyre Shelter and Oakhurst Shelter, argued that Goodwin had been mistaken about the sequence of

lithic industries at these sites. Carmel Schrire (1962), however, after also re-examining the material defended Goodwin's original division of industries.

The skeletal material excavated by Goodwin was analyzed by the University of Cape Town anatomist Matthew Drennan (1938a, 1938b). He approached the material using the same racial typological approaches as Dart and his students had used in Johannesburg.

I discuss details of the Oakhurst Shelter burials in Chapter 8.

The period of the collectors

If we leave aside the various factors that motivated the early workers to excavate southern Cape sites, we can see that considerable similarities emerge in their approaches to the projects and the ways in which they dealt with the material they collected. The primary reason, in most cases, for excavating sites was to *collect* material. The majority of this material was human skeletal remains and lithic artefacts. Little, if any, attention was paid to the contexts of the material collected (Goodwin was an exception). The reason for this selection was that it was the objects themselves that were of interest. Commenting specifically on FitzSimons and Drury, Morris (2000:75) made the point that “this was not archaeology in any sense, and looking back over 80 years we now realize that FitzSimons and Drury were destroying history in order to obtain type specimens for their racial concept of anthropology.” Early workers believed that the racial typologies they used allowed them to reconstruct the history and biology of the people they studied from the skeletal specimens themselves. Context was not necessary for these reconstructions, and, indeed, was often contradictory to them. Similarly, lithics were seen as representing fairly homogenous (at least originally so) cultures that were closely linked to equally homogenous races.

Within this sort of paradigm, objects—bones and stones—can tell their own stories without the need for contextual information. This paradigm, in part,

accounts for the poor state of excavations: the researchers' purpose was to recover objects, not to record contexts and spatial relationships. The way in which material was collected can be accounted for in the same way. Representative *specimens* were needed. Nothing could be gained by collecting and analyzing all the cultural material. As I have pointed out, this form of collecting is also very much in line with the natural history mode of collection: representative samples of species are collected to fill museum cabinets.

The purpose of this early research was, then, to describe material (cultural and skeletal) and to construct typologies. Researchers had no interest in the social processes that led to the accumulation of the deposits. Burials were excavated to collect skeletons; burials themselves were of no real interest.

The war years

The 'middle years' of research on southern Cape burials, between roughly 1940 and 1980, were relatively quiet compared with the frenzy of the early years. Fewer people excavated shelters and fewer publications were produced (Fig. 3.1). The initial downturn in research probably related to the outbreak of the Second World War. Shortly after the war, in 1948, the National Party was elected (by the white minority) to power in South Africa. Changes in funding policy that they implemented probably also contributed to the reduction in research (Deacon 1990; M. Hall 1990). By contrast, the preceding government, reflecting the interest of the premier, Jan Smuts, had been very supportive of archaeology (Deacon 1990; M. Hall 1990; Schlanger 2002).

The one major project that was undertaken during this period was the excavation of the Matjes River Rock Shelter (Fig. 3.2). The excavation was initiated by Dreyer (1885–1954), a zoologist and one of the delegates who probably attended the British Association meeting in 1905. At the time, he was based at Grey University College (now University of the Free State), Bloemfontein, but he had

strong ties to the recently formed National Museum, also in Bloemfontein. Dreyer discovered the Matjes River site in 1928 and began large scale excavations there the following year (Louw 1960:15). Although work began at the site in the late 1920s, it continued, on and off, until 1960 with next to no changes in excavation techniques. For this reason, I include the work at Matjes River Rock Shelter in my section on the middle years of research, rather than in the earlier period when it began and of which it was so unfortunately characteristic.

Dreyer's excavation was, in many ways, very similar to those of FitzSimons: they were on a massive scale, with very little vertical or horizontal control. (There was some vertical control: skeletons were marked as coming from broad layers many feet thick.) He was also, primarily and explicitly, interested in the recovery of skeletal material rather than the context of the burials or the associated archaeology (Dreyer 1934). Cultural material was seen as a way of relatively dating the burials (Döckel 1998:19). The skeletons Dreyer recovered were described in terms of racial types (Keith 1934; Dreyer 1936; Meiring 1937), very much as FitzSimons's were (Fig. 3.6). All of this work was well in line with practice in the early days.

What makes Matjes River Rock Shelter different is that work at the site was continued by Abraham Carl Hoffman (1958), J. T. Louw (1960) and Reiner Protsch and J. J. Oberholzer (1975) well into the middle period of research that is under discussion. Whilst there is nothing intrinsically wrong with the continuation of the project—indeed, a long-term project of this sort would normally be considered laudable—problems arose in the manner in which the project was continued. The same 1930s approach to excavation, curation and analysis of material that Dreyer and his contemporaries employed was continued by those following him at the National Museum. Clearly, the new generation of workers at Matjes River Rock Shelter, none of them professionally trained, did not heed Goodwin's call for more exacting field techniques.



Figure 3.6: Models of Dreyer's racial types on display in the National Museum, Bloemfontein, October 2005. From left, Florisbad Man, Proto-Bushman and Bushman (photo: S. Moodley).

The major publication on the site was Louw's (1960) *Prehistory of the Matjes River Rock Shelter* published as the first *Memoir of the National Museum, Bloemfontein* (it was also his DPhil thesis, submitted, and approved, by the University of the Orange Free State). It was slated by reviewers. Singer's review of the publication, which chiefly considered physical anthropology, began thus:

It is a great pity that Mr. Louw did not receive better guidance during the preparation of his Ph.D. thesis, because there are signs on practically every page of an almost utter unfamiliarity with the relevant literature. A thorough review would entail criticizing almost every paragraph and rewriting the entire text (Singer 1961:29).

In a companion review from an archaeological perspective, Ray Inskeep was equally scathing. He began:

Many of us have been waiting, almost with bated breath, for the definitive report on the Matjes River Rock Shelter to appear; now that it has arrived it would be difficult to imagine a more disappointing result. That such a piece of work should have been awarded a Ph.D. degree is a serious reflection on the present state of archaeology in the universities of South Africa today (Inskeep 1961:30).

Singer and Inskeep were not being overly critical. Later researchers with access to the collection have concurred (Sampson 1972, 1974; Döckel 1998). Certainly from the point of view of this thesis, Louw's publication and indeed most of the published accounts and surviving museum records contain, as I found, almost no useful information. Most critically, as Inskeep (1961:30, original emphasis and parenthesis) noted, "*The position of not a single grave or skeleton is indicated either on the plan or the sections (p. 19), nor are we told anywhere in the publication how many burials were found.*" Rumour has it that there were 'hundreds'." Without these crucial data the many burials from Matjes River Rock Shelter are of very little value for cultural studies. It is still unclear exactly how many burials were excavated. There are 121 catalogue entries for Matjes River Rock Shelter skeletons (Morris 1992a:54–59). This number, however, is deceptive. Part of the problem is that many of the remains were (incredibly) sorted and stored by skeletal element rather than as complete skeletons (Morris 1992a:54–59). One must concur with Inskeep's (1961:31) conclusion that "we know really very little about the archaeology of the Matjes River Rock Shelter."

Fortunately, in 1993, a project was begun to conserve the site. It allowed some re-investigation of the deposits and has given us some knowledge of the stratification and contents of the deposits (Döckel 1998). Another project to date skeletons from the site has provided further chronological and sequential resolution (Sealy *et al.* 2006). Details of the burials will, however, remain lost forever.

Despite the tragic and highly reprehensible state of affairs at Matjes River Rock Shelter, the middle years were overall a period of increasing standards in South African archaeology. Largely as a result of Goodwin's (and later Inskeep's)

efforts at the University of Cape Town, increasing numbers of professionally trained archaeologists were beginning to excavate. Excavations were generally better controlled and recorded. Another important change was that most excavations were then not being conducted for the prime purpose of recovering skeletal remains. Other questions were being asked and answered. Some burials, however, continued to be discovered in the course of other excavation projects (e.g., Cape St. Francis: Chappel 1968; Cairns 1975; Thackeray & Feast 1974; Die Kelders: Schweitzer 1979; Gamtoos Valley: Deacon 1965). The anatomical descriptions of these skeletons (post-1958) had taken note of the earlier critiques, and no longer made use of racial typologies (e.g., Cape St. Francis: De Villiers 1974; Gamtoos Valley: De Villiers 1965).

Science and the social

The most recent set of studies of the southern Cape burials, beginning in the 1980s and continuing to the present, has been marked by two different, yet important, approaches: isotopic studies and social studies.

The science of isotopes

The first isotopic study of skeletal material in South Africa was undertaken, in 1979, by Francis Silberbauer on specimens from the southern Cape in the collection of the Albany Museum, Grahamstown (Silberbauer 1979). It was not, however, until the 1980s that isotopic analyses came to prominence. These studies were led by Sealy and Nikolaas van der Merwe at the University of Cape Town.

Most of the early isotopic work was undertaken in the Western Cape Province. Although it is outside the geographic area of interest to this thesis, I briefly review the studies because they provide principles that are relevant to the more recent research in the southern Cape.

In the 1970s, John Parkington, then a student of Eric Higgs at Cambridge, introduced a seasonal mobility model to the Later Stone Age archaeology of the Western Cape Province. He proposed that hunter-gatherers in the Western Cape Later Stone Age moved seasonally between sites, such as Eland's Bay Cave, on the west coast and sites in the inland folded mountain belt, such as De Hangen (e.g., Parkington 1972, 1976, 1981, 1984a). He argued this on the basis of excavated, seasonally available food remains in the respective sites.

Sealy and van der Merwe (1985, 1986) undertook studies of stable carbon isotopes in skeletons from the two sites as well as marine and terrestrial plant and animal foods within the three parallel ecozones across which Holocene hunter-gatherers were supposed to have moved. On the basis of their studies they demonstrated that the coastal and inland skeletons were isotopically different. They argued that this isotopic difference meant that the two sets of skeletons came from two separate (or at least largely separate) dietary populations. The implication of their argument (in contradiction to Parkington's) was that Holocene hunter-gatherers had not moved seasonally between the coast and the mountains.

Parkington (1986, 1987, 1991, 2001) responded to the isotopic critique by questioning details of the technique and the physiology of carbon metabolism. In short, he argued that many different diets could lead to the same isotopic signature in human bone collagen.

Continuing isotopic studies have answered many of Parkington's criticisms by refining carbon isotope studies (Sealy & van der Merwe 1988; Sealy *et al.* 1995; Corr *et al.* 2005), and adding the analysis of stable nitrogen (Sealy *et al.* 1987) and strontium isotopes (Sealy *et al.* 1991; Bentley 2006) to the repertoire of discriminatory isotopic techniques. The analysis of stable isotopes in human bone is a technique now firmly established around the world for the study of past human diet.

Isotope studies of skeletal material have only recently been extended into the southern Cape (excluding Silberbauer's [1979] early study). Sealy and Susan Pfeiffer (2000) analyzed eighty skeletons from the southern Cape coast for both stable carbon and nitrogen isotopes. Importantly for this thesis, they also radiocarbon dated all of the specimens (Sealy & Pfeiffer 2000:table 1). On the basis of differences in skeletal isotope values between sites, they suggested that some territorial divisions are visible in the southern Cape. The largest discrepancy is between specimens from Matjes River Rock Shelter and sites on the Robberg Peninsula. Unfortunately, only a few specimens from these sites were included in their analysis. The differences in isotope values between these two sites has subsequently been further investigated and confirmed by Cecilene Muller (2001) and further work by Sealy (2006). The significance of these important findings is explored in Chapter 10.

The only other isotope study so far reported in the southern Cape has examined weaning patterns during the Later Stone Age (Clayton 2002). This study also made extensive use of the skeletal material available from Matjes River Rock Shelter.

Social studies

Inskeep, Goodwin's successor at the University of Cape Town, was the first writer to consider seriously the question of Later Stone Age burial practices. He discussed them in his survey of the Orange River and Cape coastal regions (Inskeep 1986). As he himself pointed out "no one has yet examined the question of whether or not data on burials can contribute usefully to our general knowledge of the past" (Inskeep 1986:222). He reviewed the ethnography on burials amongst both San hunter-gatherers and Khoekhoen herders (Inskeep 1986:222–226). He then attempted to correlate the ethnographically and archaeologically known burials (Silberbauer [1979:56] had previously attempted this in a limited way). This approach proved effective only in the Northern Cape Province, along the

Orange River, where he was able to distinguish herder from hunter-gatherer burials (Inskeep 1986:226–228).

In the Cape coastal regions, on the other hand, most burials seemed to be hunter-gatherer in origin (Inskeep 1986:236). In the southern Cape (a combination of Inskeep's southern and southeast Cape regions) Inskeep (1986:229–235) usefully divided the burials into three phases using a combination of absolute dates and relative stratigraphic positioning. He called his phases the 'early' burials, 'Wilton' burials and 'late Holocene' burials. He was, unfortunately, hampered by the paucity of direct dates. The numbers and types of grave goods seemed to change between phases, reaching their peak in both number of individual items as well as variety during the Wilton burials. Inskeep's sequence is extremely important and, strangely, has not received much attention since he proposed it. I discuss it in more detail in Chapter 9.

In the following year, Simon Hall and Johannes Binneman (1987) addressed another important issue. They attempted to explain why some burials from the same time period, and indeed from the same site, were 'richly' endowed with grave goods, whereas others have none at all. They discussed in particular the burials they had excavated in Klasies River Mouth Cave 5, The Havens Cave and Welgeluk (Fig. 3.2). They noted that the burials of infants and children at those sites were exceptionally elaborate, containing large quantities of grave goods, whereas those of adults had few grave goods, and in many cases, none at all. They related by analogy this inequality in grave good distribution to a form of gift exchange similar to the *hxaro* system that Polly Wiessner (1977, 1982) recorded in the Kalahari amongst the Ju/'hoansi (Hall & Binneman 1987:148–149). At that time *hxaro* was assuming importance in archaeological explanation (see also Wadley 1987).

The crux of their argument was that:

individuals who die well after their prime *hxaro* years will no longer have the material possessions for grave goods while those unfortunate to die at a very young age may have had the possessions because the reciprocal side of their exchange relationships have not yet been formalized (Hall & Binneman 1987:149).

Their suggestion, then, was that the ‘richness’ of the child burials represented the unfulfilled exchange-potential of those individuals. Conversely, the lack of grave goods in the burials of the elderly is evidence of the life of exchange led by the individual.

Hall and Binneman’s argument is important in that it is the first to draw on social factors to explain the elaborate burials. They posited the *hxaro* delayed exchange network as the mechanism for the accumulation of large numbers of grave goods. *Hxaro* is a Ju/’hoan word to describe a particular kind of delayed exchange relationship (Wiessner 1977, 1982). Lyn Wadley (1987; see also Mazel 1989) adapted the concept of *hxaro* into archaeology and used it as part of a model of Later Stone Age aggregation and dispersal.

Mitchell (2003) has strongly criticised the use of *hxaro* in archaeological explanations on two grounds. First, he pointed out that *hxaro* is by no means universal amongst San groups (Mitchell 2003:37–38). Amongst other extant San groups, only the Nharo have a similar system, which they call *//ai* (Barnard 1992). A number of other forms of exchange do, however, exist. Secondly, he reminded us that in very few archaeological cases has *exchange* been demonstrated; certain classes of goods (ostrich eggshell beads, for instance) are usually just assumed to be evidence for exchange (Mitchell 2003:38). A further, empirical, point worth considering is that whilst Hall and Binneman’s (1987) argument appears to explain the burials in the sites they discuss, it does not for many others. There are many recorded child burials with few, if any, grave goods, and, conversely, some elaborate adult burials. The ‘social’ emphasis in explanations of this kind has focused on objects that may be seen as evidence of social relations. Importantly,

this focus deflects attention from the symbolism of graves. In light of these cautions, it will be necessary to re-evaluate Hall and Binneman's (1987) suggestion.

A decade after Inskip's seminal work, Wadley (1997) gave a second general overview of Later Stone Age burial practices. In her work, the influence of the contemporary political emphasis on gender inequality can be clearly seen. She focused on those aspects of social relations concerned with gender relations. In her study, she therefore concentrated on dispelling the seemingly widely held belief that burials reflect the life-roles (and more specifically the life-gender-roles) of the deceased. To do this, she reviewed the grave goods commonly found in southern African Later Stone Age burials and searched the ethnographic literature for reasons why those items associated with a particular sex during life should be either included or excluded from graves. She concluded that the life gender-associations of items did not carry through to the grave (Wadley 1997:129). The implication of such a finding is significant, because it strongly suggests that 'gender issues' of the sort that concern feminist scholars were of little importance to the Later Stone Age people who constructed the graves; it seems that other, overriding, factors determined which items furnished graves.

In suggesting reasons why some goods were included in graves, Wadley (1997:127–128) pointed out that certain grave goods are known to be associated with 'medicine people', 'healers' or 'shamans'¹. She explained the inclusion of these items in the burials by equating literal death that leads to burial with 'trance death' that results in temporary transition to the spirit world (Wadley 1997:128; see also Lewis-Williams 1984).

Despite these innovative and useful interpretations, Wadley's work suffered from a methodological flaw. She assembled her sample of specimens from all over southern Africa without subdividing them geographically or temporally, as Inskip so tellingly did. In terms of the southern Cape material, this is a serious error. Burials from this region are far more numerous than those from other parts

of the sub-continent (thus biasing the sample); they are also substantially and obviously different. The effect of conflating burials from different regions was twofold. First, the rich southern Cape burials were extrapolated to the whole of the southern African Later Stone Age. Secondly, the regional specificity of the elaborate burials was lost. They no longer appeared as a geographical anomaly in need of a specific explanation—the focus of this thesis. Similarly, conflating burials from different time periods ignores the important changes that Inskeep (1986) identified.

The most recent study, by Hall (2000, see also S. Hall 1990), attempted to fit the southern Cape burials into the broader regional temporal sequence. As in his previous study (Hall & Binneman 1987), he took a social approach. He considered specifically the burial complex at Welgeluk. Initially this site was used only as a burial ground—the bodies were laid on bedrock and covered with a cairn of rock slabs (Hall 2000:140). There was no occupational deposit at that time. A nearby site, Edgehill (Fig. 3.2), was at that same time a living site, but had no burial remains.

Hall (2000:140–141) explained the differential use of sites in terms of the bi-axial model of the San cosmos proposed by Lewis-Williams (1996:124–126, fig. 1). Hall (2000:140) argued that Edgehill was the ‘camp’ site. Welgeluk, on the other hand, overlooks a large pool, and this location placed it in the ‘water’ position, the point of crossover from the horizontal to the vertical axis. Such would have been an ideal place to put the dead.

Hall elaborated his argument by pointing out the overt and repeated ‘water’ symbolism of the Welgeluk grave goods (Hall 2000:141–142). First, a turtle carapace was included in one of the burials. The second particularly significant item is a warthog tusk included in another of the graves. Hall pointed out that the behaviour of this animal relates it to water: it lives in abandoned antbear holes (i.e., underground) and frequently wallows in mud.

At a later period, the situation at Welgeluk changed: it became a living site, and the burial cairn was slowly covered by occupational deposits (Hall 2000:143). Hall related this change to an increase in population and resource stresses. The 'place' and its burials were reconceptualized (Hall 2000). The dead were used by the living as a 'link' to the area and its resources.

Conclusion

This broad overview brings out significant changes in archaeologists' approaches to the past in general and, more specifically, to the high concentration of Later Stone Age burials in the southern Cape. Sudden, Kuhnian revolution cannot be detected, but we should remember that he also wrote of 'revolution by degrees'. This is what seems to have happened in Later Stone Age research during the twentieth century. And, as Kuhn would have been the first to point out, the changes were most importantly related to the South African conceptual and political context. This relationship becomes especially clear when we recall the astonishing continuation of research concepts and techniques that the National Museum researchers kept alive at Matjes River Rock Shelter long after such concepts and techniques had been superseded in the wider national context. That these researchers ignored theoretical and technical developments in other parts of the country, and indeed, internationally, reveals a parochialism born out of a specific political environment.

¹ The word 'shaman' and the related 'shamanism' are vexatious terms. Many critics baulk at their use, claiming that the terms should not be used outside their original Siberian setting. Such a view is baffling considering that "the concept of *shamanism* has always been an externally imposed construction, and does not exist anywhere at all other than in the minds of its students" (Price 2001:6). In elaborating this statement, Neil Price (2001:6) pointed out that "[a]s both a term and a notion, shamanism is entirely an academic creation, and as such is certainly a useful tool serving to describe a pattern of ritual behaviour and belief." However, because the terms generate a response from some critics that obscures substantive arguments, I do not use them when referring to archaeological contexts. I instead use the phrase 'religious specialist'. In doing so, I acknowledge that the individuals concerned were probably not specialists in the sense that they performed religious tasks to the exclusion of other activities, but rather that they had specialist knowledge about religion.