
Chapter 2

Paul Anthony Mellars, from Swallownest to Cambridge: the Early Years¹

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When asked to describe Paul Mellars, James Sackett replied 'He's the kind of guy I would want standing next to me in an Anglo-Saxon shield wall.'²

Childhood

'My father had an allotment garden in our small village of Swallownest near Sheffield' Paul Mellars recalled in our first interview in 2006. 'One evening, he came back with a Charles II copper coin found in the potato patch. That was about the most exciting thing I had ever seen ... I became really interested and started reading Grahame Clark, including *Prehistoric Europe, the Economic Basis* and *Archaeology and Society*.'³

Paul was fortunate in his parents. His father, Herbert Mellars, was a member of the religious sect known as the Plymouth Brethren and began his work-

ing life as a coal miner, a penalty imposed for taking a religious stance as a conscientious objector to fighting in the Second World War. Although Paul broke from his father's strict religious faith as a young man, he nevertheless inherited a steadfast commitment to basic truths. Within his father's faith, there was clear right and clear wrong. Paul grew up with an elementary sense of basic definitive choices; this seemed to contribute to forming a character often described by colleagues as straightforward and remarkably staunch and consistent in interests and behaviour.

Paul matured into an uncomplicated 'straight bloke' with robustly defined and steadfast views in

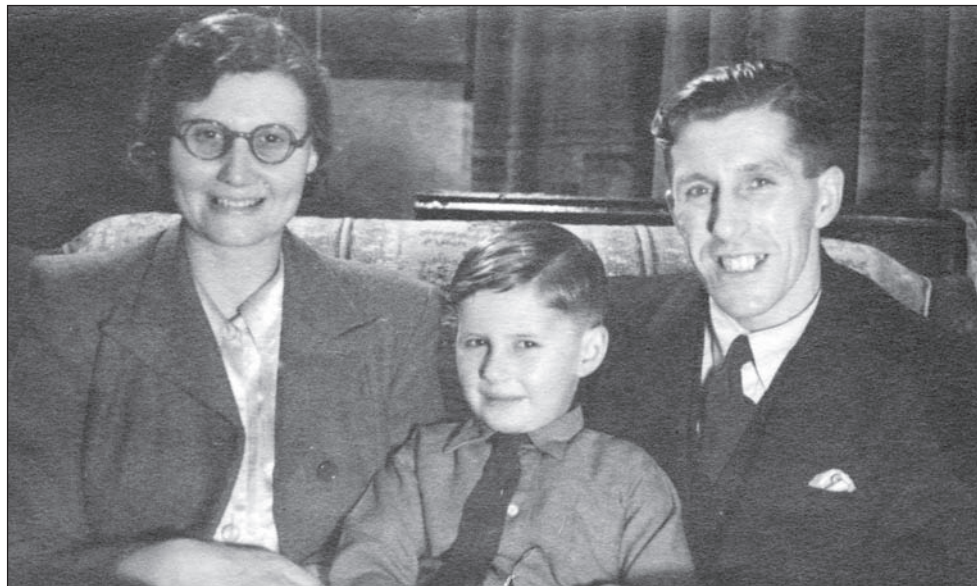


Figure 2.1. Paul Mellars, born 29 October 1939, pictured here with his parents; Paul was sturdy, cautious, determined and bright. 'My mother would say that I never took unnecessary risks. I would carefully consider all options'; and then presumably look for 'the big picture',⁴ the '64 thousand dollar question',⁵ light a cigar and synthesize the available literature. (Photograph courtesy of Mr Herbert Mellars.)



Figure 2.2. *Paul's maternal grandparents, Ernest and Etheldred Batty photographed during the First World War. In addition to contributing natural cleverness, his family supported Paul's academic pursuits. For example, Harvey Bricker, Professor emeritus at Tulane University, remembers Paul's family loaning their car for a summer expedition to Scottish Obanian sites in 1963.⁶ Years later, Paul used the knowledge thus gained when choosing to excavate Oronsay. (Photograph courtesy of Mr Herbert Mellars.)*

which there is little room for intense, ill-defined subtleties. Our species would be seen by Paul as upright and definitively intelligent with clear boundaries, rather than essentially nuanced. With a clean definition, we are rational, thinking, moral beings. As an academic, Paul would support strong arguments and stick with the steady accumulative development and expansion of certain central themes of knowledge. His father clearly believed in God; Paul, a 'fundamentalist agnostic'⁷ would be equally passionately, simply and solidly

committed to definite archaeological views. Paul seems to have inherited his father's strong character.

Coming from a modest family, Paul attended the village school. Growing up surrounded by a variety of social classes, he was still aware of those who were less fortunate. Paul realized that some could barely afford proper clothing. He was one of the brightest boys in his school and sometimes bullied and, therefore, delighted, after the 'eleven plus' examination, to qualify for and move up to Woodhouse, an excellent West Riding County Council Grammar School where he thrived. Founded in 1909 to supply free, quality education, Woodhouse had 'a good mix of social classes all judged to be capable of an academic education'.⁸

Paul's mother, exceptional for her era, had also attended Woodhouse when women seldom had that opportunity. Although Paul speaks less frequently of his mother than his father, she also was a strong supporter of his education. In 1965, she was the one who spotted the newspaper advertisement for what became Paul's research fellowship in 1965 at Sheffield University where archaeology had just begun with Warwick Bray as the first Lecturer.

Paul enjoyed Woodhouse, had inspiring and well-educated teachers, spent his teenage years specializing in science and maths and was eventually appointed Head Boy or Head Prefect which is a position of distinction and considerable responsibility within the British school system. He was certainly the first in his family to be interested in archaeology, 'I don't think they knew the word!';⁹ with fellow Woodhouse student and long-term friend, Jeff Radley, Paul collected flints at the local Mesolithic site of Hail Mary Hill and with the Headmaster's encouragement, he studied A-Level archaeology without a tutor. None other than Leslie Armstrong examined his essay on the Hail Mary Hill site and came to his school to examine the finds; Armstrong was, at that time, famous for his excavations at the well-known Upper Palaeolithic site of Creswell Crags just 20 miles from Paul's home.

And, with the Latin Master, Paul founded the first Woodhouse Archaeology Society, took part in weekend excavations and at the age of 17 published the following mission statement extolling 'Archaeology as a Hobby' in the school magazine, *Woodnotes*. 'Even the richest of unearthed relics is, to the archaeologist, only as important as the new light which it throws on the life and times of the people who made it.' And when discussing the value of amateur discoveries, he wrote, 'I have found truth in the statement "Seek and ye shall find".' (Mellars 1957/8, 27-8).

By the time Paul left Woodhouse in 1958, he had already discovered Palaeolithic and Mesolithic

archaeology, had conducted his first excavations, had published his first article and was obviously 'mad keen on archaeology'.¹⁰ He would go on to become the first in his family to attend university.

People had said that I would never make a living in archaeology and advised me to do something useful. "Get a job and then do archaeology in your spare time." I thought that the pure sciences sounded a little bit boring; my mother's cousin was married to an engineer so I thought, "Why don't I do civil engineering?"¹¹

Paul applied to Leeds University and UCL, was offered places in civil engineering at both and started attending engineering lectures at UCL in October 1958. 'They were the most boring things I had ever heard.' Every afternoon he would skip classes in drawing and go 'around all the second-hand bookshops, buying archaeology books', reading avidly. And, to add 'to the depression ... in order to get to the Engineering Department, I had to walk daily past the Institute of Archaeology in Gordon Square which had recently been built'.¹² Within weeks, Paul had left engineering and returned to Sheffield.

During his 'gap' year, Paul took a temporary post as a schoolteacher, learning a skill and trade that served him well throughout his later career. 'I then spent about six months teaching nine- to eleven-year-olds in a mining village near Sheffield. I was teaching these little mining kids in an all-age school. That was probably the hardest work that I have ever done.'

The experience paid off. Paul's ability as a teacher is now legendary at Cambridge. Among numerous other accomplishments, he was nominated twice for a University Teaching Prize, has supervised over 40 PhD candidates, has been an external examiner at 18 British and foreign universities, has been Director of Studies for Corpus Christi College for over 25 years and is one of the few professors in Britain with a dedicated Facebook, 'The Professor Paul Mellars Big Love Society' for 'those who appreciate the archaeology god/genius that is Professor Mellars'.¹³ This site is set up by undergraduates who admire Paul's charms and who are thankful for his abilities to present organized, judicious, easy-to-grasp clear lectures. As Tim Reynolds, Paul's first 'home-grown' Cambridge PhD student, recently stated, 'As a lecturer for undergraduates, Paul was brilliant and had excellent technique; he stated what he was going to say, said it and then summarized what he said; everyone understood his points'.¹⁴

During 1958–59, Paul also worked as a temporary archaeological assistant at the Sheffield Museum, excavating at Ash Tree Cave, and was encouraged by the Deputy Director, Stanley West, to apply to Cambridge to read archaeology; at that time, Cambridge was one

of the few universities offering an undergraduate degree in the subject. West wrote to Glyn Daniel who suggested that Paul 'bombard the colleges for entry now [this was July/early August], I might just get a place. I wrote out a screed about who I was, why I was interested in archaeology and what I had done, addressed to every Senior Tutor in Cambridge' and was miraculously offered an interview at Fitzwilliam in early September, three weeks before term. 'So, that is how I got into Cambridge ... by the skin of my teeth'.¹⁵

The early Cambridge years

'There were two kinds of people who came up to Cambridge in 1959,' Paul remembers,

There were those who had done National Service. They were the men. And there were those who came up from school and they were the boys. Also, public school men dominated Cambridge and, having come from a northern grammar school, I found this all rather intimidating. I was pretty low-key. I must have been rather inward-looking and felt a bit gauche.¹⁶

When Paul came up to Fitzwilliam House in Cambridge University in October, 1959, he entered what was to become one of the most illustrious classes ever to graduate from Archaeology and Anthropology; Barry Cunliffe 'Mortimer Wheeler's protégé',¹⁷ Charles Higham, 'the great rigger player' and Paul were awarded Firsts in Part I and Charles Higham, Colin Renfrew, who joined the class after National Service and a Part I in Natural Sciences, Barry Cunliffe, Gavin Brown and Paul all earned Firsts in the final examinations.¹⁸

A young Ray Inskeep, who later became an acclaimed Africanist, taught Palaeolithic archaeology to Paul in the first year. In his first supervision, along with Charles Higham, Paul was set an essay on C.D. Forde's *Habitat, Economy and Society: a Geographical Introduction to Ethnology* to examine the relevance of hunter-gatherer ethnography to the understanding of the Palaeolithic. Paul remembers this as a most challenging undergraduate essay which, more than any other, absolutely convinced him of the importance of having anthropological and ethnographic perspectives on the Palaeolithic. 'That stayed with me throughout my life.'

Ethnographically-informed interpretations have featured consistently in Paul's research. This interest was reinforced by the Man the Hunter conference held in 1966 and the subsequent widely read volume (Lee & DeVore 1968). In 1970, after arriving at Sheffield, Paul suggested that one of his first students, Gillian Drinkwater, 'collect systematic data on population



Figure 2.3. *Creswell Crags on 'a cold and miserable' Easter in 1960, left to right, Wilfred Shawcross, Robert Soper, Colin Breese, Warwick Bray, Paul Mellars, unknown,¹⁹ Glynn Isaac. 'The highlight of every day was the trip to the pub after a meal which I have NO memories of. The contrast between the dirty archaeologists and the spic-and-span miners, who had emerged from the bowels of the earth to hot showers and hot high teas, made an impression', remembers Nic David who was then a Cambridge undergraduate.²⁰ (Photograph courtesy of Barbara Isaac.)*

densities, group sizes and seasonal mobility on a wide range of hunter-gatherers to see if she could find patterns in that data that might be applicable to archaeology'.²¹ Forty years on, Paul argued that New Guinea cargo cults illustrate how technology can be copied without a simultaneous transfer of all aspects of cultural, symbolic and cognitive patterns (Mellars as quoted by d'Errico *et al.* 1998)

Also in his first year at Cambridge, in Easter 1960, Charles McBurney took a group of now-famous students to an excavation at Mother Grundy's Parlour, Creswell Crags. Paul recalls:

Charles never forgot his participation in Montgomery's campaign in the western desert. He thought that everything should be done in a military way. The camping was all about making us men. We all had

to take turns cooking dinner. People had never even fried an egg. My friend, Desmond Collins, was told to cook pigeons. That was the worst gastronomic experience ever. Barbara also went on that excavation and that is how she and Glynn Isaac met. Things blossomed.²²

In the following year, McBurney taught Palaeolithic archaeology. Paul remembers McBurney, as a god-like figure. We treated his book (1960), the *Stone Age of Northern Africa*, almost as the *Koran*, or *Holy Writ*, in the first year. But, by the second year, those ahead of me, people like Nic David, had worked in the summers at the Abri Pataud with Halam Movius and Desmond Collins had worked with Francois Bordes at Combe-Grenal; and they were exposed to the very sophisticated excavations that were going on in France.

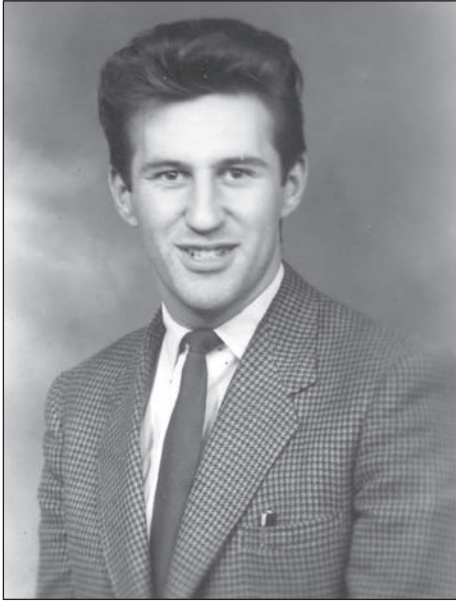


Figure 2.4. Paul as an undergraduate at Cambridge University, 1959. 'I didn't go on major excavations until the summer I graduated in 1962 when I went to work at Clark Howell's Lower Palaeolithic site, Ambrona' and subsequently at Harvard's Professor Hallam Movius's invitation, as an assistant to Nic David who was analysing the Noaillian levels at the Abri Pataud in Les Eyzies, Dordogne, France. Paul would also meet James Sackett there and later Harvey Bricker and Anny Chanut, friends for life. (Photograph courtesy of the Master and Fellows of Fitzwilliam College.)



Figure 2.5. The village of Tursac and the Chateau de Marzac where Anny Chanut lived. The Vézère River runs beneath it. 'I wonder if it's true that Paul swam down the Vézère and Anny let down her hair so that he could climb up to her.'²³ A more realistic rumour is that Nic David himself introduced Paul to his future wife in 1963. (Photograph by courtesy of Nic David.)

Also, shortly after, there was an intellectual shift in the Cambridge Department when Grahame Clark received funds from the British Academy for the British Academy Major Research Project in the Early History of Agriculture and appointed Eric Higgs as his Research Assistant, in charge of that project. Eric attracted many PhD students, including Mike Jarman, Derek Sturdy and Paul Wilkinson. Students became excited by catchment analyses and economic prehistoric approaches.

Although Paul never felt that he challenged McBurney on excavation technique as other students began to do, he did write an essay in which he argued,

the French Châtelperronian developed from the Mousterian. I came to the conclusion that the first Upper Palaeolithic communities in France were derived from the last Mousterian, which was totally against what Charles believed. We used to have supervisions, in his very cold, cavernous study with books all over the place. Charles would sit on one side of the fireplace and the students would sit on the other side. And you had to read out your essay. So, I read out this essay to Charles and he sat in stony silence; then there were a few moments of pause and

he snorted, 'I don't believe a word of it'.²⁴

Paul remained consistently faithful to this interpretation of the Châtelperronian, explaining it in detail 35 years later in *The Neanderthal Legacy*.

When remembering his PhD work at the Abri Pataud in 1962, Nic David states,

I started what Movius expected would be a de Sonneville-Bordian analysis of the Proto-Magdalenian materials. However, I soon realized that I was doing violence to the data by shoehorning tools into imposed categories. I began to experiment with what became known as descriptive attribute analysis [using means and standard deviations] to get at the social units.²⁵

At the same time, Jim Sackett was finishing his dissertation on the Aurignacian culture in the Dordogne. His was a pioneering attempt 'to use contingency table analysis, introduced into archaeology by Albert Spaulding. It may have been the first time anyone attempted analysing multi-variable interaction on a real sample of archaeological data,' Jim Sackett stated in an interview.

I was supposed to write a dissertation on Abri Pataud; then Hallam Movius kicked me off the project when it became apparent to him that the problems he was attempting to solve were, in my opinion, out-of-date. So Francois Bordes gave me my thesis topic, which entailed studying old Aurignacian collections. I then came up with the idea of re-doing artefact typology within a new statistical frame.²⁶

The resulting article (Sackett 1966), 'Quantitative Analysis of Upper Paleolithic Stone Tools' is now considered a classic.

Nic remembers discussing typology 'ad infinitum' that summer. 'Ideas were flowing back and forth between Jim, Paul and me'. Paul and Jim had dinner together nightly and 'we talked archaeology, archaeology, archaeology' and although Paul (Mellars 1996, 344–5) never engaged in the 'style debates' of the 1970s and '80s in which Jim Sackett played a key role, he greatly admires Jim's capacity for subtle thought. 'I regard those conversations with Jim in 1962 as my main education in Palaeolithic archaeology. He introduced me to the ethnologist Julian Steward's research and to the principles of what became known as cultural ecology.' Steward's arguments were set out in his article 'Ecological aspects of southwestern society' (1937), in a paper with F.M. Setzler (1938) and in *Theory of Culture Change* (1955). Steward maintained that archaeologists should use their data to study changes in subsistence economies, population size and settlement patterns.²⁷ Paul continued:

Almost everything I have done since is cultural ecology, within a strongly ecological, adaptive framework, looking at the relationships between humans and their environment. I liked ecological thinking because it put the emphasis on the kinds of things that I felt archaeology can get at, technology, environments, subsistence, seasonality; this approach did then and still does lead to understanding long-term change. That summer in '62, talking to Jim Sackett, made me a Palaeolithic archaeologist.²⁸

It was an exciting time; young researchers were re-evaluating and refining the Bordes and de Sonneville-Bordes systems. Paul added:

My school background in the sciences influenced me and I became increasingly convinced that descriptive attribute analysis was the way to go. I was obsessed with the idea that we needed to put the studies of technology and typology on a more scientific basis. We shouldn't just classify a tool. We should conduct an analysis, defining what was meant by a Levallois flake with metrical results, length, breadth, thickness, and numbers of flakes, character of the striking platform. Since the others were studying the Upper Palaeolithic, I thought I must do this sort of analysis on the Mousterian, the Middle Palaeolithic. So that is what I decided to do.²⁹

Paul's PhD project was registered as the 'Metrical and Statistical Analysis of Middle Palaeolithic Artefacts'. In looking back to statistical analyses in that era, Derek Roe, who was one year ahead of Paul at Cambridge, states, 'It is worth remembering that Paul and I were working in pre-computer days. What we did, we did by hand though David Clarke was about to change all that in his own thesis.' Derek Roe processed 38,000 items of metrical data, one at a time, using a slide rule.³⁰

Paul remembers first measuring lithics excavated by Charles McBurney from La Cotte de St Brelade in Jersey. 'I spent a lot of time trying to figure out exactly how you measure various dimensions of the flakes and how to record striking platforms and flake scars. It became problematic. It was difficult to get consistent measurements that really meant something.' Shortly after he arranged to return to the Abri Pataud to dig with Movius in the summer of 1963.

In the meantime, Paul read everything he could find on the Mousterian and was particularly influenced by Maurice Bourgon's (1957) *Les industries mousteriennes et pré-mousteriennes du Perigord*. In this publication, Bourgon systematically analysed a number of Mousterian industries from the Perigord region using the quantitative techniques which Francois Bordes eventually perfected.

Bourgon's research heralded a new era for Paul, who went on to become intricately enmeshed in the now famous 'Francois Bordes/Lewis Binford' debate. For decades, this debate exerted broad academic influence on the nature and course of Palaeolithic archaeology as it was taught in the UK and America. As Derek Roe pointed out in a recent letter, 'It wasn't long before Cambridge archaeology and anthropology students were being asked to compare, contrast and evaluate the views of Bordes, Binford and Mellars on the nature of the Mousterian in South-west France.'³¹ The debates placed industrial variability 'front and centre', making it a key disciplinary focus around which archaeology continues to revolve even today. People interviewed, who lived through the era, view that period as a time of great change that gave birth to 'the New Archaeology, processualism, new interests in theoretical archaeology'.³² Jim Sackett noted that Paul Mellars came 'at a time when the rules of the game were changing, indeed, when nobody quite knew what the rules were'.³³ As Paul commented in *The Neanderthal Legacy* (1996, 315), the debates on the significance of European and Near Eastern industrial variability were to dominate 'studies of the Middle Palaeolithic' for 30 years. In 1997, Sackett discussed the on-going relevance of this debate. In his review of *The Neanderthal Legacy*, he (Sackett 1997, 149) writes,

'Mellars has broken the log-jam created by the classic debate.' By giving 'the Mousterian a time dimension, function and style, activity and ethnicity, appear as complementary reflections of hominid behaviour that must be wrestled with.'

This debate was Paul's first experience with a major disciplinary, archaeological controversy. His life history would intersect with several controversies at turning points in method, theory and policy. Paul went on to be involved openly in many key issues, such as the debate as to whether South African delegates should be invited to the World Archaeological Congress in Southampton in 1986. He is also currently entrenched in a painful disagreement on how best to excavate and to preserve the famous British Mesolithic site, Star Carr. Although very difficult for those involved, Paul's career has added value for an historian precisely because he participates fully and forcefully in these 'turning-point' controversies.

As will be discussed fully later, Paul's contribution to the Mousterian variability debate, the chronological model, was eventually vindicated by Hélène Valladas's (Valladas *et al.* 1986) TL dating of burnt flint samples from Le Moustier published in *Nature*. Chronology and dating remained absolutely central themes in all of Paul's subsequent work.

It is therefore well worth recounting exactly how Paul developed his early thinking. Also, although readers of this biography will have studied and be aware of the functional versus cultural versus tool reduction explanations for variability, few know the personal events which led Paul to his chronological conclusions. It is best then to let Paul reconstruct his own story in his own words.

When Paul arrived in 1963, Bordes had been excavating the site of Combe-Grenal since the 1950s where he had discovered an incredible stratigraphy of 55 different Mousterian levels over nine Acheulean levels and had already published the basic sequence of the different Mousterian industries. This was not done in detail but the stratigraphic layers were listed and on the basis of this enormous sequence, and considering Bordes strong intellectual reaction to the terribly simplistic unilinear nineteenth-century French evolutionary sequences, he had concluded that the five major industrial variants of the Mousterian demonstrated 'a mosaic of different cultures and different cultural variants, more or less contemporary' (Bordes 1973, 221).

Bordes's work 'did indeed show some interstratification of what Bordes called Denticulate and Typical Mousterian' Paul noted. 'There was no question of making the Denticulate/Typical Mousterian into a simple chronological sequence'.³⁴ But Bordes

had also found at Combe-Grenal a sequence of Ferrassie Mousterian layers overlaid by Quina Mousterian layers with Mousterian of Acheulean Tradition in the top three levels.

Paul spotted this sequence immediately and began to check all other published stratigraphies on Mousterian sites. Paul stated in an interview in 2006:

It started to dawn on me, I could begin to see that at several sites the Mousterian of Acheulean Tradition was stratified above the Quina Mousterian. By digging through all old reports, the idea began to take hold that there might be a chronological sequence. These were the most typologically, technologically distinctive variants, the most clearly characterized. The more I looked, the more impressive it was. I thought that this emerging sequence could be highly significant! That is how the idea of this crucial chronological sequence developed. From that point on I was scanning literature all the time and trying to find every known stratigraphy I could.

Paul stayed in touch with excavators digging all major sites, checking the stratigraphy.

Every time I checked a new source I was on tenterhooks as to whether the next site might contradict the sequence suggested. Only a single well-documented occurrence where the Mousterian of Acheulean occurred under the Quina Mousterian would effectively shatter my whole scheme.³⁵

Then, while digging at the Abri Pataud in 1963, Paul met Bordes's illustrator Pierre Laurent with his wife, Jocelyne, who were living and working as part of the team and Laurent arranged for Paul to spend a few days digging at Combe-Grenal. Paul remembers that Bordes received him very warmly.

I only spent three or four days there but he put me to work at one of the metre squares with a little notebook. He explained how he recorded the flints and the bones by giving those numbers, square numbers and measuring the '3D' co-ordinates. Bordes was doing high-quality excavation and he was very helpful and friendly.

Then Pierre Laurent also got permission from Bordes to let Paul work in Bordes's laboratory located in an eighteenth-century chapel on the grounds of Bordeaux University.

Paul remembers:

Laurent asked Bordes to allow me to look at all of the analyses of the different assemblages from Combe-Grenal. Bordes had the full analysis of each assemblage on a large card and he recorded each of his 63 different types. He had them all in a box by layer numbers, layers 1 to 55. I spent a few days together with Desmond Collins transcribing nearly all the information from those sheets and I got all the data from all Bordes's sub-layers at Combe-Grenal on the understanding that I would never use that without

receiving permission. Bordes was immensely generous to let me have access to that material. So, I had all that data from Combe-Grenal and the sequence I was beginning to formulate was beautifully represented in the 55 levels at Combe-Grenal. Amazing!!

Paul was surprised that Bordes had never spotted this; that such an obvious chronological scheme could have been missed was puzzling. 'It never struck him', Paul stated with a certain amount of frustration during an interview. Nic David remembers that Paul, as a young Englishman, was a bit nervous about going 'up against Bordes'.³⁶ But, he also realized, and as Sackett (Laville *et al.* 1980) later explained in painstaking detail, that Bordes primarily trained as a soft-rock geologist, was completely convinced by the complex scheme of intercorrelations of sites based on sedimentological evidence; a climatic sequence had been reconstructed from the stratigraphy and sedimentology rather than from the typology.³⁷ These results provided Bordes with what he thought was a detailed, accurate chronostratigraphy for southwestern France.

In addition to looking for a chronology, Paul was also interested in applying Sackett's and Philip Phillips's idea of seriation changes within particular traditions. Paul thought he could put the different industries into a sequence according to gradually changing frequencies of different attributes.

I had the idea of seriation very firmly in mind; the Quina Mousterian occurred immediately above the Ferrassie Mousterian. So again I started gathering all the data on percentages of transverse side-scrapers, percentages of pieces which retained no cortex and of straight-edged forms, etc. I plotted the results.

With the information that Bordes had given him, Paul discovered evidence of seriation, typological trends, 'a gradual decrease in the frequencies of Levallois flakes, faceted striking platforms and blades from Ferrassie to Quina Mousterian'.

About this time, I became vaguely aware that someone in America was working on the French Mousterian; my first reaction was that he would come to exactly the same conclusion as I did so I rushed out the paper in *Nature* in 1965, pointing out a clear sequence of chronological patterning with seriation within traditions. As history has revealed, Lewis Binford had come to exactly the opposite conclusion. The obvious explanation to Binford was that the same people were doing different things, at different sites, either in different seasons or different activities or different groups of people, males or females, doing different activities using different tools at different frequencies. That could explain the five variants.³⁸

'Time passed with virtually no one noticing my 1965 *Nature* article', Paul continued in a later interview.³⁹

A few people saw the subsequent 'The chronology

of Mousterian industries in the Perigord region of south-west France' published in the *Proceedings of the Prehistoric Society* in 1969, and my 1970 *World Archaeology* article 'Some comments on the notion of functional variability in stone-tool assemblages'. I was still subdued in my whole approach and almost apologetic about what I was doing. I felt diffident. I felt that the French totally ignored everything I did.⁴⁰

This reaction of the Bordeaux University researchers did not surprise observers. As Harvey Bricker explained,

Paul's arguments were rejected because they could not be made to fit with the inter-site geological chronology being developed by Bordes's student, Henri Laville. When it came to a choice between accepting Paul's view of things versus Henri's view, it was a no-brainer. The Bordes [Francois and Denise de Sonneville-Bordes] were so convinced of the validity of Henri's approach that they seemed to regard Paul's model as almost an affront. Consider what they said on pages 65 and 66 in their (1970) paper 'We shall try here to dispose, quite definitely we hope, of the antiquated hypothesis that the different types of Mousterian represent an evolution, a hypothesis recently brought forward once again in the face of the most flagrant contradiction by stratigraphical data.'

Harvey Bricker continued:

This quote does not speak directly to Paul's model, but it shows a total impatience with his sort of approach. I don't think they saw anything methodologically unacceptable in Paul's work; it was his results that could not be allowed. Paul's chronological model, or at least much of it, was not accepted until Henri's approach was blown out of the water by chronometric dating.⁴¹

Jim Sackett also suggests that non-scientific factors underlie the Mousterian debate and that Paul was somewhat naive and failed to understand fully those factors. It was possible, for example, that the Bordeaux school 'did not appreciate a young Brit who had little experience in rockshelters telling them they were wrong'.⁴² In addition, Jim observes:

The extent Bordes really wanted parallel phyla is questionable. Between Art Jelinek and myself, Art feels he really did believe in 'tribes', whereas I believe that one has to understand the subtler background of French thinking about systematics. I'm not so sure. Bordes told me he originally assumed that the four Mousterian types represented seasonal variants of one and the same culture. However, the 'debate' with Binford hardened Bordes's attitude, as Binford was aggressive and combative and Bordes always took the opposite side whenever anyone disagreed with him.

Also

Bordes's assignment of individual strata assemblages to one of the four Mousterian industries was often

pretty iffy, given either assemblage size, assemblage make-up itself, or stratigraphic ambiguities.⁴³ Ultimately, he realized, perhaps correctly, that there weren't four industries at all and that his quartet had simply schematized a much more complex scene of industrial variation.⁴⁴

However, Jim continued:

the debate made Bordes famous in the USA, which was very important to him. He occasionally suggested to me where USA might put its missiles but he nonetheless always wore a cowboy hat and bola string tie and hugely enjoyed the admiration of American students. He was known to say that the debate made him well known in a country where people couldn't tell an end-scraper from a handaxe.

It is significant that Bordes rarely talked or wrote about the debate in France.⁴⁵ There was a general feeling among others whom I interviewed that, although the Bordes/Binford debate could not be said to have been 'staged', nevertheless Bordes and the Bordeaux centre greatly benefited; the Bordeaux traditional concern with industrial variability was successfully projected onto a world stage.

Jim Sackett also suspects that some continental scholars in the 1960s may have held a slight natural antipathy toward the 'Anglo-Saxon' who could be viewed as insular, culturally blinkered and unaware of the European research. It was also well known that Francois Bordes and Charles McBurney, Paul's PhD supervisor, thoroughly disliked each other.⁴⁶ Bordes's silent reaction to Paul's chronological results therefore seems, in retrospect, to be understandable.

In contrast to Bordes's reaction, Binford quite quickly and publicly recognized and debated Mellars's chronological arguments. He (Binford 1973, 231) accepted that there was evidence for temporal sequence but suggested that he did not 'anticipate an exclusive sequential arrangement of all the variability' and did not like the evolutionary suggestion that one form of assemblage was ancestor to another. When Colin Renfrew finally introduced them in 1971 at the University of Sheffield 'Explanation of Culture Change' conference, Lewis Binford and Paul enjoyed each other's company and have remained great friends ever since.

The Sheffield years, 1970–1980

Paul was awarded his PhD in 1967 with John Coles and Roy Hodson, who had helped with the statistics, as Examiners. From 1965 to 1968, Paul had a Research Fellowship at Sheffield University and then took up a Sir James Knott Research Fellowship at Newcastle University, after which he was offered a newly-estab-

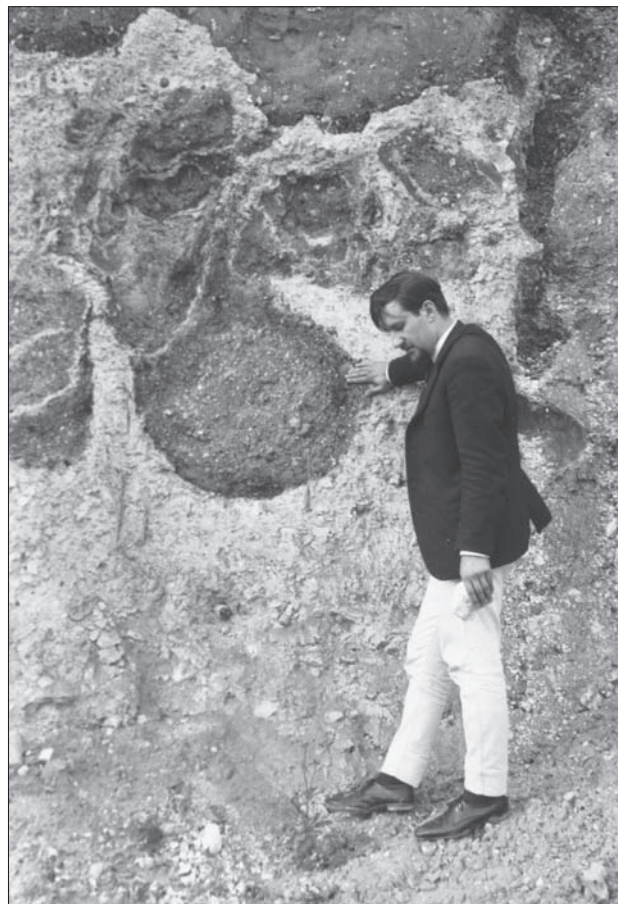


Figure 2.6. Paul in 1965 at the English Lower Palaeolithic site of Purfleet, pointing out evidence of cryoturbation deformation at the top of the chalk. (Photograph courtesy of Harvey Bricker.)

lished position to teach Palaeolithic archaeology at Sheffield. This started in October 1970.

It was Cambridge in the coalfields [remembers Paul]. All the prehistorians at Sheffield were ex-Cambridge, Warwick Bray, the first, helped to set up the teaching, then Colin Renfrew, Andrew Fleming, Colin's wife, Jane and myself came in quick succession. It was still a broad ranging department with some prehistorians, some classical archaeologists and classical historians. I started teaching courses, first-, second-, third-year courses on the Palaeolithic and offering tutorials. The number of students doing archaeology at that time was small; we could teach them in groups of three or four much as the Cambridge system. There were girls and boys from working-class backgrounds, rather timid, grey compared to Cambridge students. But, when you got to know them, they brightened up.

Paul had excellent PhD students while at Sheffield. According to Professor Keith Branigan,⁴⁷ who was at



Figure 2.7. Anny Mellars in an old car, 1965. 'At the beginning of Paul's career, he and Anny did not have a lot of money and made do with used cars.'⁴⁸ (Photograph courtesy of Harvee Bricker.)

Sheffield at the time and as we know from university records, some of these were Mike Wilkinson who worked on otoliths from the Oronsay middens, Margaret Deith who investigated seasonality as revealed by Oronsay limpets, Christine Williams who produced the thesis *Palaeoecological Studies of a Mesolithic Landscape in the Central Pennines*, David Jones whose PhD examined shellfish remains at Oronsay, Richard Nolan who wrote a PhD thesis on the spatial analysis of the Mesolithic shell midden, Cnoc Coig and Derek Sloan, who came to Cambridge to work with Paul on the re-excavation of the Star Carr area in the late 1980s. Sloan was awarded a Cambridge PhD for work on Scottish shell-midden economies in 1993.

Paul also remembers Alan Turner, Nicholas Ralph, Sally Reinhardt and Katie [Katherine] Boyle. Katie later migrated to Cambridge to become one of Paul's first Cambridge PhD students. And, in 1970, 'there was Clive Bonsall who was incredibly bright'⁴⁹ as Paul's first Sheffield PhD student. 'He had originally gone to Cambridge to study geography', Paul remembers. 'When he got there, he decided on archaeology.' But his request to transfer was declined, 'so he gave up the place at Cambridge and applied to Sheffield to read prehistory and archaeology'. Half way through his PhD, Clive, who was one of the Sheffield

students who remained successfully in archaeology, was offered a good position at the British Museum; he remembers Paul strongly advising him to take it. Before leaving for that job, Clive stated that he acted as site supervisor at the Oronsay excavation for two seasons in 1971 and 1973.⁵⁰ In remembering that work together, Paul continued

That takes the story up to the start of the Oronsay work. I did the first excavation there in collaboration with Sebastian Payne whom I met in Cambridge. Sebastian had worked with Colin Renfrew and had been a member of the Eric Higgs team. He was especially interested in how much extra small material could be recovered from fine wet sieving. He was obsessive about that.⁵¹

One day, we were talking about the possibility of doing wet sieving on shell-midden sites; I knew about the shell-midden sites on Oronsay; I had, when at Newcastle, published an article on an antler harpoon-head from County Durham which suggested a link to the Scottish Obanian (Mellars 1970b). That fired my interest, [Paul continued] so, we decided to have a look. In Easter of 1970, Sebastian, Sebastian's wife, Rosemary, baby Polly and I drove to western Scotland; somehow we got across to Oronsay at low tide. We re-discovered the famous middens that had been dug in the nineteenth century and found a new

midden; it was immediately obvious that there was potential for new work. Sebastian thought it would be a fantastic site for sieving because we might find fish bones.

This was a prophetic thought. Oronsay is now remembered by generations of students for its innovative analysis of fish otoliths as indicators of seasonality.

At that time, Paul was involved with teaching at a summer school in archaeology for American students organized by a Cambridge-based body, the Association for Cultural Exchange. The residential courses were held in one of Oxford's Colleges and since 1967 Paul had been running the course.

After we had given the students three weeks of theoretical archaeology we sent them off on digs. So I suggested that I take 10 or 12 students to Oronsay. The Association of Culture Exchange therefore funded the first excavation in July and August 1970. That is how the Oronsay operation started.⁵²

Sebastian Payne remembers that first summer,

I only dug with Paul once, and only for one season at Oronsay, in 1970. Paul was a very careful excavator, very conscious of the need to dig as little as possible in order to leave as much as possible for future archaeologists and was careful about not going beyond the available evidence.⁵³

To this day, Paul prefers to excavate small square test pits, to take column samples, to examine intensely the material recovered and to preserve the site whenever possible.

The Oronsay excavation ran in alternate years until 1979 and was conducted against a backdrop of expansive debate and innovation in British archaeology paralleled in the USA by the New Archeology. Three volumes, in which Paul's articles appeared, published during the 1970s, testify to this perceived change. 'Archaeology today is changing with astonishing rapidity' Colin Renfrew (1973, ix) observed. Again in 1974, Renfrew wrote:

Britain's past has changed in the past few years almost beyond recognition: the new datings, new discoveries and new assessments have come so fast that any survey written more than five years or so ago is inevitably out of date. (Renfrew 1974, xi)

In 1978, Paul (1978a, vii) commented, 'In common with most areas of prehistory, research into the earlier stages of postglacial occupation in northern Europe has advanced at an impressive — almost alarming — rate during the last decade.' 'To speak of a "crisis" in archaeology', Renfrew (1973, ix) stated, 'or a "methodological revolution" is now a commonplace.'

When considering the grand shifts in excavation technique which occurred in the 1960s and 1970s, Robin Dennell commented:

The days of big English excavations with scores of workmen in foreign climes were over by the 1960s so there was an enormous incentive to get more bits of data per pound per cubic metre; that was the tremendous incentive that led to the development of the seeds and bones revolution.⁵⁴

Sebastian Payne built a version of the water-sieving machine that he, David French, Andrew Sherratt and John Watson had developed and introduced at Can Hasan 3 in Turkey in 1969.⁵⁵ Bags of sieved remains and column samples were brought back from Oronsay to Sheffield. Over several years, Anny Mellars meticulously sorted the washed material in her kitchen.⁵⁶

Paul published the first results of the Oronsay work immediately (Mellars & Payne 1971) and then added further analyses over the following years (1978b; 1979; 1981; Andrews *et al.* 1985; Mellars & Wilkinson 1980), culminating in the interdisciplinary and multi-authored, team-produced, *Excavations on Oronsay: Prehistoric Human Ecology on a Small Island* in 1987. 'The remarkable concentration of Mesolithic shell middens on Oronsay' turned out to be 'unique within the context of the British Mesolithic and can be paralleled from only two or three other localities in Europe' (Mellars 1978b, 371). The material was well excavated and properly preserved so that the remains are still available for on-going investigation. The skeletal material has recently been reanalysed and may show evidence of ritualistic behaviour (Meiklejohn *et al.* 2005).

In retrospect, Sebastian Payne notes the Oronsay shell middens were especially suited to Paul's archaeological background and to the era. Paul had learned to excavate on sites which could be contained and could be treated as bounded entities and where restrained ecological interpretations were especially productive.⁵⁷

In a recent interview, Paul reaffirmed his indebtedness to Jim Sackett's 1962 tutorials, stating that the work at Oronsay and the title of the publication were influenced not only by Jim but also by Julian Steward's concept of cultural ecology, the 'rage in the early 1960s'. According to Paul, Steward had explained

the differences in some aspects of hunter/gatherer society and behaviour in terms of adaptation to the environment. I think that everything that I have done since then is in one form or another cultural ecology. Ecological thinking was a formative influence on the 'New Archeology'.⁵⁸

During the late 1960s and early 1970s, Paul was exposed to the 'New Archaeology' through reading Binford.

I became a committed processualist during the 1970s; it put emphasis on the kind of information I think we

have access to — technology, subsistence, seasonality, food supply, demography, settlement, environment and the relationship between society and environment. These are the things that archaeology can do well. And, processualists also emphasize the explanation of change. It seems to me, if archaeology can get at anything, it is change.⁵⁹

The ‘Explanation of Culture Change’ conference, held in 1971 in Sheffield, was Colin Renfrew’s creation.

Its aim was to bring together archaeologists, anthropologists, geographers and contributors from other fields ... in order to discuss afresh both the aims of archaeology and ... new approaches and methods of research. (Renfrew 1973, ix)

The conference was one of a series founded by Peter Ucko and Peter’s constructive suggestion that the papers be pre-circulated⁶⁰ made the conference especially exciting and productive, resulting in a large and ever-popular textbook. Many observers have vivid memories of the combative, almost violent, confrontation between Edmund Leach, who argued that archaeologists could not reconstruct the internal organization of prehistoric social systems and Binford who optimistically claimed he could.⁶¹ But Colin Renfrew also remembers Bordes and Binford as lively ‘interlocutors’.

‘Binford, accompanied by his wife, MaryAnn, came to stay with Jane and me during the conference’ remembers Colin. ‘I had come to know him quite well during my semester at UCLA in 1967.’⁶² Shortly after their arrival and before the conference, the Mellars invited the Binfords and Renfrews to tea at their beautifully restored home in Eyam in the Peak District.

It was a rather special occasion [Colin wrote in a recent letter]. [It] set the scene for a very pleasant social atmosphere. There is no doubt that the trip into the country from Sheffield, and Anny’s excellent tea, made this into a very agreeable excursion for all concerned and helped keep the later Mousterian interlucutions on a cheerful level.⁶³

Colin further recalls, ‘Paul had a good control of his data and was in a position to talk seriously to Binford.’ When Lewis Binford became too polemical, Paul could hold ‘him up short with a firm statement. Binford quite rapidly realized that Paul knew more about the stratigraphical sequences than he did.’ Both Colin and Paul suggest that the ‘Explanation of Culture Change’ conference offered Paul, for the first time, a platform and opportunity personally to present his hard-earned knowledge before a large, international audience.

One of the conference sessions was entitled the ‘Explanation of Artifact Variability in the Palaeolithic’ with Bordes, Binford and Paul speaking. Although

Bordes and Binford continued their Mousterian variability debate, Paul (1973) decided to present his first synthetic, overview paper on a new subject, ‘The Character of Middle–Upper Palaeolithic Transition in South-west France’. In hindsight, if Bordes had been more receptive to Paul’s chronological analysis, Paul may have remained stuck into the Mousterian debate but, given the circumstances, he was free to move quickly to new intellectual territory.

I didn’t think that I had much else to say about the Mousterian. I used to make lists of possible future papers and noticed that everyone was paying lip service to the Middle/Upper Palaeolithic transition as a major dividing point.⁶⁴

It may be difficult to believe today, but in 1970, Paul states, ‘There were all these stock generalizations in the textbooks but no one had analysed the archaeological evidence in detail.’⁶⁵ No one had yet questioned the idea of a major change and there was no thorough analysis or summarizing documentation. Paul found this to be surprising

in view of the universally acknowledged importance of these developments. [My] primary aim ... is therefore an empirical one: to examine closely the evidence from one particular area in an attempt to define what the true pattern of cultural innovations at the time of the Middle–Upper Palaeolithic transition was. (Mellars 1973, 255)

Paul’s paper went beyond mere summary of existing opinion and scholarship; he searched for the gaps in the current evidence, adding the results of his own research and analysis. For example, textbook generalizations in 1970 generally stated that Middle Palaeolithic implements were manufactured from flakes and that the Upper Palaeolithic tools were made on blades. But, on close reading, Paul found that this was an oversimplification and misleading. ‘The most original feature of Upper Palaeolithic stone-tool technology lies in the shapes of the finished tools rather than in the techniques of manufacture’ (Mellars 1973, 257). He also looked at comparative evidence on fauna specialization, number of sites, gross area of the sites and seasonality, each time pushing the limits of what was then the received knowledge. When considering Paul’s research, Colin observed, ‘He has a good sense of problem and feels things in a strong way. He broods over the problem, thoroughly and carefully reads around it and redefines it for himself.’⁶⁶

Paul considers this work to be a turning point in his career which has defined his interests ever since. This 1973 Middle–Upper Palaeolithic transition summary analysis is now seen as an iconic paper which expresses the received knowledge of its day. ‘The first

real synthesis of a broad range of data pertaining to the Middle–Upper Paleolithic transition was that of Mellars (1973) for southwestern France,’ wrote Randall White (1982, 169). ‘Mellars article is well organized and cogently written. It therefore makes an effective baseline from which debate can proceed.’ Paul’s article is viewed as an intellectual watershed which marks the beginning of the ongoing debate about the nature of this transition.

The painstaking presentation of all available evidence and literature in analytical and synthetic articles is now Paul’s trademark. As one of his former PhD students, Laura Basell, recently wrote, ‘He writes excellent big picture papers with full credit to the relevant authors and has always kept up to date with the latest developments. The criticism is that anyone can do that but in fact they don’t. Clarity in thinking and writing makes the reading easy but it is a real skill.’⁶⁷

Paul believes that synthetic, analytical articles, more than books, are the best tools to further the development and spread of knowledge. The best scholarly way to create knowledge is to produce sharply focused descriptive and detailed, concise analytical studies. Paul does this, for example, in his ever-expanding list of articles on the Middle–Upper Palaeolithic transition. The theme and purpose of these papers remain the same but the data presented changes and expands. In these ‘review’ articles, Paul follows an historical model of explanation.

‘I wrote quite a few general, synthetic papers in the 1970s on industrial variability, chronology, new radiocarbon dating methods, British Mesolithic settlement patterns and industrial variability and fire ecology’ (Mellars 1974; 1975; 1976a,b; Jacobi *et al.* 1976; Mellars & Reinhardt 1978). Some of these were especially important in the era. The 1974 volume, *British Prehistory: a New Outline*, for example, edited by Colin Renfrew, was ‘the first time a calibrated radiocarbon chronology became available for Britain.’⁶⁸ Several of these articles still figure prominently in undergraduate reading lists.

Considering the important intellectual changes that took place at the end of the 1970s and in the early 1980s, with the introduction of what became known as ‘postprocessual’ approaches,⁶⁹ it is well worth looking in more detail at one of these papers. Paul’s research into fire ecology illustrates the dominance yet complexity of ecological thinking in the era immediately before the literary turn of the early 1980s.

‘I was invited by Gene Sterud to spend one semester in 1974 at The State University of New York at Binghamton’, Paul remembers.⁷⁰ Rhys Jones, who was a year behind Paul at Cambridge, had already

introduced him to the Australian ethnographic evidence of ‘fire-stick farming’ which Aboriginal people used to encourage the growth of new vegetation and attract game. Ian Simmons (1969) had already published his ‘Evidence for vegetation changes associated with Mesolithic man in Britain’ in which he found evidence of burning. G.W. Dimbleby, (1962) Professor of Human Environment at the Institute of Archaeology, London, a specialist in forest ecology and soil formation, had published his classic study *The Development of British Heathlands and their Soils*. Dimbleby argued that the present heathlands were not natural, that they had all originally been forested and that the deforestation was an artefact of human activity during the Mesolithic. He suggested that fire could have been a major factor in this.

Coincidentally, Paul had access, at Binghamton, to the library at Cornell, a university known for its strength in forestry and ecology; Paul spent considerable time in an ‘incredible library’ and read widely an enormous amount of work on the impact of forest fires on the regeneration of vegetation and the beneficial impact on game populations. ‘I suddenly discovered this massive literature. I read that avidly. It was very exciting, all relevant to what was being discussed in Britain concerning the Mesolithic.’⁷¹ In his resulting article, ‘Fire ecology, animal populations and Man’, (Mellars 1976a, 41), Paul went beyond description and summary of the available evidence. After a detailed analysis of the ethnographic and Mesolithic pollen evidence, he argued,

You could see how the use of burning might have turned hunting strategies into husbandry, herding, pastoralist economies. Burning allowed an increase in the health, numbers and birth rate of the animals and allowed humans to control the distribution of the animals and how they moved. It allowed people to put a territorial stamp on the environment; if you had burned a bit of land, you had invested work in improving that land. You could perhaps claim ownership. I suggested that this could lead to social and economic, structural change.⁷²

The processualism of the 1970s is characterized as functionalist and behaviourist and concerned only with problems of economic subsistence, settlement patterns and demography but with Paul’s human ecology work from that period, we have an example of an attempt to explain the emergence of new social values and the importance of human agency, themes that would become popular during the 1980s.⁷³

With good students, research opportunities, a pleasant Department milieu, promise of promotion and a beautiful home, there were few incentives to leave Sheffield. As Colin Renfrew later recalled,

I do remember great discussions with Paul as to whether he should come to Cambridge or not because I was very keen that he should. It was all very open to doubt because he was well placed in Sheffield and then, of course, he did decide to come and that all worked out extremely well.⁷⁴

Cambridge, 1981–1990

The late 1970s and early 1980s were an exciting time in Cambridge. Ian Hodder organized a series of post-graduate ‘think-tank’ seminars to consider alternatives to the New Archaeology; this was in reaction to what was perceived as Binford’s polemical, reductionistic and oversimplified application of functionalist and behaviourist approaches. Hodder and others were responding as well to the naïve use of the deductive-nominological (DN) model of explanation. As Hodder (1982, 14) stated, ‘I have tried to show that the New Archaeology can be extended by reconsideration of the issues outlined by traditional and historical archaeologists, and that culture, ideology and structure must be examined as central concerns.’ A conference in 1980 on symbolism and structuralism in archaeology resulted in the edited volume, *Symbolic and Structural Archaeology* (Hodder 1982); this book now marks the beginning of postprocessualism.

Henrietta Moore, who attended that conference and who was then writing her PhD thesis, ‘Men, Women and the Organization of Domestic Space among the Marakwet of Kenya’ (1983), recently remembered those years.

I was a student with, amongst others, Danny Miller and Christopher Tilley. It was a time of immense intellectual optimism in Cambridge. We were discussing a kind of social theory that created space for human agency, for interpretation, for a creative practice. The feminist activism that I was involved in was an enormously important part of the way I approached my work ... I was interested in the questions of political economy, what difference systems of production and reproduction make to the way people lived their lives and how men and women are differentially placed in their system. Of course, we had Tony Giddens on our doorstep; we talked about human intention, motivation and much about the great problem of social theory, the relation between structure and agency. I developed an interest in Pierre Bourdieu [who was by then moving beyond French structuralism].⁷⁵

Alison Wylie, who also attended the Cambridge graduate seminars, remembered the

extraordinary group of graduate students, a pantheon of people who have since become key players in archaeology. Hodder was critiquing what he referred to as functionalist, adaptationist conceptions

of material culture. He was insisting that material culture couldn’t be understood only in terms of manifest behaviours and technical functions of material artefacts. In order to understand cultural process, it is necessary to attend to the symbolic structures. If you are going to make sense of cultural process, you must understand the inside of actions, the cognized worlds, the motivations, the symbolically-rendered environments in which people move. [It was obvious] that the New Archaeology was ill served by its positivist rhetoric. Attention to the symbolic dimensions of cultural action in the past [would require a realist philosophy of science].⁷⁶

Throughout this lively and at times derisive debate, Paul remained faithful to his preferred ecological stance, arguing that it was more productive than postprocessual approaches for Palaeolithic evidence. Although he agreed with Wylie’s argument that realist philosophy best described how science proceeds, he suspected that the social theories discussed by postprocessualists could be applied more effectively to recent periods of prehistory. Paul, with characteristic caution, felt that early postprocessual approaches relied too heavily on speculation and imagination and that people were taking too many risks with interpretation.

We would all love to know the meaning of things [he explained], but we don’t have direct access to that! The greater part of variation between Palaeolithic assemblages can be understood by reference to the environment.⁷⁷

Paul often reiterated in interviews, ‘Palaeoecological approaches put emphasis on demography, the sizes of groups, seasonality, mobility, data we can get access to.’ He would ask with a hint of frustration, ‘Why did processualism become treated as a swear word!?’⁷⁸ Paul seemed concerned by what he saw as intellectual intolerance and snobbery driven by the new post-processual approaches.

Although concerned, therefore, about post-processualism, Paul was obviously more impressed by the scientific innovations which became available at the same time as postprocessualism emerged. Robert Hedges, who has just been awarded the Royal Society’s Royal Medal for his work on accelerator mass spectrometry C14 dating, received his first large SERC grant to build the Oxford Radiocarbon Accelerator Unit in 1978.⁷⁹ The major advantage of the new technique was its ability to measure extremely small samples allowing large numbers of closely stratified samples to be dated. The ability to extract specific chemical components of the samples also reduced the possibility of contamination (Mellars *et al.* 1987).

Paul had always accepted the clear value and necessity of radiocarbon dating. As early as 1969, he sought and published dates of bone collagen samples



Figure 2.8. Paul in 1983 providing scale for a wall of flint nodules at Brandon, Suffolk. (Photograph courtesy of Harvey Bricker.)

from a new Cresswellian site (Mellars 1969b). Hedges remembered 'Paul as being a strong (!) champion of the Unit in its early days'.⁸⁰ Thus, in the early 1980s, Paul suggested to Colin Renfrew, who was then Chair of the grant's steering committee, that the Unit date the Upper Palaeolithic in southwestern France. The Abri Pataud was ideal in that it was well stratified and excavated; also the whole sequence had previously been thoroughly dated by conventional radiocarbon methods during the 1960s at the Groningen Laboratory. The Unit offered to measure a series of 30 to 40 samples to find out how the two sets of results compared. 'To get those samples I collaborated with Harvey Bricker who had been the site director.'⁸¹ When informed, Harvey responded, 'With 30 to 40 dates we can date the hell out the site,' which they did.

During a frigid December and January 1984, Harvey and Paul obtained samples from the Abri Pataud and eventually also from other Upper Palaeolithic sites which produced 'a beautiful series of dates'. The radiocarbon accelerator dates from the Abri Pataud fitted well with the Groningen dates, confirming both; these samples together with those from other sites provided a 'secure radio carbon chronology of the French Upper Palaeolithic'.⁸² 'The Upper Palaeolithic project was an early and important demonstration of where the new method could be powerfully applied',⁸³ commented

Hedges in a recent message. Paul hoped that the new scientific techniques would resolve the 'complex issues surrounding the chronology of the Chatelperronian and the earliest Aurignacian' (Mellars *et al.* 1987, 132); however, these issues would continue to haunt him.

At about the same time, in the summer of 1981, Paul had been a Visiting Research Fellow at The Australian National University in Canberra, just after Peter Ucko left the Institute of Aboriginal Studies to take up his position as Professor at Southampton. At ANU, Paul heard many stories of Peter Ucko's 'radical', abrasive, confrontational behaviour as an advocate for Aboriginals and their control of native archaeology. Immediately upon his return to England, Peter was invited to be National Secretary of the British Congress of the International Union of Pre- and Protohistoric Sciences (IUPPS). According to Peter,

I agreed to organize the conference on condition that it would take as its most serious commitment the full participation of the countries of the Third World [and indigenous peoples], to end the European domination of previous Congresses. (Ucko 1987, 3)

By all accounts, Peter's proposal to broaden the IUPPS' mandate and expand the inclusiveness of the next congress, to be held in England in 1986, was greeted with approval and considerable enthusiasm. There was general agreement that recent IUPPS' congresses may

have been poorly organized and that the IUPPS was losing touch with a growing international archaeological community. The value of Peter's vision was well acknowledged and he felt that there was a 'genuine desire to make the next IUPPS Congress into a truly world Congress' (Ucko 1987, 8).

One of the sub-themes of the forthcoming World Archaeological Congress was 'Archaeology and the Origins and Dispersal of Modern Man' organized by Paul; this theme had interested Paul since his early 1970s research on the Middle to Upper Palaeolithic transition. In 1982 and 1983, however, when Paul first suggested the topic, it was only beginning to attract the attention of British and North American colleagues. As Paul pointed out in his 1973 article, there had been some work by French scholars, but, when considering the English-speaking audience, Randall White (1982, 169) could state, 'Despite its importance, the Middle/Upper Paleolithic transition in Western Europe has been the subject of very little informed debate.' At this early stage, Paul and Randall White thought that the emergence of modern man was best documented by evidence of the transition to the Upper Palaeolithic in southwestern Europe.

Peter Ucko (1987, 34) remembered that Paul seemed ponderous and cautious at first but 'From 1983 onwards, he did much more than just the required letter-writing and took the whole enterprise [of the WAC] very seriously; he and I were to become close friends.' For his part, Paul remembered Peter as 'so charismatic, arguing his ideas with such force. He was an incredible entrepreneur, had incredible energy, drive and passion for whatever he did.' Paul admired Peter and found him 'a good man to work with'. 'Ucko used to send me off as his delegate ... to Paris once to talk to Henry de Lumley about a Lower Palaeolithic session.'⁸⁴

'Everything was going swimmingly', when in late 1983, the Pan African Association on Prehistory and Related Studies passed a motion censuring all contact with colleagues and institutions in South Africa. Then, as a State of Emergency was declared in South Africa in 1985, and hundreds were detained and many were murdered, the worldwide Anti-Apartheid movement gained momentum. The UN, Commonwealth and EEC called for sanctions. The ANC proposed that South Africa be excluded from cultural and sporting events, UNESCO banned cultural interaction with South Africans and the regime and then, after a complicated, emotionally intense series of events, the British Executive Committee voted to support an academic boycott of South Africa and exclude South African delegates from the upcoming WAC.

'All Hell broke loose.' Paul remembered intense media coverage and appalled, passionate letters from

would-be participants, especially Americans 'How can you do this! You should disassociate yourself! Support the principle of academic freedom!!' Paul recalled 'personal attacks', 'immense psychological stress' and then 'people began pulling out under the pressure'. Ezra Zubrow, who lived through the McCarthy era in the States and who witnessed academic lives destroyed by political interference, explained, during an interview, that most Americans were passionately against banning South Africans because they interpreted this as a dangerous move against individual and academic freedoms. Thurstan Shaw, on the other hand, who had worked for years in Africa, accepted the ANC's, UNESCO's and the Pan African Association's request for support. He argued that the higher hope of freedom for all South Africans must take priority over European and North American conceptualizations of individual freedom. Ironically, if Thurstan had advocated on behalf of his South African colleagues who might have wished to attend, his students and friends from the University of Ibadan would have refused or been unable to attend. The world archaeological community was thus clearly, dramatically and *very* publicly split and, in interviews, painful memories persist.

As participation in his session dwindled and after long discussions with his co-organizer, Clive Gamble, and a memorable, agonizing meeting with Peter,⁸⁵ Paul decided to withdraw and to plan an independent conference on the emergence of modern humans in 1987 in Cambridge. According to Peter (1987, 132), Paul and Clive remained helpful to the organization of what became the first WAC. This was held successfully, independently of the IUPPS, in Southampton from 1 to 6 September 1986.

Paul's decision to hold a separate, larger interdisciplinary conference was not politically or personally motivated. Paul did not participate in the 'freedoms' debate. His over-riding concern was that discussion on the emergence of modern humans should proceed and he realized that he would need a proper venue and broader stage.

It should also be remembered that Allan Wilson's (Cann *et al.* 1987) revolutionary research on mitochondrial DNA (mtDNA) as 'a source of new perspectives concerning the evolutionary history of our species and the genetic relatedness of human populations' was then, in 1985, just becoming available and known.

We proposed that all mtDNAs in modern human populations are descended from a single common ancestor who lived in Africa some 200,000 years ago (Stoneking & Cann 1989, 17).

Chris Stringer remembers

When things went pear-shaped at Southampton, it was Paul's idea to rerun the sessions now wrecked by

withdrawals ... I was already in correspondence with [Allan Wilson] about his latest results in early 1986, having heard about them from Roger Lewin and Peter Andrews, who had attended the Cold Spring Harbor meeting with Wilson. However, he [Allan Wilson] refused to participate in the Cambridge plans because he accepted [the] view that taking part would undermine the Anti-Apartheid movement. Nevertheless, he said that he would not oppose my proposal to invite Mark Stoneking and Becky Cann. I told Peter Ucko that the Cambridge meeting might be of great benefit to science and to Africa.⁸⁶

'The new line of evidence [was] extremely healthy for science', stated Chris in Roger Lewin's report on the Cambridge meeting for *Science* (Lewin 1987, 1292). 'We decided to do this [meeting] on a worldwide basis. We had everybody,' Paul remembered. 'I was very grateful to Paul and Chris for their courage in inviting me to the first conference in the political climate of academic boycotts' wrote Hilary Deacon, Professor of Archaeology at Stellenbosch University.⁸⁷ Paul concluded, 'The story we had to tell to the world about modern human origins was tremendously important for all people.'⁸⁸

Stan Ambrose, in a recent message, wrote, that the 'Origins and Dispersal of Modern Humans' meeting, which took place from 22 to 26 March 1987,

happened at a crucial time when most scholars of human evolution were first confronting serious scientific evidence which challenged the idea that modern humans and modern human behaviour developed first in Europe.

Academics began to realize, 'that we should look to Africa ... it set the tone for a new understanding'.⁸⁹

Roger Lewin (1987) agreed that the conference offered an unusual opportunity. In his review of the conference proceedings, Roger noted,

Until relatively recently there was a strong sentiment among anthropologists in favor of extensive local continuity. In addition, Western Europe tended to dominate discussions ... In fact ... if it is the origin of modern humans you are interested in, then Western Europe is something of a backwater. One of the things that is becoming clear is that the real action was elsewhere (Fred Smith as quoted by Lewin 1987, 1293).

Lewin (1987, 1295) concluded, 'Overall, the Cambridge meeting probably further tilted opinion toward the idea of replacement' and an African origin.

To some Africanists, however, the tilt was insignificant when considering the archaeological as opposed to the interdisciplinary papers. Hilary Deacon wrote,

The archaeological portion of the first conference was really about the replacement of Neanderthals by

Homo sapiens and how dumb the Neanderthals were and how Upper Palaeolithic newcomers were seen as the first undoubtedly modern people. Another colonial remarked that the ideas she had heard *x* number of years ago as a student had not changed at all at the first conference. She and perhaps other non-Europeans were a little jaded at hearing about the wonders of the Upper Palaeolithic without any explanation of what that meant for other areas of the globe where the Upper Palaeolithic was not recognized as a leap in human achievement. It was a very much a Eurocentric conference focused on the Middle to Upper Palaeolithic transition. I was totally on a limb stating heretically that at more than 100 ka, people in Africa were in all respects modern ... The fixed idea was that the Mousterian and the Middle Stone Age were non-modern equivalents because they share a Levallois technology and, therefore, it needed an African Late Stone Age to initiate an Upper Palaeolithic. Paul was concerned with the dating of the Upper Palaeolithic showing it appeared earlier in the east than the west and famously saw the Upper Palaeolithic as symbolizing the revolution.⁹⁰

The European focus of the archaeological presentations, published as *The Emergence of Modern Humans: an Archaeological Perspective* (Mellars 1990), was also noted by Michael J. Mehlman, who wrote in his review:

This book, with its strong Eurocentric bias in subject matter, is really not about the emergence of modern humans; it is mostly about a regional changeover from Middle to Upper Palaeolithic that seems to be broadly penecontemporary with replacement of Neanderthals by anatomically modern humans in Europe. (Mehlman 1992, 731)

To be fair, it should be noted that both Paul and Chris Stringer disputed the charge of Eurocentrism, pointing out that six of the papers presented at the meeting were devoted specifically to the African evidence, four to the Australian evidence and that several other African and Asian specialists were invited. Chris wrote in a letter (25 August 2008):

Maybe [these] comments are justified from the archaeological contributions but I would take a different view, given the people I invited on the biological side, and what they presented and published in volume I (e.g. all the genetic contributions favoured OOA). In fact I was accused by some of bias towards an African origin. It may be that the biological side was more open to the idea of a recent African origin, though there was fierce opposition from the likes of Milford Wolpoff, of course.

Scholars certainly did find that the broader interdisciplinary and international nature of the discussions opened intellectual venues. Marcel Otte noted that the conference,

made me discover the English thinking and speaking world. It is totally different than the one in which we are accustomed to working in Europe! No doubt this is thanks to Paul who, in a way, belongs to both worlds. The first meeting opened my mind in a drastic way and, yes, provoked an inflexion in my research.⁹¹

'I don't think we could claim that this kind of meeting was the first,' remembered Chris Stringer. 'Wenner-Gren had run a number of conferences on that scale and breadth'. Also, Erik Trinkaus (1989) had organized a closed workshop in Santa Fe in 1986, which brought together Binford, Wolpoff and others.

But, I think that it was the first major international meeting that focused purely on modern human origins from all perspectives: behaviour; fossils; genetics; what we now call evolutionary psychology; and other related disciplines ... and it ... had a great effect on my thinking.⁹²

Other attendees also remembered the conference as marking a crossroads. Hilary Deacon suggested that, since the conference, 'there has been a remarkable shift in thinking about where Paul's symbolic revolution fits into the picture. Paul too has changed his ideas.'⁹³ Geoffrey Clark states in his forthcoming tribute,

Attendance at the conference was a career-altering event for me. After the early 1980s, I shifted my geographical focus from Iberia to the other end of the Mediterranean (Jordan) and my theoretical interests from the Mesolithic back into 'deep time' ... [this] also resulted in a growing critique of Paul's construal of the Middle–Upper Paleolithic transition.⁹⁴

And, John Shea commented:

I attended the 1987 conference as a grad student. I think that I was possibly the only pre-PhD on the speaker list, and thus both very honored to be invited and fairly intimidated by the company. Dr Mellars was a most gracious host who made this very junior researcher very comfortable ... being invited ... was a turning point in my professional career.⁹⁵

In addition to the momentous DNA research which became available just before the conference was convened, Valladas and colleagues (1987; 1988) released the ground-breaking dating results from two critical Levant sites before the papers were published. A robust Neanderthal fossil from Kebara Cave was dated c. 65,000 bp (Valladas *et al.* 1987) and

Burned flints from strata containing the remains of anatomically modern-looking humans at the caves of Skhul and Qafzeh ... yielded dates between 80,000–100,000 bp ... Having early modern humans dating some 15,000 to 35,000 years earlier than their purported ancestors understandably causes some difficulty for unilinear models of human evolution. (Shea 1992, 81)

Thermoluminescence dating therefore revolutionized the then received view that the Neanderthal were necessarily ancestral to modern humans.

Looking back on the 1987 conference, Paul stated:

It was organized from Corpus Christi College, through my room [Paul recalled]. We had hardly any assistance administratively. Chris and I did everything ourselves, xeroxing papers, organizing coffees etc. It was incredibly hard work. We were both utterly exhausted and I hardly remember the conference itself ... but it all went off successfully.⁹⁶

The conference resulted in a massive series of 55 papers and CUP said it was too big. So I approached Edinburgh Press who had published the excavations at Oronsay. Chris and I decided to make the big red volume the interdisciplinary one. I remember debating what to put on that cover of *The Human Revolution: Behaviour and Biological Perspectives in the Origins of Modern Humans* (1989). The late Neanderthal from St Cesaire in western France had been dated, as had the skull from Qafzeh, an early modern human. So I put these two iconic images on the cover. The spine was so thick, we decided to add the Venus of Willendorf; she filled the space very nicely.⁹⁷

According to Paul, the big article he published in *Current Anthropology* in 1989 was, in hindsight

a very important article for me because it came in the wake of that conference. As I started to write the introduction to *The Human Revolution*, I realized that I wasn't writing an introduction but instead a massive article. So, I submitted the larger version to *Current Anthropology*. Adam Kuper was then Editor. I was dreading the review process. Having almost killed myself writing this incredibly comprehensive review, I would have to let a dozen smart alicks read it but Adam made a decision to get it out fast.⁹⁸

The article, 'Major issues in the emergence of modern humans' (Mellars 1989) has since been widely used by students and other colleagues. Its content reveals that, as Marcel Otte and Hilary Deacon suggest, Paul had broadened and changed his thinking. In the article's first section, 'Population dispersal and replacement', Paul went way beyond any of his previous work on the subject. For example, he agreed with some Africanists that, 'the situation' in 'the extreme western fringes of Eurasia ... could no doubt be seen in certain respects as ... highly peripheral and atypical' (Mellars 1989, 354). Certainly Paul's concentration on origins and his detailed inclusion of material from Africa reflected the conference agenda. He accordingly expanded and greatly refined his now well-known definition of the 'human revolution' (Mellars 1989, 370–72). He included observations about 'behaviour adaptations' in southern Africa and also theorized as to why a period of 'essential equilibrium' could have

been followed by such apparent strong changes in Europe, western Asia, and northeast Africa. When considering new evidence for behavioural change, Paul demonstrated a maturing recognition of the complexities involved.

At the outset, it must be recognized that these problems are ... largely dependent on different theoretical approaches to the interpretation of the archaeological data. For example, exactly what criteria can be used to discriminate objectively between the products of 'hunting' and those of 'scavenging' in faunal assemblages? (Mellars 1989, 355)

Also, his augmented stress on the central importance of language, as a 'single development in human evolution which could, potentially, have revolutionized the whole spectrum of human culture and behaviour' and his questions about 'whether there were some significant shifts in the basic mental or cognitive abilities of human groups' anticipated his work with Kathleen Gibson in the early 1990s (Mellars 1989, 375, 377). Finally, his statement that a period of coexistence between the Aurignacian and Châtelperronian populations 'led to a significant degree of acculturation' was to remain central to his thinking.

As mentioned briefly earlier in this essay, immediately before the 'human revolution' conference, there had been a major development in Mousterian research. In the July issue of *Nature* 1986, Helene Valladas (1986), 'the finest chronometric worker in France ... an excellent physicist who had developed 'TL' dating methods'⁹⁹ published, with her co-authors, the now famous thermoluminescence dates on burnt flint samples from all the levels at Le Moustier. Reviewing the impact of this paper on the long-standing Mousterian chronology debate, Paul stated in an interview:

Henri Laville believed, based his chronostratigraphy, that he could correlate the main Mousterian sequences in southwestern France, particularly the sequence at Le Moustier, with the much longer and more detailed 55-layer sequences that Bordes had excavated at Combe Grenal which had MTA at the top. He claimed that he could correlate the sequences of MTA at Le Moustier with Combe Grenal and that Le Moustier could be synchronized with the whole of the Ferrassie Mousterian and Quina sequences at Combe Grenal.

When *Nature* published the Valladas *et al.* article, the editors asked me to contribute a 'news and views' article commenting on the significance of the dating results. What was absolutely amazing about this dat-

ing was that it showed that the entire sequence at Le Moustier only spanned about 15,000 years. Whereas according to Laville's and Bordes's interpretation it spanned the entire Mousterian going back to 70,000 or more years, exactly the same time length as Combe Grenal. In fact, the earliest layers at Le Moustier dated from 55,000 at the bottom of the MTA; the top layers dated about 40,000. This was a 'God send' to me! Of course, I hammered home the point.

This proved that the chronology I had proposed in my PhD thesis was right!! That was a flashpoint!! That dating led to the collapse of Laville's scheme. It was a long time to keep faith with my early work but there was no way that so many stratified sites could be wrong! I had been fighting that battle for 20 years and it was over.¹⁰⁰

Unfortunately as a result of time and space constraints, this biography must end here with the conclusion of the 1980s but Paul's life continued productively, beginning the new decade of the 1990s with a flourishing cluster of postgraduate researchers which included Mark White, Paul Pettitt, Dimitra Papagianni, William Davies and Nathan Schlanger. Referred to by students as 'Oncle', Paul began work on his *magnum opus*, *The Neanderthal Legacy: an Archaeological Perspective from Western Europe* (1996) which was to be brilliantly reviewed. His ferocious appetite for new material and solid, very difficult debates would lead him to the intense Neanderthal acculturation exchanges as well as the seriously taxing controversy concerning excavation methods at Star Carr and the vast over-arching issues of the origins of biologically and behaviourally modern human populations in Africa and their dispersal over all regions of Europe and Asia. In 2008, Paul was awarded the prestigious Grahame Clark Medal for Prehistory by the British Academy and, in 2010, a Knighthood 'for services to scholarship' in the New Year's Honours List, making his 95-year-old father proud and representing a long journey from the coal mines of Yorkshire.

When asked to contribute a concluding comment and fair evaluation of the early decades of Paul's life portrayed here in 'From Swallownest to Cambridge', his colleague, Clive Gamble, stated:

Paul's biggest contribution has been that he [has often been] right. He was right about the vexed Mousterian variability question; he was right about the origins of modern humans; he was right to resign from WAC; and he was right about the significance of science in archaeology. It is an impressive record and most of us just follow his lead.¹⁰¹

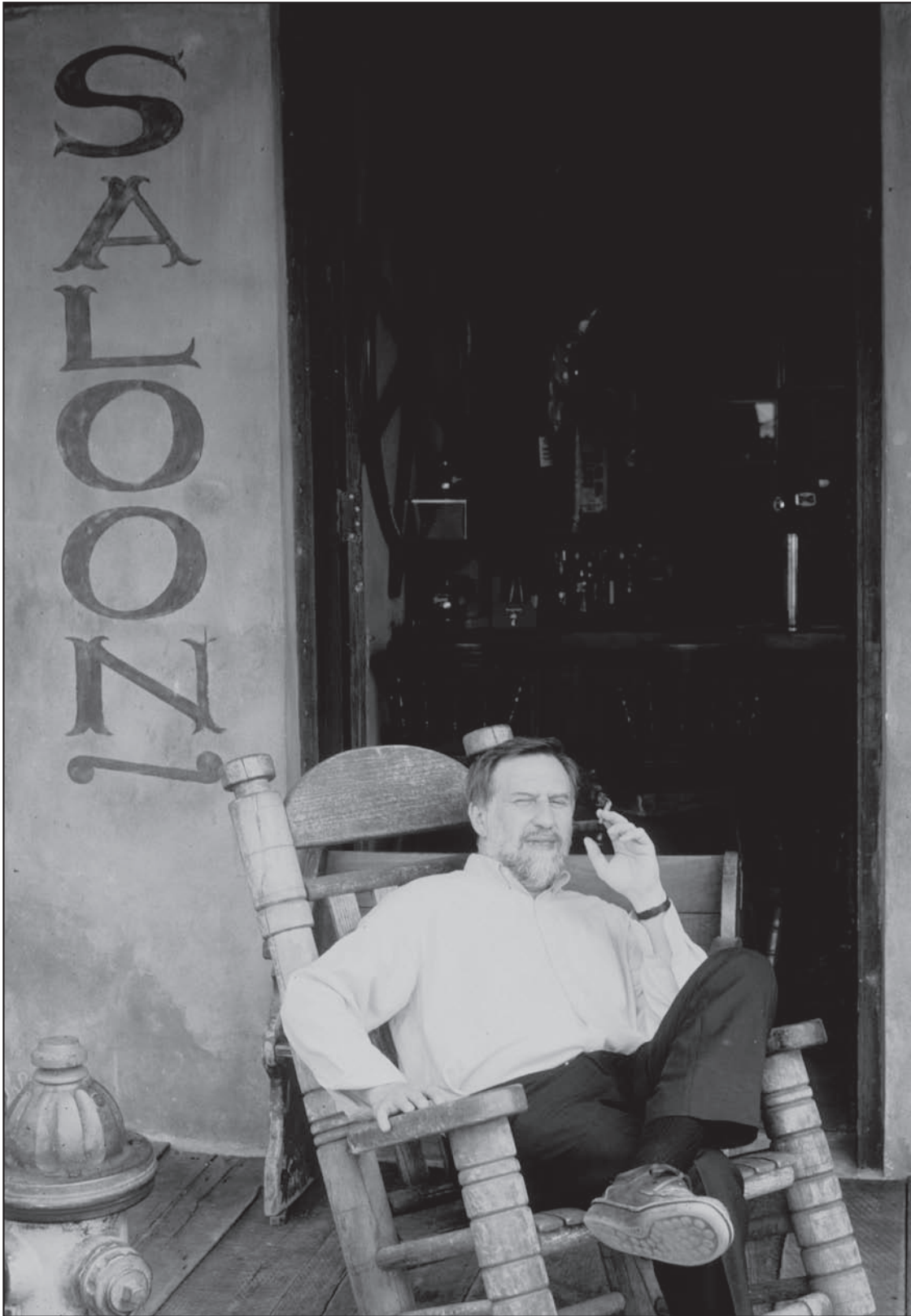


Figure 2.9. June 1987, Paul relaxing at Natchez-Under-the-Hill, Mississippi, following a visit to an archaeological site on the Natchez Trace. (Photograph courtesy of Harvey Bricker.)

Acknowledgements

Thank you to Sir Paul and to the dozens of friends and colleagues who generously contributed their time and memories. It was my great pleasure to work with such a pleasant group!! The piece is dedicated to my husband, Thurstan Shaw, to commemorate his 96th year and to our great friend Jim Sackett.

Notes

1. *Preface*: For those unfamiliar with oral-historical work, a biography based on oral-historical evidence specifically values and concentrates on the individual's own narrative. This narrative is then used to augment written sources. This essay, therefore, offers a personal explanation of how and why Sir Paul Mellars produced his well-known publications. It fills in the details of his life and illuminates questions of motivation, influence and intention. This type of biography is not expected to be utterly objective and does not summarize or analyse Mellars's written material only. This is an intellectual biography based on detailed, new oral-historical interviews conducted over a number of years. The story has not been publicly available before and hopefully you, the reader, will find this type of history enjoyable, educational and valuable. Through Paul's experiences, we see aspects of the development of archaeology over 55 years. It is assumed that the reading audience has some archaeological background and some knowledge of Paul's published research. It should be mentioned that all quotations have been approved.
2. James Sackett, correspondence with author, 20 April 2006.
3. Paul Mellars discussing his Yorkshire childhood with author, 27 October 2006.
4. 'Paul Mellars has a knack for grasping and clearly articulating the "big picture": John Shea, correspondence with author, 31 March 2008.
5. When asked to name a memorable 'Paulism' Laura Basell responded 'And the 64,000 dollar question is ...': correspondence with author, 14 March 2008.
6. Harvey Bricker, correspondence with author, 6 April 2008.
7. Anny Mellars describing her husband's religious views, 25 March 2008.
8. Woodhouse Grammar School ex-student (1946–54), Mr Jon Layne, correspondence with author, 3 April 2008.
9. Paul Mellars, interview with author, 27 October 2006.
10. Paul Mellars, interview with author, 27 October 2006.
11. Paul Mellars, interview with author, 27 October 2006.
12. At that point, the Institute offered only a postgraduate diploma. As John Evans stated in an interview with the author, 17 August 2000, 'It wasn't until 1969 we were able to establish a first degree course. Some people on the staff were very much against it, like Kathleen Kenyon and Wheeler himself.' Archaeology was considered to be a postgraduate activity. Paul could not therefore have studied for a degree in prehistoric archaeology at UCL or the Institute in 1958.
13. The site includes poems as well as tributes, 'Paul Mellars was my DOS's name, yes grey bearded, of Neanderthal fame, Bumbling of walk, middling of gait. But rather wonderful and simply great.' copyright L.A. Tyrrell, 27 January 2006.
14. Tim Reynolds, interview with author, 17 March 2008.
15. Paul Mellars, interview with author, 27 October 2006.
16. Paul Mellars, interview with author, 3 November 2006.
17. 'Barry used to refer to him as Rik; the rest of us would call him Sir Mortimer Wheeler. We were enormously impressed by that.' Paul Mellars, interview with author, 3 November 2006.
18. The Cambridge undergraduate degree is a three-year course with a Part I and II; a perfect GPA in the North American system would be the closest equivalent to a First in the British system; in Cambridge, Firsts are rare indeed.
19. Derek Roe, who was also there, suspects that the unknown undergraduate is 'Edward' whom I cannot trace. (Roe, correspondence with author, 2 June 2008).
20. Nic David (described by former classmates as 'very dashing'), correspondence with author, 22 November 2006.
21. Paul Mellars, interview with author, 11 January 2007.
22. Paul Mellars, interview with author, 27 October 2006.
23. Nic David, correspondence with author, 15 May 2006.
24. Paul Mellars, interview with author, 3 November 2006.
25. Nic David, correspondence with author, 27 October 2006, 29 November 2006.
26. Jim Sackett, correspondence with author, 28 May 2007.
27. For an explanation of the beginnings of ecological archaeology in the 1930s, see my PhD thesis (Smith 2009) and Trigger (2006).
28. Paul Mellars, interview with author, 6 May 2008 and 20 June 2008.
29. Paul Mellars, interview with author, 3 November 2006. This was a hard decision for Paul. Since his school days the British Mesolithic had fascinated him and, as an undergraduate, he had investigated Deepcar, a Mesolithic site in Yorkshire. During 1962 and 1963, Paul was writing up these results which successfully challenged the long-held view that the Maglemosean was found only in eastern lowland England (Radley & Mellars 1964). Paul further explored this in 1974, finding that the broad blade and narrow blade assemblages were chronological phrases of an earlier and later Mesolithic.
30. Derek Roe, correspondence with author, 3 April 2008.
31. Derek Roe, correspondence with author, 3 April 2008.
32. Derek Roe, correspondence with author, 3 April 2008.
33. Jim Sackett, correspondence with author, 31 May 2007.
34. Paul Mellars, interview with author, 24 November 2006.
35. Paul Mellars, interview with author, 24 November 2006.
36. Nic David, interview with author, 15 May 2006.
37. This was Laville's mistake. 'In some parts of the world at certain times, typology is a LOT more capable of sensitive dissection of the past than geoarchaeology,' wrote Nic David in a letter to the author, 7 July 2008.
38. Paul Mellars, interview with author, 24 November 2006.
39. Paul Mellars, interview with author, 11 January 2007.

40. Paul Mellars, interview with author, 11 January 2007.
41. Harvey Bricker, correspondence with author, 6 May 2008.
42. Jim Sackett, correspondence with author, 5 March 2008.
43. 'The real problem was that he put novice excavators to work and gave them very little guidance, and Combe Grenal was way too complex to excavate without constant supervision.' Jim Sackett, correspondence with author, 10 August 2008.
44. Jim Sackett, correspondence with author, 8 July 2008.
45. Jim Sackett, correspondence with author, 5 March 2008.
46. Derek Roe, correspondence with author, 3 April 2008.
47. Branigan, correspondence with author, 15 March 2008. Branigan remembers Marmaduke taking Paul for walks and recalls the persistent rumour that Paul was an admirer of Dolly Parton's 'music rather than the lady herself'.
48. Harvey Bricker, correspondence with author, 6 April 2008.
49. Paul Mellars, interview with author, 11 January 2007.
50. Clive Bonsall, interview with author, 29 November 2006 and correspondence, 4 May 2008.
51. Paul Mellars, interview with author, 11 January 2007.
52. Paul Mellars, interview with author, 11 January 2007.
53. Sebastian Payne, correspondence with author, 25 May 2008.
54. Robin Dennell speaking at the 2006 Personal Histories in Archaeological Theory and Method panel, 23 October 2006.
55. The first water sieve was, 'a square tank with a sieve sitting on a ledge inside the tank, water coming in through a sort of bubbler and then bringing all the water out over a weir so that the floating stuff went into a second sieve that caught it. This machine was later further developed at Siraf by David Williams.' Sebastian Payne, interview with author, 23 May 2008.
56. Paul Mellars, interview with author, 13 March 2008.
57. Sebastian Payne, interview with author, 2 July 2008.
58. Paul Mellars, interview with author, 20 June 2008.
59. Paul speaking at the Personal Histories Panel, 23 October 2006.
60. Colin Renfrew, interview with author, 31 March 2008.
61. It was at this conference that Edmund Leach (1973, 762) made his oft-quoted, prophetic statement, 'The paradigm which is currently high fashion among the social anthropologists, namely that of structuralism, has not as yet caught up with archaeologists. Don't worry, it will!'
62. Colin Renfrew, interview with author, 31 March 2008. For additional information about Colin Renfrew and other former 'New Archeologists', see the film, Personal-Histories in Archaeological Theory and Method; the Beginnings of Processualism <http://www.arch.cam.ac.uk/personal-histories/video.html>
63. Colin Renfrew, correspondence with author, 12 July 2008.
64. Paul Mellars, interview with author, 16 April 2008.
65. Paul Mellars, interview with author, 16 April 2008.
66. Colin Renfrew, interview with author, 31 March 2008.
67. Laura Basell, correspondence with author, 14 March 2008.
68. Colin Renfrew, interview with author, 31 March 2008.
69. For documentation of these shifts, see 'Personal-Histories; the Beginnings of Post-processualism.' On YouTube at <http://middlesavagery.wordpress.com/2008/03/28/personal-histories-at-cambridge/>
70. Paul Mellars, interview with author, 16 April 2008.
71. Paul Mellars, interview with author, 16 April 2008.
72. Paul Mellars, interview with author, 16 April 2008.
73. Despite the fact that Paul wrote fluidly and clearly and published repeatedly in major journals, he suffered from intense writer's block throughout the 1970s and still remains always cautious about his results.
74. Colin Renfrew, interview with author, 31 March 2008.
75. Henrietta Moore, speaking at the Personal Histories Panel, Cambridge, 22 October 2007.
76. Alison Wyllie, speaking at the Personal Histories Panel, Cambridge, 22 October 2007.
77. Paul Mellars, interview with author, 20 June 2008.
78. Paul Mellars, interview with author, 20 June 2008.
79. Robert Hedges, correspondence with author, 24 July 2008.
80. Robert Hedges, correspondence with author, 21 July 2008.
81. Paul Mellars, interview with author, 16 May 2008.
82. Paul Mellars, interview with author, 16 May 2008.
83. Robert Hedges, correspondence with author, 21 July 2008.
84. Paul, speaking, 6 May 2008. 'And, Peter was so utterly disheveled. I remember when we went to Paris, I met him at the airport. Peter typically arrived; he wasn't wearing a jacket, his trousers were falling down, half his shirt tail was out and he was carrying an enormous bag with every conceivable file that possibly related to anything we were doing, a massive weight ... and that is the way Peter operated.'
85. 'We did respect each other ... Peter said, 'I got to talk to you.' He took me to a little restaurant outside Waterloo Station and spent an hour and a half trying to persuade me. There were tears streaming down his face. This was terrible and that's how it ended': Paul Mellars, interview with author, 6 May 2008.
86. Chris Stringer, correspondence with author, 4 May 2008.
87. Hilary Deacon, correspondence with author, 8 May 2008.
88. Paul Mellars, interview with author, 6 May 2008.
89. Stan Ambrose, correspondence with author, 28 April 2008.
90. Hilary Deacon, correspondence with author, 8 May 2008.
91. Marcel Otte, correspondence with author, 29 April 2008.
92. Chris Stringer, correspondence with author, 27 April 2008.
93. Hilary Deacon, correspondence with author, 8 May 2008.
94. Geoffrey Clark, manuscript.
95. John Shea, correspondence with author, 31 March 2008.
96. Paul Mellars, interview with author, 6 May 2008.
97. Paul Mellars, interview with author, 6 May 2008.
98. Paul Mellars, interview with author, 20 June 2008.
99. Paul Mellars, interview with author, 16 May 2008.
100. Paul Mellars, interview with author, 16 May 2008.
101. Clive Gamble, correspondence with author, 25 March 2008.

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