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# The making of the British Early Palaeolithic, 1880-1960

**Anne O'Connor**  
**Department of Archaeology**

Thesis submitted for the Degree of Doctor of Philosophy  
At the University of Durham

**2003**

Volume 1 of 1

‘And there were giants in those days’



Watercolour of Scottish scenery and giant hammer labelled ‘Hutton’. The inscription reads: ‘And there were giants in those days’. James Hutton was a pioneer of geological research. Reproduced courtesy of Imperial College, London (Pencil signature ‘E.J’: ICL: KGA/Ramsay/4/5/6).



28 APR 2004

**Anne O'Connor**

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**Thesis submitted for the degree of Doctor of Philosophy, 2003**

**Abstract**

This historical study explores the character of interpretations of the British Palaeolithic record between *c.* 1880 and *c.* 1960, focusing on attempts to classify and order the Early Palaeolithic industries. Interpretations were developed through complex interactions between individuals and groups, who were influenced by a range of aims, expectations and research opportunities. The impact of their contributions was partly dependent upon academic standing and the style of approach taken within the competitive arena of day-to-day research. General expectations of industrial patterning were founded upon the Early Palaeolithic industrial sequence from Western Europe, particularly the chronological succession from the Somme Valley, France, where the hand-axe was the most notable artefact. The assumption of progression and the use of prominent type-fossils such as the hand-axe in classifying and ordering industries coloured interpretations of the British Early Palaeolithic sequence. This is evident even in the approaches to the naturally-fractured stones known as 'eoliths'. The idea of a single, progressive line of industries also fuelled argument over the position and affinities of the flake-rich Clactonian industry in the 1910s and 1920s. In the wake of rising uncertainties, the parallel culture scheme proposed by Henri Breuil achieved great popularity in the 1930s. The value of the Palaeolithic industrial sequence as a relative Quaternary chronology encouraged a complex interdependence between interpretations of the Palaeolithic and of the Quaternary sequence which helped to promote and to reinforce the new range of expectations that had been generated by Breuil's scheme. However, by the mid-1940s, the rigid chronological order of industries proposed by Breuil had been weakened. By the late 1940s, researchers doubtful of the accuracy, scale and value of his scheme, expressed a desire to move away from the constraints of chronology and typology and towards more ecological and anthropological interpretations.

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## **Declaration**

None of the material contained in this thesis has previously been submitted for a degree at the University of Durham, or at any other university.

## **Statement of copyright**

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## **Terminological Note**

Past ideas about the broader groupings of pre-Upper Palaeolithic industries were varied and changeable. To avoid confusion, the term 'Early Palaeolithic' has been employed to encompass what we would now refer to as the Lower and the Middle Palaeolithic periods (i.e. pre-Chellean, Chellean, Acheulian, Clactonian, Levalloisian, Mousterian). However, the use of industrial terminology below varies through the course of the thesis. An attempt has been made to follow the contemporary meaning of the period under discussion in order to retain important nuances. Any changes in this meaning have been made clear as they occur.

# CHAPTER 1

## **Introduction**

In 1883 Worthington G. Smith, a draughtsman from London, was considering the future of a relatively new scientific subject that was of the greatest interest to him: the British Palaeolithic. He wrote:

‘The day will come when we shall know much more of palaeolithic men than we know now. At present we only know that such men once existed and made weapons and tools of stone during long periods of time. How or where they first appeared as human creatures we can only guess. When we know we shall modify our use of such terms as “River Drift Men”, “Cave Men”, &c., and we shall probably be able to mark out more or less distinctly a succession of men, a succession of geological events, and a distinct succession of progressive steps in the men from the lowest savage to the barbarian’ (Smith 1883, 274).

This thesis explores the fascinating but undervalued period of research into the British Early Palaeolithic that followed this prediction. The object is to understand what drove the varied and changing interpretations of Smith’s ‘weapons and tools of stone’ that were offered between the 1880s and the 1950s and, more specifically, to examine how these interpretations were influenced by the social, institutional, and intellectual backgrounds of the researchers involved. This far-ranging question can be split into two linked areas: the interpretations that were put forward, and the context within which those interpretations were presented. Each of these areas, in turn, suggests a series of more specific questions, which are outlined below to give an idea of the approach taken in the following chapters.

- *Interpretations.* Is it possible to identify any key assumptions regarding the patterning of the Early Palaeolithic record that may have influenced the character of interpretations? What kinds of intellectual arguments were used to defend or question expectations derived from such assumptions?
  
- *Research context.* What kind of individuals and groups of workers gathered around Palaeolithic research? What distinctions did *they* draw between each other, and what were their different respective research agendas: their aims, approaches, and expectations? What kind of interactions occurred between



these individuals and groups, and can any tactics be identified? And, returning to the first question, what were the results of those interactions on approaches to, perceptions of and interpretations of the British Early Palaeolithic record?

## **1.1: Scope of research**

This thesis is intended as a historical study: a detailed examination of research into the British Early Palaeolithic between the 1880s and the 1950s. My research has been carried out within an archaeology department, largely because the history of archaeology rarely makes an appearance in history and philosophy of science departments. The reader should not expect this thesis to make connections either to current archaeological research, or to add to the growing body of material on the philosophy of science. It is tacitly assumed below that scientific theories are not objectively built upon facts, but that facts are value-laden and theory-laden, and that theories are influenced by the social, cultural, intellectual and historical contexts of their proponents. The task here is not to prove this assumption, which is now widely accepted, but to reach a deeper understanding of the complexities of interaction involved in scientific research through a specific historical study, treading a line between detailed biography and generalised theory. It is nonetheless hoped that this level of historical detail might prove useful for philosophers of science in developing or refining their theories, or to those currently working in British Palaeolithic research in giving a sense of historical perspective to current debates and approaches.

## 1.2: Reasons for research

The historical study carried out over the following chapters fills an important gap in existing knowledge about the history of British Palaeolithic research and makes a significant contribution to current understanding of the levels of intricacy involved in scientific argument. The history of Palaeolithic archaeology has received remarkably little academic attention. James Sackett (1981; 1991) has given an illuminating analysis of the development of French Upper Palaeolithic research, and a few detailed examinations have been carried out on specific areas connected with British Palaeolithic research (such as Grayson 1986; Spencer 1990; Dennell 1990; McNabb 1996), but this subject has received very little attention overall. Such comparative neglect of this fascinating period of scientific history is surprising, particularly because the research into the British Early Palaeolithic that was carried out between the late nineteenth century and the 1950s offers a unique and complex perspective on the social, institutional, and intellectual processes that lie beneath the sober results of academic study. This is clearly demonstrated by the three following points:

- First, a great variety of social groupings of researchers were involved in Palaeolithic archaeology: professionals, non-professionals, specialists, generalists, theoreticians, fieldworkers and so on.
- Second, these researchers were ranged around an equally diverse spread of institutions: museums, universities and the Geological Survey, as well as national and local societies.
- Third, and perhaps most interesting of all, Palaeolithic archaeology provided a major, and very animated, focal point for debate between representatives of an astounding variety of disciplines (glacial geology, river-terrace geology, palaeontology, anthropology), each of whom brought their own agendas, expertise and expectations to the interpretation of the Palaeolithic.

The various interpretations of the British Early Palaeolithic, presented and debated between the 1880s and the 1950s, were stimulated not only by new discoveries, but

also by traditions of research centred upon this immense diversity of social, institutional and intellectual interactions. Together, these provide the key to answering the questions posed above.

### **1.3: The period under scrutiny**

The main body of the thesis starts in the 1880s and ends in the 1950s. Although little has been published recently on British Early Palaeolithic interpretations between these dates, this thesis is not intended as a narrative history. Certain themes and debates have been carefully selected from this complex period that illuminate the relations between interpretations and the social, institutional, and intellectual context of the researchers who made them. Research has focused on the contentious task of ordering the British Early Palaeolithic industries and positioning them within a broader Quaternary framework, in particular the context and content of arguments over the eoliths and the Clactonian industry. It is hoped that these areas of past debate will illustrate the complicated relations between interpretations of the British Palaeolithic and the wider historical context of research.

The analysis of competing classifications and correlations of the Palaeolithic provides a revealing standpoint from which to view the influences that drove different interpretations. Research into the process of classification and categorisation can suggest deeply held assumptions that directed the choice of areas considered 'relevant' to Palaeolithic research (see Douglas and Hull 1992, 2). Such assumptions influenced decisions about where to draw boundaries between different Palaeolithic industries, decisions that would, in turn, constrain future interpretations. Arguments over how to order the Palaeolithic record also illustrate the kinds of distinctions that were being set up between different research areas, and these intellectual classifications deserve attention since they had an enormous influence on approaches to and interpretations of the Palaeolithic.

Arguments over how to position palaeolithic industries in time and space involved significant interaction between a range of Quaternary researchers, only a minority of whom might have been described by their contemporaries or by us as 'archaeologists'. Many nonetheless played a vital part in Palaeolithic research, not least by supplying information on the Quaternary context of industries. However, since different individuals might offer very different readings of this context, each with different implications for Palaeolithic interpretation, this became a particularly contentious field. The interactions and arguments between those working on the

problem of how to order the past clarify many of the research agendas, assumptions, and techniques of intellectual argument that inspired different interpretations of the Palaeolithic. This revealing area of past research is explored through a detailed study of two particular debates, the British Eolithic arguments and the discussions over how to classify and interpret Early Palaeolithic flake industries such as the Clactonian.

### *1.3.1: The eolith debates*

The arguments over the British eoliths comprise the first of the two major debates examined in this thesis. This controversy offers an unusual perspective on the expectations, approaches and interactions that lay behind ordering and classifying the earliest lithic discoveries to come from Britain. Discussion centred upon the correct interpretation of chipped stones from Kent and from East Anglia, considered by many researchers of the late nineteenth and early twentieth centuries to have been manufactured by some of the earliest inhabitants of Britain, but regarded by others as natural products.

Today, the eoliths are widely regarded as a dead debate – an invented industry created upon naturally flaked stones. However, an examination of the variety of researchers who converged on this question in the past, their expectations and assumptions about palaeolithic industries, their social strategies, and the distinctions they drew between different research groups can reveal many of the unspoken assumptions and expectations that guided mainstream Palaeolithic research – and the eoliths were indeed part of mainstream research for longer than many care to remember.

The British Eolithic arguments provide a particularly good debate with which to start this historical study. First, these emerged in the late 1880s, three decades after the existence of a Palaeolithic era came to be countenanced in the aftermath of the human antiquity debates. By the latter half of the nineteenth century, British Palaeolithic research had developed a more clearly-defined character, and this is a great help when trying to follow complicated arguments and unravel the historical context within which various interpretations were presented. Second, the eolith debates offer a comparatively unencumbered glimpse at the workings behind

Palaeolithic research, being relatively distant from current intellectual baggage and emotive responses that could afflict the historical study of more acceptable industries such as the Acheulian or the Clactonian.

### ***1.3.2: The Clactonian, and other Early Palaeolithic flake industries***

The themes above are explored in more detail through an examination of one of the most influential and long-running debates in the entire history of Palaeolithic archaeology: the reactions to newly recognised Early Palaeolithic flake industries and the varied arguments surrounding the problem of how to incorporate these within the existing industrial scheme which was dominated by the hand-axe. A study of the individuals and groups involved, their respective aims, approaches, and expectations, suggest a number of the driving forces behind various interpretations of the Palaeolithic. Particular attention has been directed towards the reception, in the first half of the twentieth century, of the flake-dominated industry that eventually became known as the Clactonian. The discussions over this industry, one of the few to be named after an English rather than a French locality, aroused academic interest beyond British shores, thus introducing some of the Continental contributions that had such an important influence on research into the British Palaeolithic.

The debates over the Clactonian, a very early flake industry, also provide a good focus for analysis because they remained relatively clear compared to those directed towards flake-dominated industries dating to later periods of the Palaeolithic. As the Early Palaeolithic drew on, the branching tree of flake industries described by researchers become increasingly complicated, and discussions become confused by arguments over the relations between these later flake-dominated industries. However, with the Clactonian, it is somewhat easier to pick up general patterns behind different interpretations.

The analysis of the Clactonian examines the context of early reactions to this industry, and then explores the rise, fall and aftermath of certain influential expectations of Palaeolithic patterning popularised by Henri Breuil in the 1930s which were subjected to a backlash in the 1940s. The discussion of the Clactonian is brought to a close in the 1950s. This marks the end of the main body of the thesis.

#### 1.4: Methodology: a critical review of the sources used

A wide variety of sources have provided useful information on the approaches and case-studies outlined above. Although relatively few secondary sources exist, a vast quantity of primary published material has been consulted: major treatises and synthetic works, journal articles, and reviews of these works. A further level of detail has been supplied by a variety of unpublished materials: notebooks, photographs, maps, drawings and private letters scattered in archives across the country (see the list of archives in the Manuscript Sources section in the Bibliography).

The importance of published works, which contain the publicised opinions of the main protagonists, is self-evident and needs little elaboration. However, it is important when using such sources to be aware of the restrictions imposed on the style, format, and content of academic publications, which have to conform to traditions of scientific narrative in order to achieve plausibility. An example of such constraints is given in the comment made below on the Reports of Field Meetings, which was published in the Proceedings of the Geologists' Association in the 1950s:

'The reports, while generally adequate on the geological side, tend to be somewhat inhuman; no-one, reading the staid and factual accounts would suspect that geologists could ever be guilty of "letting their hair down". As is truly stated, in the report of the Swiss meeting of 1947, on Sunday, 7 September, "a lunch was provided at Guggisberg". No mention is made of the high light of the occasion, namely, that between courses, the party was regaled with a concert by Professor R.F. Rutsch on the concertina and Dr. K. Arbenz on the double bass, with songs by a bevy of the local beauties clad in their cantonal costumes, yet this was an outstanding feature of the meeting' (Himus 1954, 8).

Such reticence also coloured the tone of official scientific communications, including those on Palaeolithic subjects. The critical analysis of published works can nonetheless give a good indication of the bare framework of past research: the influential and emotive arguments, and the alignments of different researchers. However, the letters of past protagonists supply more personal and colourful details about the social, institutional, and intellectual context within which the official published interpretations were constructed and debated. They do not always use the inhuman tone of published work.

Although the narrative traditions surrounding private correspondence also stimulated self-conscious or tactical writing, this was usually of a rather different character. For example, James Geikie, the glacial geologist who was soon to become Professor of Geology at Edinburgh University, could describe the prestigious journal *Nature* as a one-sided ‘kiss-my-arse of Macmillan & Co’ with equanimity in private correspondence (J. Geikie to Peach, 9<sup>th</sup> March 1881: BGS: GSM1/321/69), although he used what he described as more ‘parliamentary language’ in print (J. Geikie to Ramsay, 27<sup>th</sup> February 1881: ICL: KGA/Ramsay8/412/15). Some researchers even broke out in occasional verse to describe and defend their opinions.

The most immediate but also the most fleeting communication took place in face-to-face discussion. However, this could be restricted by time constraints, or by physical or personal distance, so contact between different parties was often sustained through the writing of letters. At first glance, a letter contains only half of a two-way, ongoing process, which can make these seem a rather restricted field of analysis. For instance, when visiting X’s archive it can often be frustrating to discover so many letters written to X, but none of X’s replies. However, if the other half cannot be located in another archive, then the tone or content of the original can often be inferred through careful analysis of the response. A few might contain annotations made by the recipient, or even comments scribbled by more than one hand suggesting that the communication had been passed on by the recipient to other interested parties. Such was the importance of letters to Palaeolithic research that direct impressions were sometimes taken of original letters to facilitate wider discussion of their contents.

Letters are amongst the most interesting and valuable relics of past interaction and they contain echoes of communication on a range of levels. They might simply give a friendly greeting or pass on an interesting piece of new information, in which case the level of formality can suggest the social and academic relationship between the writers. Letters could be employed to discuss more complex, even annotated, concepts in an arena less constrained by the restrictions and commitment inherent in published works, thus providing another important level of detail about the reasoning behind intellectual arguments. Letters also provide information on the social context

of arguments. They served to reinforce alliances, sustain antagonisms, and direct the various battle plans behind debates. Heated and heartfelt outbursts that might have been suppressed in more public arenas of dissemination were often aired in private correspondence. Aside from all this, letters give an idea of the individuals behind the academic debates, through style, tone, anecdotes, and even handwriting. Personal idiosyncrasies survive in letters better than in print: Charles Lyell's robotic wish-lists of information needed for the next edition; Hugh Falconer's script, curling like elephant tusks; Prestwich's massive, indecipherable scrawl; Harrison's fountain-pen flourishes; James Geikie's ribald stories of pre-Professorial years on the Survey; A.S. Kennard's rounded letters and lack of punctuation; Warren's upright hand and kindly courtesy; Breuil's spiky, illegible hieroglyphs which gradually drift to the right as he fills the page.

Enough on the importance of letters. Suffice it to say that a certain critical appreciation is required when interpreting such accounts, which were heavily influenced by the respective interests and roles of the correspondents and their positions within the broader research community – a bias that can be most illuminating.

## 1.5: Chapter outline

A loosely chronological order has been followed in the chapters below, which discuss the Eolithic and the Clactonian debates in turn. Each chapter also selects a different historical facet of research for particular attention. Although the social, institutional and intellectual aspects of research are bound together in a most complicated fashion, they have here been pulled apart a little for clarity of argument.

Chapter Two introduces the background to later arguments through an exploration of the character developed by Early Palaeolithic research over the latter half of the nineteenth century: the people who gathered around this new field, the kinds of questions they were asking, and how they fell into social, institutional and intellectual groupings. Approaches and interpretations were diverse and often contradictory, and this would nurture debate in the future.

Chapter Three uses the eolith debates (1880s-1920s) to explore broader expectations and approaches that influenced interpretations of industrial patterning, and stimulated dissent. It also examines the social and intellectual context of research: the way in which different aspects of Palaeolithic research were divided between different research groups (particularly archaeology and geology), and the use of social strategies in gaining academic consensus on industrial definitions.

Chapter Four looks at the work carried out between the early 1910s and the early 1920s at Swanscombe and Clacton-on-Sea, where a flake-dominated industry later described as the 'Clactonian' was discovered. Two areas have been selected for particular consideration. First, the institutional context of research has been examined: the kinds of interactions between employees of various different institutional bodies, and the influence of employment on research strategies. Second, we have observed how reactions to discoveries of flake industries, both at these sites and on the continent, contributed to the development of new interpretations of Palaeolithic patterning. The reception of flake-dominated industries was connected to an important shift in perception from a single unilinear sequence of British Early Palaeolithic industries to a vision of contemporary, parallel industrial streams, which had important implications for interpretation.

Chapter Five examines the reasons why the parallel culture scheme offered by Henri Breuil in the late 1920s and early 1930s had such a great influence over British Palaeolithic studies. This analysis of the rise and fall of Breuil's version between the 1930s and the 1950s explores why his scheme was taken up, how his theories influenced interpretations of industrial patterning, and the way in which these interpretations were connected to broader developments in British Quaternary research over this period. The discussion returns to the Clactonian industry at each stage of the argument, using the Clactonian as a touchstone for a more detailed assessment of how these developments influenced interpretations of the British Palaeolithic.

Chapter Six gathers together the discussions of the preceding chapters in order to draw some conclusions about the driving forces behind the interpretations of the British Early Palaeolithic over the period under discussion. Certain persuasive assumptions and expectations structured interpretations between the 1880s and the 1950s. However, the character and direction of the arguments differed enormously with the aims and scale of research, and with the research context within which these individuals were working.

## CHAPTER 2

### **Introducing the late nineteenth century Palaeolithic research community, their assumptions and their expectations**

‘Men observe only those things which their occupations or their education enable them to understand or appreciate’ (Lane Fox 1867, 612).

‘What is the difference between temptation and geological time? The one is a wile of the devil and the other is a devil of a while’ (Dawkins to Hughes, March 17<sup>th</sup> 1870: SMC: TMH).

The existence of our ancestors in geological times, before the time of the Romans or even of the British natives described by Caesar, only became a widely accepted notion in the mid-nineteenth century when a series of papers on the subject were delivered to various learned societies of Great Britain. The sequence of events that led to these presentations on the part of researchers like John Evans (1860), Joseph Prestwich (1860a) and Charles Lyell (1859) have been addressed in detail elsewhere (Gruber 1965; Grayson 1983; Riper 1990; 1993), and need not be repeated here. However, rather less has been published on the decades that followed, when researchers from a great variety of social and intellectual backgrounds converged on the problem of how to interpret these chipped stone tools, discarded so long ago, which now lay mingled with the bones of extinct creatures like the mammoth in British river-gravels and caves.

This chapter will explore the events between the 1860s and the 1890s. During this period, the discrete discipline of British Palaeolithic archaeology that we know today did not exist, and a framework for interpretation had to be gradually built up from a variety of fields that appeared relevant to the subject at the time. The individuals and groups who became interested in this problem, the questions they asked, and the assumptions and expectations which emerged in these early years would set the tone for future research and for future debate. This chapter is not intended as a detailed consideration of this complex period of social and intellectual development, but supplies a *précis* of themes that will feature in future chapters. To this purpose, the chapter has been split into the following four sections:

- The first section (2.1) provides an introductory sketch to illustrate the varied character of British Palaeolithic research.
- The second section (2.2) examines a variety of approaches to palaeolithic tools offered by British researchers and by influential workers in France. An attempt has been made to identify common expectations or widely-held assumptions that continued to direct the interpretation of such artefacts in later decades.
- Palaeolithic research was also dependent on the Quaternary context of these artefacts, and this provides the focus for the third section (2.3), which looks at the development of relative chronologies and their application to the Palaeolithic record. The glacial chronology has received particular attention, as this would have a great influence on later interpretations of the British Early Palaeolithic.
- The fourth and final section (2.4) explores the social background of such research to see how it nurtured this varied range of approaches, looking at the institutions that offered employment and the societies which were invaded by members who wanted to discuss various aspects of this new subject.

## 2.1: Introducing the varied interests of the Palaeolithic researchers

It is very difficult to typecast the range of interested parties who converged on the youthful subject of British Palaeolithic research following the establishment of human antiquity in the mid-nineteenth century. They emerged from various social backgrounds, had different questions to ask of the record, and brought with them a range of expertise. Before meeting a few of these individuals, brief mention must first be made of a recent effort to distinguish between two different research agendas that were applied to the British Palaeolithic record. Bowdoin van Riper (1990; 1993) argues that whilst geologists such as Joseph Prestwich or James Geikie worked on the chronological question of when humans first appeared in Europe, another group that he calls the ‘geological archaeologists’ were trying to understand the Palaeolithic in cultural terms – for example, through the use of ethnographic analogies – although they were also interested in the chronological question and drew heavily on geological and palaeontological methods (Riper 1990, 227-228, 232-233; 1993, 206-207).

This terminology, though attractive, has not been adopted, as it seems that there was too much diversity between individual approaches around this time to split a party of main Palaeolithic players off from those Quaternary specialists working on the chronology. Although many researchers were undoubtedly more concerned about dating and ordering the Palaeolithic, certain geologists took a deep interest in cultural aspects of Palaeolithic research. William Boyd Dawkins, on the other hand, one of van Riper’s geological archaeologists, tended to view the Palaeolithic tool-makers as just another marker species within his Quaternary chronology: a defining characteristic of van Riper’s ‘geologists’ (Riper 1993, 206).

Van Riper (1990, 237-239) has also suggested that the contribution of geologists to Palaeolithic research waned through the nineteenth century as early humans became more clearly situated within stratigraphical and palaeontological context. However, many of those described by their peers as ‘geologists’, would continue to make important ‘cultural’ contributions to Palaeolithic archaeology well into the twentieth century (demonstrated by Hazzledine Warren’s efforts, described in future chapters). Indeed, in an interesting twist examined in Chapter Five, the fine-tuning of the Palaeolithic sequence meant that geologists of the twentieth century

would take up this sequence to assist the interpretation and ordering of the Quaternary deposits, thus maintaining the complex link between cultural and chronological interpretations of the Palaeolithic and research into the broader Quaternary context within which these artefacts were found. These are only minor points, but they illustrate one of the most important and influential characteristics of British Early Palaeolithic research: diversity, a point that van Riper has also remarked upon.

### ***2.1.2: Four brief vignettes***

The varied interests of four individuals who worked during this early period have been outlined below: John Evans, John Lubbock, Augustus Pitt-Rivers, and William Boyd Dawkins. These brief biographies will provide a general introduction to the varied social, institutional and intellectual backgrounds that characterised the practitioners and the practice of Palaeolithic research, areas that will be examined in more detail in the course of this chapter.

#### *John Evans (1823-1908)*

John Evans (Fig. 2.1), a paper-maker from Hertfordshire and acknowledged authority on palaeolithic implements, certainly drew a clear distinction between the archaeological analysis of artefact character and the geological task of assessing the antiquity of the deposits (Evans 1863, 81; 1877-78, 150). However, Evans was also a competent geologist – indeed, he first met Joseph Prestwich on the way to a water-rights case in which they had both been appointed as geological experts, but on opposing sides (Joan Evans 1943, 89). Evans was perhaps a little unusual in the specificity of his later focus on Palaeolithic implements. According to Hugh Falconer (palaeontologist and colleague of Evans and Prestwich),<sup>1</sup> when Evans rejected some flints found by Boucher de Perthes at Moulin Quignon as forgeries, de Perthes put this down to:

‘the maunderings of a well disposed, rather intelligent mind, gone a little agog from working too much in one groove’ (Falconer to Evans, 10<sup>th</sup> July 1863: FMF: HF, 363S).

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<sup>1</sup> Falconer, as Superintendent of the Suharunpoor Botanic Garden, India, during the 1830s, was also largely responsible for introducing the China tea plant to India. He observed, ‘My tea services are undeniable’ (Falconer to Cautley, 2<sup>nd</sup> May 1844, in Murchison 1868, xxx, footnote 2).

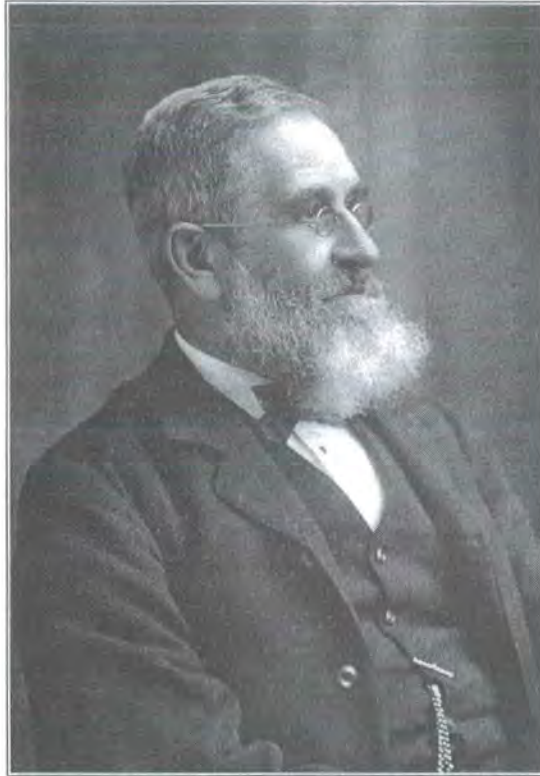


Fig. 2.1: John Evans (1823-1908) (Prestwich 1899, facing p. 104).

Evans was keen to encourage collaboration between archaeology, geology, and anthropology (particularly the first two subjects) in developing interpretations about the earliest appearance of humans (Evans 1860, 280; 1875, lxii; 1877-78, 150; 1897b, 458-459). He made such statements from a variety of platforms – to the Society of Antiquaries of London, the Geological Society of London, and the Anthropological Institute. Evans managed neatly to occupy the Presidency of the two former societies, and was proposed as President of the third by John Lubbock (Joan Evans 1943, 157).

#### *John Lubbock (1834-1913)*

John Lubbock (Fig. 2.2) was born into an influential family of bankers and gained an *entrée* into many prestigious societies through his early connections with Charles Darwin (Hutchinson 1914, 22-24). Lubbock was heavily involved in politics as a Member of Parliament, and was as interested in the habits of ants as he was in prehistory. Many of his contemporaries regarded him as a polymathic dabbler and synthesiser rather than an original thinker. Bonney, himself a thoroughbred geologist,

thought Lubbock ‘has too many interests really to excel in any one’, but admitted ‘He is much more distinguished in primaevial archaeology than in geology’ (Bonney to Sollas, 18<sup>th</sup> December 1909: UMO: TGB, 1909/5); and Herbert Spencer advised Lubbock, ‘Take warning from me and do not overtax your brain, which you seem in danger of doing by your multifarious avocations’ (Spencer to Lubbock, 12<sup>th</sup> June 1896: BLL: JL, Add 49662/46-47).



Fig. 2.2: John Lubbock (1834-1913)  
(Hutchinson 1914, frontispiece).

*William Boyd Dawkins (1837-1929)*

Boyd Dawkins, another polymath, was generally regarded as a palaeontologist, and he gained his Fellowship of the Royal Society on the basis of research into the Pleistocene mammalia.<sup>2</sup> However, he had also been employed as an Assistant Geologist on the Geological Survey, and later became the Professor of Geology at Owen’s College, Manchester. Dawkins managed to serve as President of both the Anthropology Section of the British Association in 1882, and the Geology Section in 1888 (Dawkins 1882; 1888; Woodward 1909, 530). Notwithstanding his great interest in anthropology, many of his interpretations of the Palaeolithic were coloured by his attempts to establish a Pleistocene chronology based on the mammalia, with the

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<sup>2</sup> This Royal Society Fellowship was awarded largely for Dawkins’ monograph of the more recent fossil mammals for the Palaeontographical Society, on which he had been working between 1866 and 1872.

palaeolithic tool-makers as yet another species of Quaternary fauna (Dawkins 1874, ix, 425). For Dawkins, the Palaeolithic was associated with a geological rather than a prehistoric age: his three important periods comprised the Pleistocene ('the equivalent of the Palaeolithic'), the Prehistoric, and the Historic (Dawkins 1874, 11; 1880a, 10).

*Augustus Pitt-Rivers (1827-1900)*

In contrast, Augustus Pitt-Rivers, known as Lane Fox before he came into his inheritance, claimed Palaeolithic archaeology as a legitimate research area for anthropologists, 'in whose science that of antiquity and ethnology are combined with physiology and geology' (Lane Fox 1867, 618). In *Primitive Warfare*, Lane Fox pointed out that the flint tools were found 'in situations, and under circumstances in which, alone, they would convey but little evidence to the antiquary'; it was the ethnologist, and the anthropologist who could 'throw a flood of light [...] to clear up the mystery which now hangs over everything connected with the origin of mankind' (Lane Fox 1867, 618). Lane Fox saw contemporary savages as survivals – so these were one kind of fossil, just as the hand-axes were another, which meant that there was a smaller distinction between the two disciplines in these early years (a point made by Peter Rowley-Conwy, pers. comm. 2003). Anthropology has changed tremendously since those days. It originally had more of a historical component, later lost by the European school. Hawkes noted how the *Congrès International d'Anthropologie et d'Archéologie Préhistoriques* began with both the archaeological and anthropological component being prehistoric. When the plural was lost after the Great War and anthropology became timeless, this caused great antagonism (Hawkes to Daniel, 11th May 1951: BLO: CH, 9). In the American school, of course, archaeology is still a branch of anthropology.

These four brief vignettes demonstrate how variable the approaches to the Palaeolithic could be. There seem to have been many different perceptions about what counted as 'Palaeolithic research' in the nineteenth century, a subject that was moulded by a variety of individuals nominally attached to a number of different research communities: prehistoric archaeology, Quaternary geology, Quaternary flora and fauna, ethnology and anthropology. These disciplines were also only just emerging as coherent research areas around this time.

Thus Prestwich, despite his devotion to Quaternary geology, felt able to make archaeological suggestions to an Anthropological Institute meeting about human antiquity (Prestwich 1877-78, 178). Flaxman C.J. Spurrell, well known for his work on refitting and perceptive suggestions about the manufacture of tools also wrote geological papers (e.g. Spurrell 1890); and Worthington George Smith took careful consideration of geological context (Smith 1882a) as well as archaeology (Smith 1883, 271). For the sake of convenience, the terminology used here has largely been taken from twentieth century disciplines. However, in the period under discussion, terminology was in a state of flux, and the boundaries between the disciplines had not yet congealed into the forms recognised today. Such diversity fashioned the character of Palaeolithic research; it also laid the foundations for dissent, as we shall see below.

## **2.2: Subdividing the Early Palaeolithic industries: assumptions, expectations, and the development of a chronology**

The acceptance of human antiquity in the mid-nineteenth century was accompanied by a rapid increase in reports of chipped stone tools from river-valley and cave deposits. These stimulated attempts to discover patterns and amass information that could reveal more about their makers. John Lubbock, for example, famously articulated the already popular distinction between chipped stone tools of the Palaeolithic period and polished stone tools of the Neolithic period (Worsaae 1859, 889; Lubbock 1865, 60; 1869, 74). However, more detailed subdivisions of the Palaeolithic would be suggested over the next century as workers tried to agree on where to draw distinctions between different groups of artefacts and justify the naming of different industrial epochs or cultures. An examination of the process of classification provides a revealing insight into the areas that were perceived as important or relevant to Palaeolithic research (see Douglas and Hull 1992, 2). This section will explore some of these efforts, introducing the picture of the Palaeolithic, the assumptions, and the expectations that would be inherited by future researchers.

The most famous Early Palaeolithic industrial succession created in the nineteenth century is generally associated with the name of Gabriel de Mortillet, who based his sequence on sites in France. However, an attempt has been made to look beyond de Mortillet's celebrated industrial framework and explore the reasoning behind other approaches to the record, in Britain and on the Continent. The research of future decades would be coloured by the most successful of the archaeological sequences developed during these early years, and would also be heavily influenced the strategies and perceptions that developed over this period.

### ***2.2.1: Lartet and Christy in the caves of France***

The early work of Edouard Lartet and Henry Christy in cave-rich France provides a general introduction to some of the industrial distinctions that were being drawn around this time. Edouard Lartet (1801-1871), a good friend of Hugh Falconer, the British palaeontologist, was better known for his palaeontological rather than his archaeological work. He is now best remembered in archaeological spheres for his

collaboration with Henry Christy,<sup>3</sup> and his 1861 relative chronology for Palaeolithic implements based on associated fauna. This faunal chronology was based on the idea that various distinctive animal species had made successive appearances into Europe, followed by later successive extinctions (Lartet 1861, 230-231). The use of such relative chronologies would become an important part of attempts to order the industries – and the development of various competing Quaternary chronologies will be examined later in the chapter.

In outlining his four-fold palaeontological classification, Lartet was fully aware of the problem that such palaeontological schemes, like archaeological cultures, were geographically restricted in their application, and only applied his scheme to France (Lartet 1861, 222, 231-232; see also Quatrefages 1881, 148). Nonetheless, his scheme was severely criticised for being over-generalised (Evans 1872, 433-434; Dawkins 1872, 419-420; 1874, 352; Mortillet 1873, 433-434; Boule 1888, 131-132). Dawkins noted:

‘Our present imperfect knowledge renders it impossible to subdivide the latest stage of the Pleistocene by means of the Mammalia, although the archaeologists may be able to establish a rude sequence based on a comparison of the implements and weapons found in caves and the deposits of rivers’ (Dawkins 1872, 421).

But long before Dawkins wrote those words, and before de Mortillet’s classifications appeared, Lartet had already noted such distinctions between the implements from the caves and the river-drift. Lartet, together with his colleague Henry Christy, made a number of early suggestions about the Palaeolithic sequence prior to the appearance of de Mortillet’s scheme. Their famous collaboration would produce the *Reliquiae Aquitanicae*, a detailed work on the French caves (Lartet and Christy 1865-75). But certain distinctions were already apparent in conclusions drawn before the first number of this enormous work appeared. In 1864, Lartet and Christy published an article based on their research in the Perigord, in which they distinguished between tool types from the river-drift site of St. Acheul (which was to become de Mortillet’s ‘Acheulian’, later renamed the ‘Chellean’); and the cave sites

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<sup>3</sup> Lartet and Christy began working on the Dordogne caves in 1863, another instance of collaboration between a palaeontologist and archaeologist, like the Falconer-Prestwich relationship.

of Le Moustier (de Mortillet's 'Mousterian'), Laugerie Haute (de Mortillet's 'Solutrean'), and Les Eyzies (de Mortillet's 'Magdalenian') (Lartet and Christy 1864, 234-235, 238-239, 255; Reinach 1889, 94; 1899, 86-87). The St. Acheul flints were assumed to be older than the Le Moustier artefacts, which in turn were older than those of the other cave deposits (Lartet and Christy 1864, 234-235, 238-239).

Lartet welcomed various British visitors to his cave-sites in the course of this research, so he probably shared these discoveries and opinions in person as well as in print. Hugh Falconer recalled the terrible heat of May 1864 when he departed from the Dordogne 'leaving M. Lartet to survive, if he can, another day's work at the Caves' (Falconer to Grace, 20<sup>th</sup> May 1864: FMF: HF, 119). John Evans also visited the caves in company with Lartet and Christy and, in an article published only in abstract, remarked on the relative antiquity of the deposits according to geology, palaeontology, archaeology, and comparison with other deposits nearby (Evans 1864, 444). Although Evans made no mention of archaeological conclusions in this abstract, he referred to Lartet and Christy's 1864 paper when formulating his own divisions in 1872. In his section on the chronology of the Caverns, his first two ages were the Age of Le Moustier and the Age of Laugerie Haute (Evans 1872, 435-436).

These events suggest a number of general conclusions. British researchers at this time were clearly involved in a wider sphere of interaction, and drew on the work of French researchers. The task of ordering the industries seems often to have been linked to the wider sphere of relative Quaternary chronologies, such as Lartet's palaeontological chronology – an aspect of Palaeolithic research that will be discussed later in the chapter. The problem of the regional specificity of classifications has also been raised in connection with Lartet's palaeontological scheme, and we shall see in later chapters how the overuse of purely local, but prominent, schemes would nurture major problems in the future.

However, turning to industrial classifications it seems that, first, Lartet held a largely-forgotten precedence over de Mortillet's scheme (examined below). Second, it appears that one of the most widespread divisions in the industrial classifications offered in these early decades was a distinction between implements from two sets of deposits: those from the caves (our Middle and Upper Palaeolithic), and those from

the river-gravels or 'drift' (our Lower and Middle Palaeolithic – the flake-dominated industry soon known more widely as the Mousterian was found in both caves and river-drifts). This distinction would have an important influence on future interpretations.

### **2.2.2: *The cave / river-drift distinction***

It was soon widely accepted that the difference between the implements of the caves and the river-drifts had a chronological aspect, although there seemed to be some overlap between the two. Peter Rowley-Conwy has observed how John Lubbock placed the caves of the Dordogne between the Neolithic period and the Palaeolithic of the river-drifts, although Lubbock recognised more ancient tool types at the site of Le Moustier that resembled those from the drifts (Rowley-Conwy 1996, 941; Lubbock 1869, 314, 321, 327).

John Evans also distinguished between the cave and river-drift tools. He suspected that some caves might be far later than the drift period, although he also noted that in other cases, cave and drift implements might be contemporaries, and that the differences were due to preservation bias or different activity *facies* (Rowley-Conwy 1996, 943-944; Evans 1872, 426-427, 574). The earlier cave tools (Mousterian) could, admittedly, be found in later drift deposits and drift types could also be found in the lower parts of the cave deposits (Dawkins 1880a, 194-196; Smith 1882a; 1882b, 579), but despite this overlap, such stratigraphical observations seemed to support the broader chronological conclusions.

Kent's Cavern, for example, was a cave that contained both types of implement, although the British Association Committee which had been directing excavations at the cave from the mid-1860s concluded that the two types were so distinct as to suggest their manufacture by two races widely separated in time. A particular distinction was drawn between the tools from the Breccia, which had been made directly on nodules, and those from the cave-earth that had been made on flakes purposefully detached from a nodule for that purpose (Lyell *et al.* 1873, 208). This chronological distinction between earlier core-tools (tools made using the original nodule or 'core' of flint) and later flake-tools would become increasingly important,

and would stimulate an interesting reception for the flake-rich, but Early Palaeolithic Clactonian industry, as we shall see in Chapters Four and Five.

The drawing of a cultural or biological distinction between the makers of different industries would also become a familiar aspect of Palaeolithic interpretation. Here in the nineteenth century, William Boyd Dawkins was drawing on his palaeontological expertise to reconstruct the former geographical ranges of the cave and river-drift peoples. He linked the widespread group of river-drift tool-makers to his southern group of fauna, and described how the more geographically-restricted cave tool-makers, associated with his northern group of fauna, represented a later incursion into Britain from a different area of Europe (Dawkins 1880a, 232-233).

### ***2.2.3: The river-drift industries of Britain***

Moving from the cave / river-drift distinction to subdivisions of the river-drift deposits, the early classifications to emerge from Britain were surprisingly diverse. Some were intended solely as descriptions of local sequences; others made a claim to wider application; and many were centred upon the implementiferous succession of the Thames Valley river-drifts. Some drew attention to differences in tool form, others pointed out the varied quality of workmanship; the question of chronological *versus* cultural difference was again raised, and a variety of suggestions were made regarding relative age. John Evans, for example, divided the river-drift implements into three divisions: flint flakes, pointed weapons, and oval implements with a cutting edge all round (for the latter two, see Figure 2.3), and he drew a clear distinction between the unifacially-worked flakes, and the great variety evident in the other two classes, which merged into each other (Evans 1860, 288-292; 1863, 75-78). This flake / hand-axe distinction, made by many others besides Evans, would set the tone for future discussion: distinctive hand-axes attracted far more attention than the apparently nondescript flakes, since 'there is little by which to distinguish them [the flakes] from similar implements of more recent date' (Evans 1860, 290).

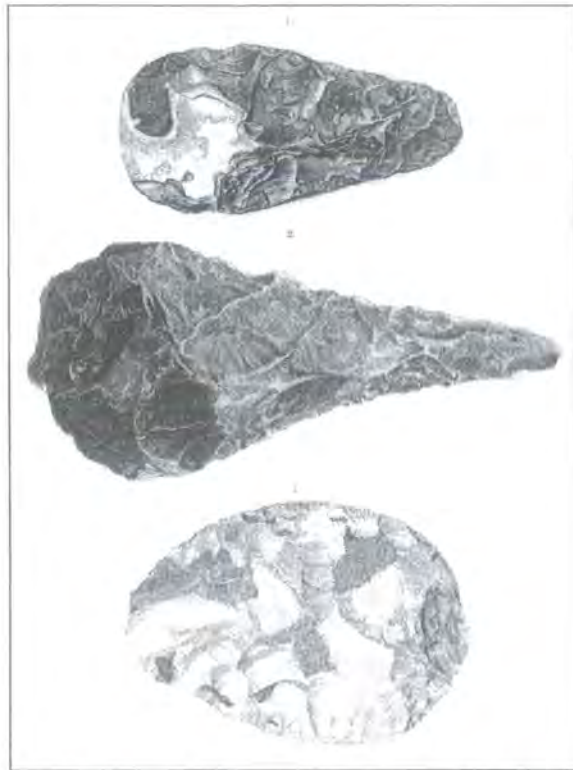


Fig. 2.3: Hand-axe types (Evans 1860, Pl. xv, facing p. 291). The upper two hand-axes are examples of Evans' pointed weapons; the lower hand-axe provides an example of the oval implement with a cutting edge all round.

Turning to the hand-axes, the distinction between ovate and pointed forms soon became widespread. However there was various ways to interpret this difference. Evans suggested a tentative chronological explanation, associating the flakes and ovate hand-axes with lower-level deposits, and pointed forms with the older deposits from higher levels (Evans 1863, 81-82). However, Evans and Lane Fox also observed out that this difference in tool form might (in the words of Lane Fox; 1868): 'have resulted from no more significant cause than a difference in the form of the flint nodule out of which the implement was made' (Evans 1863, 78; Lane Fox 1868, 410; 1872, 458). Lane Fox preferred this explanation as it followed his penchant for progress. He drew a distinction between the implements left rough at one end, and those with a cutting edge all round – two definite stages in an evolution from hand-held to hafted tools (Lane Fox 1868, 409-410).

Similar expectations seem to have characterised Worthington Smith's work in the 1880s on the Thames Valley succession at Stoke Newington: the concept of progression; the observation of different tool forms (ovate and pointed hand-axes);

and the distinction between the hand-axe industries and later flake-dominated assemblages. Although Smith was not explicitly attempting to set up a framework for the river-drift implements, he did note that his observations were not just true of Stoke Newington, or the Thames, but could be observed elsewhere in southern England (Smith 1883, 271).

Worthington Smith saw a definite progression 'from the large and rude, to the extremely small and neat scraper', which he thought suggested progress in the skill with which these tools were used (Smith 1883, 273). He also used staining and abrasion in describing his sequence of three distinct classes of implement belonging to three distinct geological periods. The first class of large, crude and abraded hand-axes, unassociated with trimmed flakes (Fig. 2.4), was followed by a second class of more finely made hand-axes (Fig. 2.5) often associated with scrapers. Both oval and pointed forms were represented in these two classes, and he described how they were succeeded long afterwards by the third class of smaller, lighter and neater implements of the Stoke Newington floors (Fig. 2.6), by which time scrapers were common, and the oval implements with a cutting edge all round, described by Evans, had appeared (Smith 1883, 271-272).

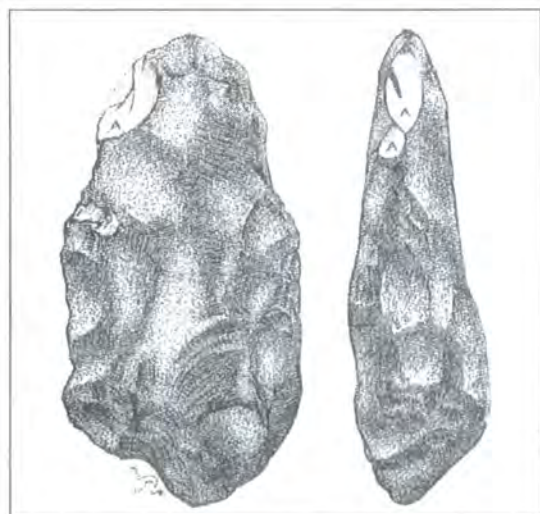


Fig. 2.4: The oldest class of Worthington Smith (Smith 1883, 272, Fig. 1).



Fig 2.5: The second class of Worthington Smith (Smith 1883, 272, Fig. 3).

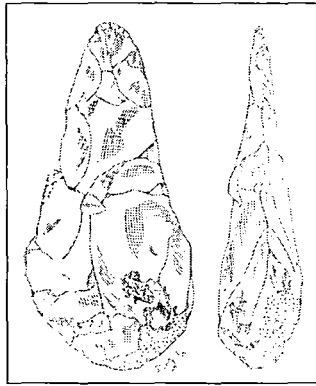


Fig. 2.6: The third class of Worthington Smith (Smith 1883, 274, Fig. 8).

#### ***2.2.4: The Palaeolithic sequence of Gabriel de Mortillet***

The same major structuring principles can clearly be seen in the most famous of nineteenth-century Palaeolithic classifications: the French scheme of archaeological epochs developed by Gabriel de Mortillet (1821-1898). Though based on observations in France, particularly the Somme Valley sequence, de Mortillet considered his scheme to have a broader application. The infiltration of his scheme into British research by the late nineteenth century compelled Evans (1897a, 482), rather grudgingly, to adopt it in the second edition of his *Ancient Stone Implements*.

By the twentieth century, as we shall see in Chapter Four, the terms used by de Mortillet for the river-drift industries had entered mainstream discussion of the British Early Palaeolithic through the mediating influence of research undertaken at sites like St. Acheul in the Somme Valley by Victor Commont. The fame of his Chellean-Acheulian-Mousterian industrial succession became assured. However, de Mortillet was drawing his classifications from the same pool of more general principles that were in widespread use at the time: the distinction between implements from cave and river-drift deposits, and between the hand-axe and flake industries of the river-drifts, and the concept of progression (which de Mortillet took to greater extremes).

De Mortillet systematized the casual distinctions of Lartet and Christy (1864) within a more explicit classification. He drew an analogy between certain distinctive Palaeolithic artefacts and the organic fossils used by geologists to link geological strata of similar relative age, using distinctive artefact-types, each named after characteristic localities, to define periods of time (Mortillet 1873, 436). This system

was outlined in a series of papers, the earlier ones focusing on the industries of the caves (such as Mortillet 1869), and the later ones gradually moving back to cover the earlier river-drift artefacts (Mortillet 1873; 1881; 1883).

De Mortillet clearly distinguished between an earlier epoch characterised by the hand-axe (which he variously called the *Acheuléen* or the *Chelléen*, for reasons outlined below; see Figure 2.7) and the later flake-dominated *Moustérien* (Fig. 2.8), with scrapers and points (Mortillet 1873, 436-437; 1881, description of Plate XI; 1883, 131). By 1900, the *Acheuléen* succeeded the *Chelléen* as a distinct epoch in its own right (Mortillet 1900, 21). These Palaeolithic epochs of Quaternary times were preceded by an Eolithic period of Tertiary times, when de Mortillet believed his hypothetical hominids, the *Anthropopithec*i, were making the crudest of all these chipped stones in Europe (Mortillet 1883, 248). Similarly ancient eolithic discoveries from Britain will be discussed in Chapter Three.

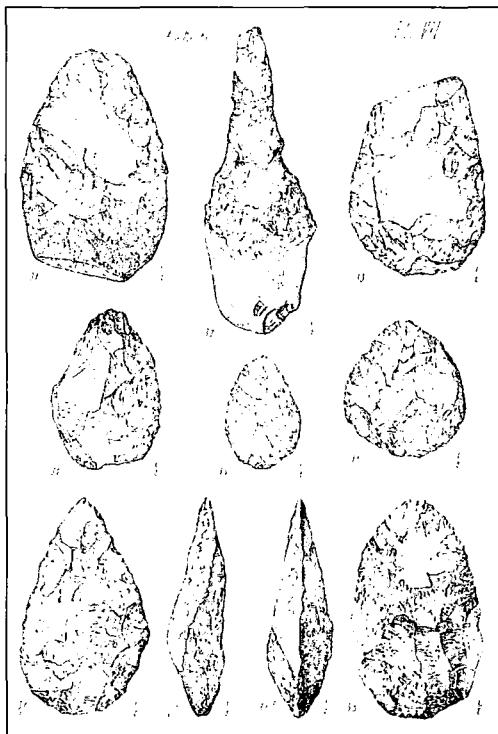


Fig. 2.7: 'Instrument chelléen' (Mortillet 1881, Plate VII). Some of these would soon be described as Acheulian.

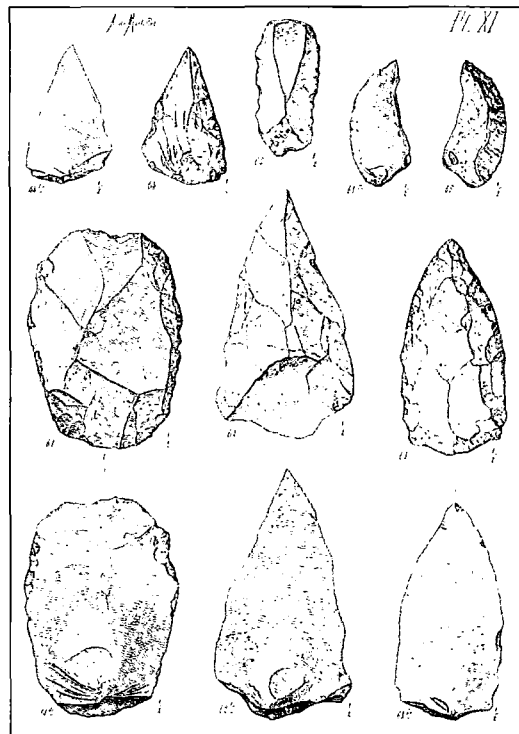


Fig. 2.8: Moustertian implements: flakes, points, scrapers and blades (Mortillet 1881, Plate XI).

Evolutionary progression, both in hominid races and in the tools that they left behind, was an important part of de Mortillet's vision of the Palaeolithic (Hammond 1980, 120). James Sackett and Nathalie Richard have studied de Mortillet's Palaeolithic classifications and noted how his assumptions of evolution and technological progress led to the development of a unilinear succession of time-specific industrial epochs, with little recognition of variation outside the defining artefact types (Sackett 1981, 85-86; 1991, 114-116; Richard 1999, 98-99). De Mortillet's gradual alterations to his definitions of hand-axe-dominated industries, outlined below, provide a clear illustration of such evolutionary and progressive concerns.

In 1873, the industry at the base of de Mortillet's Palaeolithic sequence was named the Acheulian (Mortillet 1873, 436). However, he later observed what he considered to be influences that were transitional to the Mousterian industry at the type-site of St. Acheul. These did not fit with his conception of Palaeolithic development, and drove de Mortillet to find a new type-site for his earliest Palaeolithic industry. The site of Chelles replaced the previous (and now impure) type-site of St. Acheul, and his old 'Acheulian' now became the 'Chellean' (Mortillet 1883, 132-133, 148).

This striking reaction – the fact that de Mortillet was driven to altering a type-site – becomes understandable in the light of his conviction that the hand-axe, though variable in form, size, finish and material, comprised the only implement of the earliest Palaeolithic industry '*elle se compose d'un seul instrument en pierre*' (Mortillet 1883, 133) ('it is composed of only one stone instrument'). At St. Acheul, scrapers were found alongside particularly thin and finely crafted hand-axes which indicated an advanced technological stage. With the Chellean now at the base of the Palaeolithic sequence, the industry from St. Acheul was reinterpreted as a transitional stage between the hand-axes of the Chellean and the flake-rich Mousterian (Mortillet 1883, 254; Mortillet 1900, 234). This sequence of events suggests that although de Mortillet saw the progressive development of hand-axe form and technique from simple to complex as an important indicator of relative age (Mortillet 1900, 155-156), the distinction between bifacially-worked hand-axes and the unifacially-worked flakes of the Mousterian was also central to his classification (Mortillet 1873, 436-437; 1883, 254-255; 1900, 162-163).

Despite the later fame of his classification, de Mortillet was only one of many who were working on the problem of subdividing the industries of the river-drifts, some of which have been outlined above. Many suggestions were being made around this time about how to reduce this variety into some kind of order and few were as pessimistic as d'Acy, a firm opponent of de Mortillet, who argued that all of de Mortillet's supposedly distinct industries were present within the same assemblage, and that the entire Chellean-Acheulian-Mousterian sequence should be encompassed under the all-embracing term of Chelleo-Mousterian (Mortillet 1900, 240; Déchelette 1908b, 461).

### ***2.2.5: In summary***

To conclude this overview, various attempts made over the latter half of the nineteenth century to order the chipped stone tools of the river-drifts seem to have been characterised by certain shared assumptions and expectations, reinforced by the parallel development of standard principles of classification. The preconceptions that developed over these decades would have a significant influence on future interpretations of the Palaeolithic, and three of the most prominent patterns identified in the above discussion deserve particular mention: the concept of progression; the strong hold that distinctive type-fossils such as the hand-axe had over classification; and the distinction between flake-dominated and hand-axe dominated industries.

Let us take these in turn. First, the idea of progression in tool-working skill through time, though taken to extremes by de Mortillet, was not unique to his scheme, but was part of a wider set of assumptions (Voget 1967), and would have a considerable influence on future interpretations. Second, the prominence of distinctive, typologically-attractive retouched tools such as hand-axes in classification, often at the expense of the less diagnostic flakes, would also guide later interpretations. John McNabb has described the idea of an evolutionary progression of distinctive tool types that could be used as zone-fossils as a 'conceptual lock', noting the enormous influence this had on restricting the range of possible interpretations of the Palaeolithic (McNabb 1996, 47). These two aspects will be explored more fully in the following chapters.

Third, a widespread distinction was drawn between the hand-axe industries of the river-drifts and the flake tools of the later river-drifts and the caves. On a similar line, a

distinction was also drawn between core tools and flake tools by the Kent's Cavern excavation committee in the 1870s, and repeated by Pengelly a decade later (Pengelly 1883, 558). Such divisions would lead to certain rigid links being drawn between the character of an industry and the expected age of that industry. We will see in Chapters Four and Five how this hand-axe / flake distinction would become a major focus for later classifications.

### ***2.2.6: Aside ... the use of ethnographic analogy in interpretations of the British Early Palaeolithic***

Before moving on to examine the early development of relative Quaternary chronologies, which would assist these efforts to classify and arrange these newly identified Palaeolithic industries but which would also set up constraints to future research, some mention must be made of the attempts to reconstruct the lives of these tool-makers. Although the discussion in this thesis will focus upon the development of industrial sequences, it must be remembered that more anthropological considerations also formed an important part of interpretations. The fields of anthropology and ethnology were plundered for analogies, and interpretations were split between the two scales of the Palaeolithic archaeologist: the moment of industrial manufacture, and the millennia that were encompassed by a palaeolithic 'industry'.

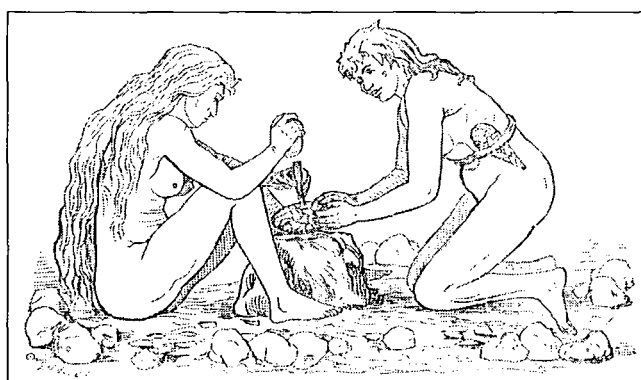


Fig. 2.9: 'Two persons producing the bold secondary chipping of Palaeolithic implements; a sketch founded on the method adopted by modern forgers' (Smith 1887, 89, Fig. 11).

Looking first at the smaller of these two scales, efforts were being made to reconstruct the methods used by past tool-makers through ethnographic analogy (Mackie 1861, 23-26; Evans 1872, 13). Some looked to exotic tribes; Worthington Smith used the

example of the local flint forgers of the Stoke Newington area, illustrated in Figures 2.9 and 2.10, to reconstruct tool-making techniques (Smith 1887, 87-89).

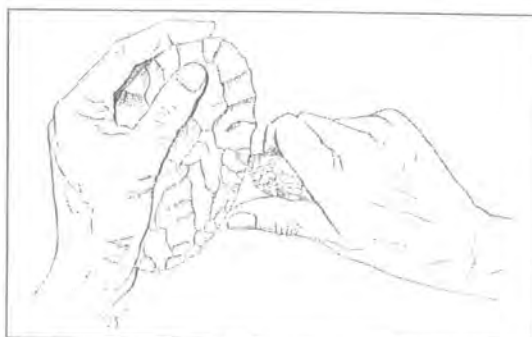


Fig. 2.10: 'Sketch showing how the tertiary or finest chipping was produced by the Stoke Newington forgers' (Smith 1887, 89, Fig. 13).

On the broader scale of the palaeolithic industry, different industries were soon associated with different groups or races of past tool-makers, and analogies for these Palaeolithic races were supplied by anthropological and ethnological research. We have seen above how Dawkins divided the cave and river-drift Palaeolithic into two different groups on the basis of their association with animal species of different geographical range (Dawkins 1880a, 232-233). Others also considered Stone-Age Britain to have been peopled by a succession of invading races, each having achieved a higher level of progress than the previous one (Allen Brown 1893, 95), and many agreed with Lane Fox that 'the existing races, in their respective stages of progression, may be taken as the *bona fide* representatives of the races of antiquity' (Lane Fox 1867, 618). The most popular choices were Tasmanians and Australian aborigines, usually for the Drift stage; and Eskimos, commonly for the cave stage (Lane Fox 1867, 616; Dawkins 1880a, 233, 244-245; Allen Brown 1887).

Palaeolithic research was clearly dependent on a range of related subject areas. The use of anthropological and ethnographic analogy to reconstruct past activities and affiliations of the makers of the chipped stone tools that had recently been recovered in such quantities from the river-drifts recalls Lane Fox's claim, mentioned earlier in the chapter, that anthropology could make an immense contribution to Palaeolithic archaeology (Lane Fox 1867, 618). Another point to note is that the idea of progression had once again been brought to the forefront, this time through the intermediary of such anthropological theories. However, returning for a moment to a theme of the previous

section, tools were not just the products of cultures to be interpreted through geographical patterning and ethnographic analogy; industries also provided the means to create a chronology for the Palaeolithic.

This is an immensely important point, because when the concepts of chronology and those of culture or race became linked together via distinctive tool forms, this provoked major problems for interpretation. Debate about whether industrial differences were due to the lapse of time, or were made by contemporary groups, has been mentioned in the previous section, and had characterised Palaeolithic research from the start (Evans 1872, 427; Lane Fox 1868, 410; 1872, 458). As we will see in future chapters, links between races and cultures would be used to defend the idea of a progressively-evolving linear sequence of industrial stages and explain away contemporaneous industrial variation, the reasoning being that if various migrating races brought the industries, then they might well appear in some areas before others (see for example, Sollas 1911, 120). However, such arguments also tended to weaken the value of industries as time-markers.

### **2.3: The development of a Quaternary framework for the British Palaeolithic**

A reliable, fine-grained, widely-applicable relative chronology was required to solve all these problems, but the various relative Quaternary chronologies were, like Palaeolithic research, still in their infancy. These fields of research would grow up together and influence each other's development. The possibility of using the newly-identified palaeolithic industries as time-markers would encourage a symbiotic relationship between the Quaternary specialists, who were trying to create and defend various competing relative chronologies based on different data-sets, and those who wanted to understand the cultural and chronological patterning of the British Early Palaeolithic. Quaternary chronologies and Palaeolithic industrial patterning gradually became more intricate, and we shall see in Chapter Five how the interplay between these different areas of research influenced interpretations of the Palaeolithic in the twentieth century. However, this section will take a look at the development of Quaternary chronologies in the decades before events grew more complicated.

The British Quaternary deposits only began to receive detailed attention from around the 1850s, when Joseph Prestwich (1812-1896) was publishing his researches on the Thames Valley (Fig. 2.11), and R.A.C. Godwin-Austen (1808-1884) was working on similar beds in southern England. Their findings and those of colleagues in Britain and France made a great contribution to the recognition of a more ancient human presence in Europe. After the establishment of human antiquity the Quaternary deposits were scrutinised even more carefully, since they now provided the context and framework for broader interpretations of Palaeolithic industrial patterning. However, the complexity of these deposits stimulated extensive argument through the nineteenth century as researchers tried to sort out the relations between the varied mix of scattered deposits and their palaeontological and archaeological contents.

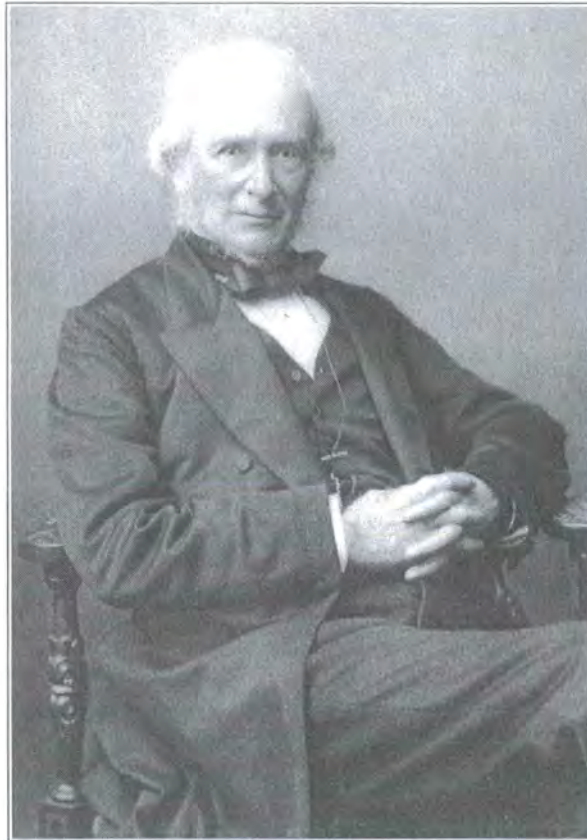


Fig. 2.11: Joseph Prestwich (1812-1896) (Prestwich 1899, frontispiece).

Even Prestwich, highly experienced in the intricacies of the Quaternary deposits, was often at a loss when it came to arranging them in the correct order of superposition. The diagram illustrated below was enclosed in his letter to Lyell (Fig. 2.12), together with the following despairing comment:

‘As for the exact order of succession they [the southern drifts] are so complicated that as often as I imagined I had detected it as often have I been thrown out again. When I think about it, some 3 or 400 sections & facts flit before me some tempting me one way & some another until I feel fairly bewildered. In the great coast sections the matter is clear enough but when we come inland the confusion is great’ (Prestwich to Lyell, July 6<sup>th</sup> 1859: ULE: CL, Gen 115/4931-4936).



Joseph Prestwich (Fig. 2.13), was undoubtedly one of the most notable of the nineteenth-century palaeontologists, and mention must also be made of the later researches by William Boyd Dawkins who leant heavily on Falconer's work. Their findings, and those of numerous other workers, were synthesised by authors such as John Lubbock (1865) in his *Pre-Historic Times*; John Evans (1872) in *The Ancient Stone Implements*; Charles Lyell (1863) in *The Antiquity of Man*; James Geikie (1874; 1877; 1894) in his controversial work on *The Great Ice Age*; and William Boyd Dawkins (1874; 1880a) in *Cave Hunting* and *Early Man in Britain*.



Fig. 2.13: Hugh Falconer (1808-1865) (Prestwich 1899, facing p. 110).

The amount of information on Quaternary deposits and their contents was expanding rapidly. However, this would not only enhance understanding, but would also provoke further debate over the correlation of several apparently incompatible relative chronologies: river-terrace stratigraphy, glacial geology, and Quaternary fauna. A flavour of these arguments will be given below, as these were often the source of disagreement over the patterning of associated palaeolithic industries, both in time and in space. It is important to understand the origins of such arguments, because the interplay and conflict between those working on different data-sets would become even more complex in the twentieth century, although the general reasons for disagreement remained very similar.

### 2.3.1: *The glacial chronology*

In the absence of absolute dating techniques or deep-sea coring, some way was needed to link local findings into broader schemes. Distinctive fauna, river-terrace sequences, or archaeological industries seemed to many researchers too localised in extent to form the basis of such correlations; but climatic oscillations arguably had a wider range. The distinctive cycles of climatic change would come to form the 'usual basis' of Pleistocene classification in the twentieth century (Warren 1924c, 265). However, the use of glacial-interglacial cycles as a master-sequence for dating industries and comparing industrial sequences from distant areas relied on two important developments. First, the old ideas of the Ice Age as a single advance and retreat of cold conditions had to be replaced by the idea of multiple glaciations; and second, the Palaeolithic had to fall within this frigid period of time. This section will examine the development of those two crucial ideas, before following the disagreements that arose over the number of glacial cycles, their reflection in the glacial and river-terrace deposits and in the faunal record, and their relation to the Palaeolithic industries.

The Ice Age, and its material reflection, the boulder clay, was originally envisaged as one immense stratigraphic marker, and the main question relating to Palaeolithic archaeology was simply whether humans arrived before or after this era (Mackie 1861, 30). From the early 1860s, most geologists assigned the Palaeolithic gravels to the 'postglacial era', by which they generally meant the deposits above the boulder clay. In 1860, following work carried out in collaboration with Prestwich in the Gower caves, Falconer delivered his conclusions on the postglacial age of the cave fauna to the Geological Society (Falconer 1868a, 531; Falconer in Murchison 1868, 589-590). The response was not initially favourable. Falconer warned Prestwich, 'You know what a fierce onslaught was made on me by Lyell and Austen. I thought the latter was going to eat me up' (Falconer to Prestwich, 2<sup>nd</sup> June 1860: FMF: HF, 340). However, Joseph Prestwich continued to back the postglacial age of the caves, and of the gravels and brickearths of the Thames Valley (Prestwich 1855, 110; 1864, 248, 251). This view rapidly gained support and by the time Lyell published his *Antiquity of Man* it had become the standard interpretation (Lyell 1863, 166; Evans 1872, 611-612).

William Boyd Dawkins, however, saw evidence for a different pattern in the Quaternary fauna, and his arguments introduce a certain tone of debate that would characterise much Quaternary research. It is important to note that the relation between the Thames Valley river-drifts and the boulder clay, which lay so thickly on the ground a little further north, would not be observed in a single geological section until the end of the nineteenth century. Although Dawkins agreed that most of the implementiferous river-drifts and the cave deposits post-dated the boulder clay, he argued from a palaeontological perspective that the fauna from the lower brick-earths of the Thames Valley – Crayford, Erith, Ilford, and Grays – belonged to a far earlier era, possibly the pre-glacial period.<sup>4</sup> Crude Palaeolithic flakes had been recovered from the former two sites in the 1870s, and Dawkins' argument would make these artefacts far older than the hand-axe dominated drifts (Dawkins 1867, 92; 1869, 206, 212; 1872, 413-414; 1880a, 136-137, 232; 1880b, 397-399).

However, Dawkins had many opponents amongst both the glacial geologists and those working on the stratigraphy of the Thames valley gravels (Prestwich in Dawkins 1872, 445; Whitaker 1889, 365). Attitudes had changed from the time when Falconer had noted that:

‘the pure Geologist, in most of his conclusions where age or climatal conditions are in question, is more or less at the mercy of the Palaeontologist, since he must accept the palaeontological evidence as it is laid before him, and square his speculations to fit and dovetail into the various mortices which the data inexorably present to him’ (Falconer 1868b, 1).

Searles V. Wood Jr., who championed a postglacial age for the Thames valley gravels (Wood 1866, 105), was at the Geological Society when Dawkins's 1867 paper was read. He reported to his friend Harmer ‘We had a great fight on Wednesday over Mr. Dawkins' paper on the Thames Brickearths’, and Wood was generally dismissive of what he called ‘the Geology of Palaeontologists’ (Wood to Harmer 11<sup>th</sup> January 1867: BGS: GSM1/542/51). William Whitaker (1836-1925), who had done a great deal of work on the Thames Valley stratigraphy, was similarly disparaging at the meeting. According to Wood, Whitaker remarked, ‘if the palaeontologists cannot make their palaeontology

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<sup>4</sup> These are now considered to be much younger than many other river-drift sites (Bridgland 1994, 234, 258-259).

square with the Geology so much the worse for the former!' (Wood to Harmer 11<sup>th</sup> January 1867: BGS: GSM1/542/51) Whitaker would later write:

'It is strange that whilst geologists are generally ready to give their full value to palaeontological reasons, yet palaeontologists sometimes show a tendency to dogmatise on geological matters, and to pass over stratigraphical considerations, or those which depend upon the character and position of the beds, as things of small importance, though the field-geologist may have worked long and hard at them. The two lines of evidence should be combined, though perhaps only stratigraphical evidence can decisively settle questions of the relative age of nearly related deposits' (Whitaker 1889, 335).

Before returning to the great question of whether the Palaeolithic tool-makers were about *within* the glacial period, which would not only make them very old but would also place their industries within the sphere of relative glacial chronologies, it is enlightening to take a more general consideration of this and other cross-disciplinary conflicts. Dawkins clearly found it difficult to reconcile the conclusions of geologists working on the physical geology of the Thames valley with his palaeontological conclusions, and, judging by the comments of Wood and Whitaker, geologists could be equally scathing about the contributions of palaeontological colleagues. Dawkins, for his part, remarked on the dangers of using terrace height as a guide to relative age (Dawkins 1867, 108); other palaeontologists were also cautious about the complex river-valley stratigraphy (Falconer 1868b, 191).

Attacks came from the side of glacial geology as well. James Geikie (1839-1915) (Fig. 2.14) contended that the terraces of the Thames river-valley were not discrete but tended to overlap and shade off into each other, and that the most that could be done in the way of classification was Prestwich's separation into a supposedly older high-level and a younger low-level series (Geikie 1894, 629). Like Dawkins and many other researchers, Geikie found that certain views of his colleagues engaged in Quaternary research were incompatible with his own area of expertise.

James Geikie had been amassing evidence to support the glacial theory of land-ice and interglacials. Around 1870, he began to build up his argument that the Palaeolithic river-drifts were deposited before postglacial times, basing his conclusions 'on grounds partly geological, partly zoological & partly common-sense-ical' (J. Geikie to Ramsay,

12<sup>th</sup> December 1871: ICL: KGA/Ramsay8/412/3). Geikie wrote to his friend A.C. Ramsay:

‘until English geologists lay aside their prejudices & candidly & impartially consider the evidence bearing upon the existence during the glacial epoch of long continued periods when genial conditions of climate prevailed, they will continue to blunder on about the postglacial age of Palaeolithic man. He was preglacial, as you insisted long ago; and he is also of inter-glacial age as I have tried to show, & never, in Britain at least, of post-glacial age’ (J. Geikie to Ramsay, 16<sup>th</sup> June 1877: ICL: KGA/Ramsay8/412/13; emphasis in original).

However, the perspective he developed in the course of this research did not harmonise with Dawkins’s arguments that the majority of the river-drifts post-dated the Chalky Boulder Clay, and Dawkins’s divisions of the Thames Valley fauna did not dovetail neatly with Geikie’s glacial divisions (Dawkins 1872, 426, 440). This would lead into a serious and revealing disagreement.

### **2.3.2: Geikie versus Dawkins**

The argument that developed between Geikie and Dawkins concerned the correct interpretation of the ‘curious intermixture of Arctic mammalia & beasts that have a southern warm habitat such as Hippopotamus’ (J. Geikie to Peach, November 1871: BGS: GSM1/321/32). Geikie took this ‘mixed’ fauna as evidence for successive glacial and interglacial cycles (Geikie 1872a, 169), whereas Dawkins argued that this represented a single, loosely contemporaneous group in a zone of overlap between northern and southern species, both appearing in such areas during seasonal migrations (Dawkins 1867, 104; 1872, 428-430; 1874, 413; 1877, 156).

From the start of the researches which led him to counter those who favoured a postglacial age for the Palaeolithic deposits, Geikie considered that ‘Dawkins’ papers on the subject are full of the wildest absurdities’, and his most frequent attack on Dawkins’s views was that southern mammals such as the hippopotamus could not have existed in postglacial times (J. Geikie to Ramsay, 12<sup>th</sup> December 1871: ICL: KGA/Ramsay8/412/3). Their disagreement escalated when Dawkins (1881a) published an uncomplimentary review of Geikie’s (1881a) *Prehistoric Europe* in *Nature* in February 1881. Dawkins announced: ‘Dr. James Geikie takes his stand upon the glaciated mountains of Scotland, and attempts to throw the glacial net woven in his previous work, “The Ice Age”, over the

whole of Europe' (Dawkins 1881a, 309).

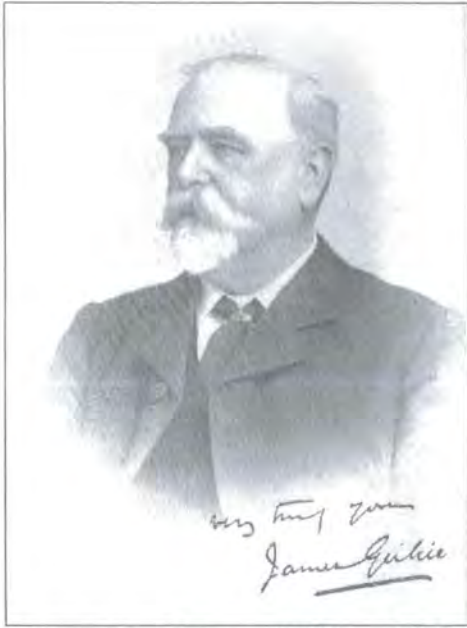


Fig. 2.14: James Geikie (1839-1915) (Woodward 1913, facing p. 241).



Fig 2.15: William Boyd Dawkins (1837-1929), reproduced courtesy of Birmingham Museum Services.

Geikie privately expressed the opinion that Dawkins was 'a nincompoop in physical geology' and 'a vain cocky humbug, who has endured so long simply because no one has been examining the evidence derived from foreign sources' (J. Geikie to Peach, 17<sup>th</sup> February 1881: BGS: GSM1/321/68). He confided to Ramsay that he was greatly surprised that Dawkins had been given his book to review in the first place:

'as it was notorious that my views were directly opposed to his, & that he could not therefore give an unbiased notice of my work. But a more unjust and mendacious notice I never saw' (J. Geikie to Ramsay, 27<sup>th</sup> February 1881: ICL: KGA/Ramsay8/412/15).

In an interesting twist, James Geikie learnt that the geological editor of *Nature*, who had let the uncomplimentary review through, was his brother Archibald (J. Geikie to Ramsay, 27<sup>th</sup> February 1881: ICL: KGA/Ramsay8/412/15). In his letter of reply to *Nature*, Geikie responded angrily to Dawkins' accusation that he had based his interglacial theories and his 'ice-classification' 'on ice, and ice only' (Dawkins 1881a, 310), throwing the criticism back at Dawkins:

‘Geologists rightly refuse to accept classifications which are based upon so narrow a foundation as a single series of phenomena, such, for example, as Mr. Dawkins’ attempt to classify the Pleistocene by reference to the mammalia alone’ (Geikie 1881b).

The controversy continued through later numbers of *Nature* (Geikie 1881b; 1881c; Dawkins 1881a; 1881b), a journal that Geikie accused of being a one-sided ‘kiss-my-arse of Macmillan & Co’ when the editor tried to avoid printing his next reply to Dawkins (J. Geikie to Peach, 9<sup>th</sup> March 1881: BGS: GSM1/321/69). He also informed Ramsay, ‘if it does not appear in next week’s *Nature* I will send it with additions to the Academy & Athenaeum’ (J. Geikie to Ramsay, 27<sup>th</sup> February 1881: ICL: KGA/Ramsay8/412/15; emphasis in original).

So what can be learnt from these explosive exchanges? It is revealing that both Dawkins and Geikie accused each other of having an overly restricted data-set, in terms of both the geographical regions covered and the Quaternary subject-matter tackled. The ideal was one of collaboration and conciliation, seen in the comments of Wood and Whitaker above, but in practice each researcher had developed certain presuppositions in the course of a more restricted line of research. The desire to create a general classification, usually one with considerable chronological overtones, seemed to encourage researchers to overextend schemes that had been developed from a restricted geographical area. Critics tended to pick up on this point, and their criticisms were generally voiced in terms of expertise derived from a different regional or research area.

This was not just the case with palaeontology or glacial geology: over-generalisation was a criticism which had been frequently levelled at temporal frameworks developed for Palaeolithic archaeology, perhaps most notoriously at the scheme offered by Gabriel de Mortillet. De Mortillet, it must be remembered, had dismissed Lartet’s palaeontological scheme for similar reasons, before replacing it with his own archaeological classification. This conflict between chronological generalities and geographical specifics would continue to afflict attempts to develop a framework for, and interpretations of, the British Palaeolithic.

### ***2.3.3: Pre-glacial or post-glacial Palaeolithic, multiple glacial phases, and the problem of regional variation***

Let us now return to the arguments between a postglacial or an interglacial appearance of Palaeolithic industries in Britain. Here, too, various different regional perceptions of the glacial epoch stimulated debate, and the lack of a standardised terminology provoked much confusion (Whitaker 1889, 387; Geikie 1894, 613). When researchers from different parts of the country referred to the 'pre-glacial' or 'postglacial' era, they could well be referring to very different points on a more general pan-British relative glacial chronology. It was often observed that conclusions drawn about whether humans were pre-, post- or interglacial varied with the area studied: southern England and East Anglia, or northern England and Wales (Tiddeman 1877-78, 169; Dawkins 1877, 159-160; Hicks 1886, 5-6; Smith 1894, 8; Whitaker 1889, 387).

Those working in southern England, such as Dawkins, tended to use the terms postglacial and pre-glacial for deposits that were above or below the Chalky Boulder Clay, widely regarded as the latest glacial deposit in southern England (Whitaker 1889, 377). Indeed, many saw this as the *only* glacial deposit in southern England.<sup>5</sup> As we have seen above, this boulder clay was generally considered to pre-date the deposition of the river-gravels of the Thames – thus making the Palaeolithic of southern England postglacial.

However, other researchers, many of whom were trying to build more detailed classifications of British glacial-interglacial stages, recognised additional, later boulder clays, particularly in deposits from further north (Geikie 1874, 474-475; 1877, xv). Geikie arranged these into a general British glacial chronology, with the Chalky Boulder Clay as one of the *earlier* glacial deposits (Geikie 1874, 426; 1877, 489). He and others therefore argued that the Chalky Boulder clay was only *locally* postglacial, and since it corresponded to areas in northern England which were interglacial, it should be described as such (Geikie 1872b, 220; Miller and Skertchly 1878, 532-535). As H.B. Woodward explained:

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<sup>5</sup> Work carried out by Bristow and Cox in the 1970s would lead to a resurgence of the view that there was only one widespread Chalky Boulder Clay in East Anglia (Bristow and Cox 1973, 15). Until recent work by Jim Rose (pers. comm. 2001, lecture at the University of Durham), this view still prevailed.

‘The Thames-Valley Deposits are no doubt later than the Chalky Boulder Clay, and are considered Post-Glacial in the sense of being newer than the latest glacial deposits in the district; but they are Inter-glacial if we include in the Glacial Epoch the later evidences of ice-action in North Britain’ (Woodward 1886, 129).

This point is crucial to attaining a clear understanding of the literature and the arguments of the time, which cannot be characterised simply as a question of monoglacialisists *versus* polyglacialisists. The problems did not just revolve around different interpretations of the *same* deposits; they were once again also closely tied to the *scale* of those interpretations. No consensus could be reached over a glacial chronology until the scale of interpretation, not just the number of glaciations, had been agreed upon.

#### ***2.3.4: Locating the Palaeolithic within the glacial period***

Meanwhile, claims for implements beneath the boulder clays were mounting. Hicks announced discoveries from the caves of North Wales (Hicks 1886, 18), and Tiddeman from Victoria Cave, Settle (Tiddeman 1877-78). Probably the most dramatic discovery, however, was Sidney B.J. Skertchly’s implements from beneath the Chalky Boulder Clay in East Anglia (Skertchly 1876, 476; Miller and Skertchly 1878, 547).

Skertchly, who was involved in mapping the Fenland Survey between 1869 and 1876 (Miller and Skertchly 1878, 543), announced the discovery of palaeolithic implements from pits at Brandon, Mildenhall, West Stow and Bury St. Edmund’s (Miller and Skertchly 1878, 548). Since these apparently came from beneath a boulder clay from the *south* of England, they were particularly important in the light of the terminological intricacies discussed above, and were seized upon by Geikie as evidence for an ‘interglacial’ arrival of the palaeolithic tool-makers (Geikie 1877, xiii). However, this aroused the same familiar arguments detailed above, and Geikie wrote despairingly to Ramsay:

‘English geologists are so conservative, and some of them are so wooden that there is no getting them to see a logical conclusion. Prestwich ignoring Skertchly’s work, reiterates that all palaeolithic beds must be post-glacial because all that he has seen rest upon “the boulder-clay”. Now the boulder-clay of East Anglia, (that near Brandon, etc.) has been demonstrated not to be the only boulder-clay. There is an older one, and there are two younger ones: all of which I have seen in actual sections over and over again. The mere occurrence of

Palaeolithic beds lying on the boulder-clay of East Anglia, therefore, is no proof that these palaeolithic things are younger than the glacial period' (James Geikie to Ramsay, 16<sup>th</sup> June 1877: ICL: KGA/ Ramsay 8/412/13; emphasis in original).

Prestwich later became temporarily convinced of Skertchly's finds (Prestwich 1887, 406-407), but others found the evidence from Wales and East Anglia less persuasive (Evans in Prestwich 1887, 408). However, for Skertchly and Geikie, another level of contextual detail had become available: a relative glacial chronology, within which the sequence of Palaeolithic industries could be placed. Skertchly noted that:

'the generally received opinion is that palaeolithic implements belong to one unbroken series – some a little newer or older than others, as the case may be, but still geologically speaking of one date. This, my own researches have proved to be far from the case' (Miller and Skertchly 1878, 534).

Taking advantage of this means of dividing up the Palaeolithic, Skertchly used his finds to describe three successive palaeolithic occupations of Britain (Table 2.1 below). This was substantially the same as James Geikie's view in 1877 (Geikie 1877, 489; J. Geikie to Ramsay, 16<sup>th</sup> June 1877: ICL: KGA/Ramsay8/412/13).

<i>Glacial deposits and archaeological phases</i>	<i>Glacial succession</i>
Neolithic Period	Post Glacial
Hessle Boulder Clay	Glacial
Modern Valley Palaeoliths	Inter-Glacial
Purple Boulder Clay	Glacial
Ancient Valley Palaeoliths	Inter-Glacial
Chalky Boulder Clay	Glacial
Brandon Beds	Inter-Glacial
Cromer Till	Glacial

Table 2.1: The glacial and archaeological succession in Britain (after Miller and Skertchly 1878, 551).

The Early Palaeolithic sequence was gradually being put in order. Many saw the large scale climatic shifts of successive glacial episodes as a more reliable way to create a relative chronology for Quaternary times than fauna, which were regionally variable, adaptable, and often migratory (Boule 1888, 132), or archaeological industries. Boule thought that there had been three major glaciations, and put the Chellean between his second and third glaciations (Boule 1888, 667). De Mortillet believed that the Acheulian and Chellean were pre-glacial (Boule 1888, 678). James Geikie's conclusions closely

followed those of Miller and Skertchly, outlined in Table 2.1 above.

By the end of the nineteenth century, while many English geologists were prepared to accept that there had been more than one glacial period (Marr 1896, 771), most still believed that the implementiferous cave and river-drift deposits were postglacial (Boule 1888, 678). A single deposit, the Chalky Boulder Clay, still dominated interpretation and was usually taken as a marker deposit that lay *beneath* palaeolithic levels: i.e. these were post-Chalky Boulder Clay (Evans 1872, 611-612; Dawkins 1881a, 309; Prestwich 1892b, 302-303; Smith 1894, 2, 7-8).

This was apparently supported by two important observations made late in the nineteenth century: first, the sections of Hoxne and Hitchin, observed in the late 1890s (Evans *et al.* 1896, 411; Evans 1897b, 461; Lubbock 1900, 323); second, T.V. Holmes' discovery of gravel above boulder clay at Hornchurch, Essex in 1892, which seemed to prove Whitaker's earlier theory of the post-glacial age of the Thames valley gravels (Holmes 1892, 370). Expectations of a postglacial Palaeolithic would be inherited by researchers of the early twentieth century (Halls and Sainty 1926, 89; Kennard 1916, 259; Boswell 1936, 149), and as we shall see in Chapter Five, this would severely restrict the kinds of analogies and correlations they could use to interpret British Palaeolithic industries.

Although a glacial chronology had little relevance for those who believed in a postglacial Palaeolithic, it is important to mention one last piece of research that would later be revisited alongside the sections mentioned above and offer the chance to link the glacial context of British Palaeolithic discoveries with Continental findings. This was the work of Albrecht Penck, a German glacialist who was comparing notes with James Geikie,<sup>6</sup> and was hard at work with Eduard Brückner on the glacial deposits of the Bavarian and Austrian Alps (Preller 1894, 29; Imbrie 1979, 115). Penck and Bruckner (1901-09) published the first volume of their seminal work *Die Alpen im Eiszeitalter* in 1901, in which they described four distinct glacial advances in the Alps. This was to

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<sup>6</sup> James Geikie was in correspondence with Penck from at least 1880. See the fifty-seven letters from Penck to Geikie in the Library of the Geological Survey, 1880-1914 (BGS: GSM1/321).

become an influential master-sequence in the 1920s and 1930s, when attempts would be made to correlate local Quaternary sequences and their palaeolithic contents, and to link British palaeolithic industries to popular Continental patterns (see Chapter Five). These later attempts would raise similar arguments to those discussed above, regarding the scale of interpretation and the relative reliability of different relative chronologies.

## **2.4: The social and institutional context of British Palaeolithic research**

Palaeolithic research had brought together a diverse range of researchers, and the efforts made to understand and order the chipped stone tools of the river-drifts drew on expertise from archaeology, anthropology, palaeontology and geology. However, the contributions introduced above were nurtured and directed by the social and institutional structure of nineteenth-century research, which will form the subject of this final section of the chapter. As a new subject, Palaeolithic research could boast no institutions or societies unique to itself, and the scattered patchwork of early connections to institutions and societies reflected and reinforced the varied nature of research.

### ***2.4.1: Employment for the Palaeolithic researcher in the nineteenth century***

The variety of approaches displayed by those working on the British Palaeolithic was partly stimulated by the range of available employment options and arenas for dissemination, and these provide an important insight into the context within which much research was carried out. The most popular fields of employment that were related in some way to Quaternary research comprised a few general archaeological posts in the larger museums, and an increasing number of geological positions offered by the Geological Survey of Great Britain and the universities.

A number of the researchers introduced above had at one time been employed by the Survey: William Boyd Dawkins (1862-1869), James Geikie (1861-1882), S.B.J. Skertchly (1869-1881). It was also quite possible to move from one field of employment to another, as William Boyd Dawkins found out when he left his post as Assistant Geologist on the Geological Survey (where he had worked from 1862 until 1869) to become Curator of Manchester Museum, before taking up the position of Professor of Geology at Owen's College, Manchester in 1872. The nature of employment had a great influence on the possibility and scope of research.

University posts offered flexible research options, but were not well paid and initially attracted those with independent means or a post elsewhere. A.C. Ramsay, geologist, glacialist, and confidant of James Geikie, received a salary of £100 when he occupied the Chair of Geology at Imperial College, London, so had to keep his job at the

Survey at the same time (O'Connor and Meadows 1976, 80). When Archibald Geikie became Professor of Geology at Edinburgh in 1871 (a post later occupied by his brother James), he also continued working at the Survey. This was standard practice – neither A.C. Ramsay nor Archibald Geikie had to ask permission from the Survey for this privilege, and the two positions were relatively compatible. As Geikie noted:

‘to take away all real cause of complaint I arranged that I should only lecture in winter when Survey duties keep me here at any rate, and that my lecture-hour should be 4 P.M. that is, after Survey Office hours’ (A. Geikie to Reeks, 31<sup>st</sup> July 1871: BGS: GSM1/320).

However, as the Survey expanded towards the end of the nineteenth century prospects of both promotion and pay were reduced. Many Survey staff, like Boyd Dawkins, were keen to gain employment in the expanding number of universities where there was more chance of promotion, even if many positions were just as poorly paid. In addition, the Survey had a history of publication restrictions, the legacy of a series of dogmatic Directors (such as Henry de la Beche and Roderick Murchison) who issued minutes and memoranda condemning the unofficial publication of Survey work (Ramsay to de la Beche, 13<sup>th</sup> November 1854: BGS: GSM1/420; Murchison to A. Geikie, 11<sup>th</sup> August 1867: GSL: 789/111). This could make it difficult for Survey staff to contribute to public discussion or win wider recognition in arenas such as the Geological Society. By the early 1870s it was noticeable that:

‘many men look upon the Survey now only as a step to something better, Dawkins for instance [...]. They come for their own convenience to get a practical knowledge of geology and a position, but with a fair determination from the first not to stop, and, just when they get to be really valuable, they leave, and a new man has to be trained (A.H. Green to Hughes, June 26<sup>th</sup> 1872: SMC: TMH).<sup>7</sup>

Meanwhile, prospects at the universities were growing brighter. William Sollas (1849-1936), soon to become the renowned author of *Ancient Hunters* (Sollas 1911), joined the Survey in 1893 whilst also Professor of Geology and Mineralogy at Trinity College Dublin. When he resigned in 1897 to take up the post of Professor of Geology

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<sup>7</sup> The same concerns were still expressed by disgruntled Survey staff over forty years later in a Memorandum, Henry Dewey being amongst the instigators (see Chapter Four). They complained, ‘the Survey is apt to be regarded less as a career in itself than as a stepping-stone to more remunerative appointments’ (Memorandum, 1918: BGS: GSM1/291).

and Palaeontology at Oxford, he was earning between 7 and 14 shillings a day as a Temporary Assistant Geologist. After his appointment at Oxford, the Survey temporarily re-employed Sollas to complete the petrological work in which he had previously been employed – now at a rate of 30 shillings a day (Treasury to Science and Art Dept., 2<sup>nd</sup> November 1897: BGS: GSM2/189). When the Treasury queried the basis for more than doubling the rate, Archibald Geikie, the Director General replied that petrography was a skilled subject, that Sollas and the other officer who left ‘could not be expected to remain with us at such low salaries, when any better appointments offered themselves’. Geikie, who had encouraged Sollas to take up the Oxford post, explained that he had failed to find any permanent replacements due to ‘the small salary and poor prospects of promotion of a Temporary Assistant’, and concluded:

‘The sum of 30/ a day is the smallest fee that could be offered to experts for their assistance. We could not expect or ask Mr. Sollas, now Professor at Oxford, to accept the totally inadequate remuneration which he received from us when on the Staff in Dublin’ (A. Geikie to J. Donnelly 5<sup>th</sup> November 1897: BGS: GSM2/189).

It is therefore not particularly surprising that individuals such as Boyd Dawkins and William Sollas wrote their famous syntheses, *Cave Hunting* (Dawkins 1874) and *Ancient Hunters* (Sollas 1911) in the more flexible environment of university employment. Even after earlier Survey restrictions on publication had faded, Survey work was very time-consuming. This correlation between the production of general syntheses and variety of employment should not be taken as an absolute rule (see the prodigious output of James Geikie, for example). However, in general, Survey employees put forward more restricted contributions to Palaeolithic research, both in the region studied and the variety of approaches used, which partly reflects the localised nature and restrictions of their work. Searles V. Wood Jr., who carried out a immense amount of work on the glacial deposits of south-east England between the 1860s and 1880s (although not for the Survey), remarked to one Survey employee, ‘the Survey men are never allowed to keep long enough to one part to master the subject properly’ (Wood to Hughes, November 25<sup>th</sup> 1869: ULC: Add 7652/V/P/19). He compared their efforts to:

‘Tailors working at a coat; one takes the skirt, another the sleeve, another the collar, &c; & they do not, so Hughes told me, even pay much attention, often none, to what their colleagues close by are doing; so that they do not grasp the whole of a subject’ (Wood to Fisher, May 11<sup>th</sup> 1869: ULC: Add 7652/V/P/56).

#### ***2.4.2: Non-professional researchers***

One large group of researchers still remains to be discussed – the so-called ‘amateur’ workers who observed and recorded a great deal of Palaeolithic archaeology revealed in the days of before mechanised gravel-digging (Roe 1981, 215), and studied the local geology in detail. Although employed neither in museums, nor the Survey, nor universities, they comprised a valued and integral part of the wider research community. John Evans, John Lubbock, and Augustus Lane Fox might be described today as ‘amateurs’. However, the division between amateur and professional in the late nineteenth and early twentieth centuries was not as we might regard it today; and the most important distinction was drawn between collectors and those who undertook careful observations of the conditions under which artefacts were discovered. The ‘mere collector’ made no record of the stratigraphical or palaeontological context of the palaeolithic implements they found or bought (Shrubsole 1885, 196; Hinton and Kennard 1905, 76) and gathered the magnificent hand-axes in preference to ruder flakes. Very different was the practice of workers such as Henry Stopes, Worthington G. Smith or Flaxman C.J. Spurrell; John Brown or Stanway and John Gunn in the field of palaeontology; or Searles V. Wood Jr. and F.W. Harmer in glacial geology, to name but a few of the more careful observers of the time.

Worthington G. Smith (1835-1917) observed that a false impression of the Palaeolithic was being created through the selectivity of collectors; he himself kept all flint fragments rather than just the larger impressive pieces and recorded their provenance in detail (Smith 1883, 274; Sampson 1978, 7). Implements were collected personally or from foremen and diggers at gravel pits. As Kennard noted, ‘The diggers were not overpaid, and they saved everything that might bring the price of a “pot” (not of tea), for they were thirsty souls’ (Kennard 1947, 273). Worthington Smith walked miles in his scruffy ‘Palaeolithic clothes’ (Smith 1888, 8), and became a familiar figure around northeast London where he kept a close eye on temporary exposures.

‘Once, when I was raking over a gravel heap at Stamford Hill, two labourers (both unknown to me) were sitting close by, when one said to the other, “Do you see that gentleman, Jack?” “Yes”, said the other. “Well”, said the first, “if you ever sees a heap of gravel anywhere, it don’t matter where, if you keep your eye on that heap of gravel long enough you will be bound to see that gent come and walk about on the top of it’ (Smith 1888, 10).

Henry Stopes (1900, 300) also carefully gathered all worked stones when he visited the Thames valley gravel pits. However, one of the most impressive researchers of this period was Flaxman C.J. Spurrell, whose retiring nature meant that his published output supplies a poor reflection of his great contribution (Kennard 1944, 162; 1947, 286). Spurrell was widely recognised to be the first to refit flakes to the cores whence they came in order to determine methods of workmanship (Spurrell 1883, 103; 1884, 110); the inspiration for Smith's own efforts in this field, one example of which is illustrated in Figure 2.16 below (Smith 1887, 83; 1894, 126).

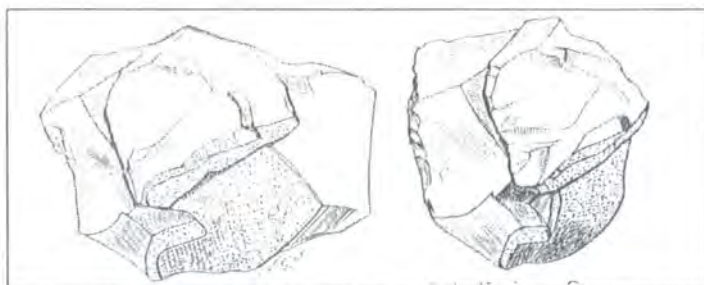


Fig. 2.16: 'Conjoined Palaeolithic flakes from near Stoke Newington Common' (Smith 1887, 84, Fig. 2).

Spurrell undertook such detailed observation that he even noted how the patterning of a small heap of flakes on a knapping floor at Crayford indicated 'that the operator *sat* on the sand with his legs but slightly apart' (Spurrell 1884, 112; emphasis in original). He also made an immensely detailed analysis of the twist on some small hand-axes, which he interpreted as the result of turning the tool during manufacture, even noting that the direction of the twist indicated a predominance of right-handedness (Spurrell 1883, 96, footnote).

The syntheses of the more renowned non-professional researchers, such as John Evans (1872; 1897a) in his lauded work *The Ancient Stone Implements*, were founded upon the detailed local efforts of many such individuals who were working on the palaeolithic archaeology and its wider Quaternary context. Their contributions appear in works such as James Geikie's (1874; 1877; 1894) *The Great Ice Age or Prehistoric Europe* (Geikie 1881a); or more anonymously in Lyell's notoriously plagiarised *The Antiquity of Man* (Lyell 1863; see Falconer 1863a, 1863b; Prestwich 1863; Lyell to Lubbock, 25<sup>th</sup> May 1865: ULE: CL, Gen 113/3813-3820; or Wilson 2002 for a more

hagiographic interpretation of Lyell's role).<sup>8</sup> Palaeolithic research leant heavily on the observations, collections, and interpretations of many individuals who had received no professional training or employment. However, before looking in more detail at the influence of their contributions, some consideration must be given to the various channels that were available for publicising this research. As we shall see below, the content, style and impact of research was strongly influenced by the arena in which that information was disseminated.

#### ***2.4.3: Societies, dissemination and debate***

Private discussion aside, one of the most accessible channels for pooling information and arguing over the respective merit of different interpretations was supplied by local societies. By the end of the nineteenth century there had been a great expansion in the number of provincial societies, and society journals became important repositories of information about temporary exposures in the area. At least they were expected to serve as such, although Whitaker found occasion to remark to a local Essex society: 'One of the most irritating sites to a field-geologist is a new railway-cutting carefully soiled over, of which no geological record has been kept. The Essex Field Club ought to have saved me from such irritation!' (Whitaker 1887, 180)

More prestigious, but also more exclusive, were the learned societies of the metropolis. Some early discussion of Palaeolithic archaeology took place at the Society of Antiquaries (Evans 1860; 1863) and the Anthropological Institute (Smith 1884; Spurrell 1884; Prestwich 1892a; see Stocking 1971, 375), but many intensive discussions also occurred at more geological forums. The Geological Society of London, founded in 1807, was the most prestigious of the geological arenas. Papers were welcomed on a variety of

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<sup>8</sup> Falconer was outraged at the way Lyell passed off the work of Prestwich and himself as his own in *The Antiquity of Man* (Lyell 1863). He wrote to his niece, Grace (who would later marry Prestwich):

'I charge him with deliberate appropriation of labours without acknowledgement, and of systematic disparagement of my discoveries to enable him to make an attempt of a shew where he absolutely had done nothing. I am not alone in the matter. Prestwich has been similarly treated' (Falconer to Grace, 14<sup>th</sup> March 1863: FMF: HF, 93).

The following week he attacked Lyell on these points in the *Athenaeum* (Falconer 1863a), which was soon backed up by a more mild-mannered public reproach on the part of Prestwich (1863).

Palaeolithic-related subjects: accounts of artefact discoveries (Prestwich 1861; 1889; Lane Fox 1872; Spurrell 1880) or, more particularly, their palaeontological or geological context (Dawkins 1862; 1872; Prestwich 1887; 1892b). A similar range of material was published in the *Proceedings of the Geologists' Association*, which provided a more open and less formal arena for the dissemination of information.

The Geologists' Association had been founded in 1858 with very low subscription rates, specifically for the encouragement of the varied interests of 'beginners' and non-specialists alongside the contribution of better-known geologists blessed with more time and money (Rupert Jones 1881-82; Kennard 1947, 271, 291; O'Connor and Meadows 1976, 79). The general perception was that members of the Geological Society tended to specialise on new geological theories and facts; whereas members of the Geologists' Association carried out broader consolidation: 'to add up their conclusions, and see what the specialists have done, whether they be of our circle, or belonging to other societies' (Rupert Jones 1881-82, 21). In addition there was the *Geological Magazine*, founded in 1864 by Henry Woodward, which also welcomed more local and controversial geological papers which promoted discussion and would not have been accepted by the *Quarterly Journal of the Geological Society* (Sheets-Pyenson 1982, 184).

This wide range of specialist institutions encouraged and nurtured a diversity of approaches to the Palaeolithic. However, as discussed at the beginning of the chapter, there was common ground within all this variety. Many prominent Palaeolithic researchers were equally at home at a number of different specialist societies. In addition, some societies were also actively encouraging broader discussion. The pattern of order and meaning that was founded upon the chipped stone tools from ancient geological deposits was tightly knitted together from geological, archaeological and anthropological threads of argument. This diversity encouraged misunderstanding and argument, but it also inspired new perspectives as these researchers were drawn together around the complex task of interpreting the British Early Palaeolithic record.

## 2.5: Conclusions

The scene has now been set for the next three chapters, which will present a more detailed analysis of approaches to the British Early Palaeolithic between the late nineteenth and mid-twentieth centuries. A varied research community had interested itself in the task of ordering and interpreting the Early Palaeolithic industries, and collaboration between individuals with interests in the artefacts and those who studied the wider context of these tools was set to continue. Non-professionals would remain an integral part of this wider research community, and contribute information in a variety of arenas alongside those employed by the Geological Survey, the museums or the universities. The tactics behind such dissemination will be explored in Chapter Three, and the institutional restrictions facing various different researchers will be studied in more detail in Chapter Four.

As far as the industries themselves are concerned, certain clear distinctions had been developed over the nineteenth century, surprisingly few of them linked to the famous name of Gabriel de Mortillet. The British Early Palaeolithic was characterised by a distinction between hand-axe dominated and flake dominated industries, and there was much support for the idea that workmanship grew increasingly skilled over time. This would provide a baseline for future work. In addition, successive industries were seen by some researchers as the products of different races of tool-makers who had wandered into Europe in turn over the Quaternary period, an idea that would play an important part in early twentieth century argument. As Early Palaeolithic industries became subdivided still further, the kinds of cultural and biological interpretations that were used to defend chronological expectations became increasingly complicated, as we shall see in Chapters Four and Five.

Some idea has also been provided of the convoluted arguments that took place between Quaternary researchers as they tried to order the complex and variable Quaternary deposits in the latter half of the nineteenth century using relative chronologies based on a number of different data-sets. Chapter Five will take up a more detailed analysis of the relations between the development of Quaternary chronologies and interpretations of the British Palaeolithic between the 1920s and the 1950s. By that time the typological and technological aspects of Palaeolithic artefacts had gained yet more

detailed chronological associations, and the industries themselves were commonly used as yet another relative chronology. The practice of using industries to refine geological interpretations, particularly the glacial-interglacial succession, would increase the mutual reliance between Palaeolithic interpretation and Quaternary debate described above. But regardless of such developments, much of the essential conflict of the early twentieth century seems to circle around similar polemical pivots to those of the nineteenth century.

The origins of one recurring theme can be traced to the nineteenth century and certain areas discussed above, and this centres upon the question of *scale*. This particular catalyst for debate can be caricatured as the irreconcilable problem of undertaking a detailed regional interpretation on the one hand; and developing a general, broadly applicable classification on the other, whether this happened to be based on palaeolithic industries, geology or fauna. Taking the example of palaeolithic industries, local observations of tool form and technique amassed from the 1860s onward were compared to other findings and used to build sequences. As these were extended to cover larger geographical scales, more generalised sequences were developed. These enabled spatially-scattered researchers to enter a broader discussion, but such generalised classifications were often criticised for stereotyping spatially-variable assemblages into a single generalised temporal scheme. Although there were many interpretations that lay somewhere between these two points, the extremes clarify some of the underlying causes of debate. The conflict between attempts at general classification and more specific interpretation can be seen in the criticisms levelled at Quaternary researchers who were developing faunal or glacial classifications (see Geikie *versus* Dawkins above). It can perhaps also be glimpsed today in the criticism and counter-criticisms of supposed (in fact, often caricatured) theoretical trends: culture-history, processualism, and post-processualism.

The various expectations of Early Palaeolithic industrial patterning that had been reached by the end of the nineteenth century would soon be shaken up by just such a shift in the scale of analysis. Chapters Four and Five will look at the varied responses to an expansion in the arena of research beyond the original localised areas of western Europe where the original Palaeolithic classifications had been based. The recognition of greater industrial variation stimulated a fascinating variety of interpretations that clarify the way in which arguments about Palaeolithic were constructed and promoted.

### CHAPTER 3

#### **The British eolith debates: expectations and arguments**

In 1912, the esteemed biologist E. Ray Lankester (1847-1929) was busy exhibiting some interesting and ancient flints at a *soirée* held by the prestigious Royal Society, when he became involved in an angry contretemps with William Boyd Dawkins. Lankester had introduced these recently-discovered flints, which he regarded as very early tools, to the academic world in a lengthy paper published in the *Philosophical Transactions of the Royal Society* (Lankester 1912a), in which he drew particular attention to a distinctive form: the ‘rostro-carinate’ flint. He recorded soon after this incident, ‘Dawkins was there and I made him go over them with me’ (Lankester to Moir, 9<sup>th</sup> May 1912: BLL: RL, Add 44968/154).

Dawkins proceeded to attack Lankester’s view that the flints in question had been flaked by human hands. Lankester recalled how Dawkins ‘idiotically said that such conchoidal fractures as they show could be produced by pressure’, and added that ‘he [Dawkins] could produce scratches like the glacier scratches by rubbing the flints with sand.’ Dawkins then placed the burden of proof on Lankester’s shoulders, saying “‘unless you can show that these flints could not possibly be produced by natural agencies, I shall refuse to attribute them to man’”. Lankester responded ‘that that was a preposterous & unscientific attitude’, and further informed Dawkins that ‘neither I nor any one who had studied the subject, attached any importance to his opinion!’ (Lankester to Moir, 9<sup>th</sup> May 1912: BLL: RL, Add 44968/154; emphasis in original)

This confrontation – one of many vitriolic exchanges over the East Anglian flints, or ‘pre-palaeoliths’ as their discoverer, James Reid Moir, named them – illustrates the debate over the eoliths in microcosm. Lankester was trying to describe what he thought was an important new archaeological industry and did not welcome the suggestion that he ought to be able to prove that these were not produced by natural agencies. Dawkins could only see evidence of natural chipping in these stones, and drew on recent research into the principles of flint-fracture to confound Lankester.

This chapter will explore the source of arguments over the eoliths and pre-palaeoliths as each side tried to convince the opposition of the archaeological or geological origin of these chipped stones. The kinds of expectations behind the interpretations of each side, the lines of argument used, and the way in which cases were presented in person and in print give a fascinating insight into the reasoning which lay behind a major archaeological debate. Many of the concerns raised by the eolith debates also characterised the theory and practice of palaeolithic archaeology in general, so the tone of these debates also provide an introduction to the social and intellectual background of Early Palaeolithic research explored in later chapters.

The main focus of this chapter, however, will be directed towards the arguments over the eoliths of East Anglia between their discovery in 1910 and a visit by the respected French professor, the Abbé Henri Breuil in 1920. Before setting off into early-twentieth-century British intellectual society and meeting some of those who became involved in the arguments over the pre-palaeoliths of East Anglia, a brief run through previous Eolithic claims from Kent and the Continent will place these debates within a broader historical and intellectual context.

### 3.1: Background to the eolith debates

In the early decades of Palaeolithic research, when this was still a new and uncertain subject, there was a widespread suspicion that older works had preceded the famous palaeolithic hand-axes of the valleys. Many thought that the creations of the earliest tool-makers would be crude and resemble the products of nature, and others argued that the oldest users of tools would not make them themselves, but would use naturally fragmented stone. Some cautioned that the earliest artefacts would not necessarily be found in Europe, following the theories of Darwin, Huxley and Wallace that the birthplace of humanity was in Africa or Asia (Dawkins 1874, 425; Smith 1894, 2-3; Hutchinson 1896, 15). Disagreements over the interpretation of crude stones from ancient deposits appeared inevitable.

The first major eolith claims emerged in France, soon after human antiquity had been generally accepted but when the extent of that antiquity was still unknown. When J. Desnoyers (1863, 1077) announced his discovery of cut-marked bones from Pliocene deposits at St. Prest in 1863, he aroused understandable interest amongst his peers – these bones came from a far more ancient stratum than previous discoveries (such as Lartet 1860). It was probably no coincidence that Hugh Falconer, the same year, wrote to a colleague working at Lexden with the following advice:

‘Let me recommend to you to give a glance over the surface of all the bones to see if any of them present knife or cut marks, or grooves made by a scraping implement, some instances of which have lately been turning up in deposits of very considerable antiquity’ (Falconer to Osmond Fisher, November 2<sup>nd</sup> 1863; ULC: Add 7652 II NN / 33).

In this expectant and fluid atmosphere it was not long before ‘implements’ themselves were also found in what seem today to be incredibly ancient deposits. In 1867, the Abbé Bourgeois presented his stone ‘tools’ from Miocene deposits at Thenay (Wilson 1899, 325-326), which were soon accepted by a number of his contemporaries, including Gabriel de Mortillet (Sollas 1911, 55). More claims followed and received a varied reception. De Mortillet accepted the Upper Miocene ‘tools’ found by J.B Rames in 1877 from Puy Courny, Cantal, and the Portuguese eoliths of Carlos Ribeiro, and finally cemented his support for the eoliths by including

Tertiary man in his 1883 synthesis, *Le Préhistorique Antiquité de l'Homme*, which extended his unilinear sequence of Stone Age industries back still further into the past (Mortillet 1883, 25-126; Newton 1898, 66-67; see Grayson 1986).

De Mortillet also coined terms for their makers, matching hypothetical hominid stages to his evolving cultural stages, which provided him with good ammunition for his evolutionist stance against those who were suspicious of Darwinian transmutation, such as Quatrefages (Quatrefages 1881, 89-128, 152; Hammond 1980, 120; Richard 1999, 96, 101). Thus *Anthropopithecus bourgeoisii* emerged as the maker of the ancient Thenay flints; *Anthropopithecus ramesii* as the more recent Puy Courny eoliths; and *Anthropopithecus ribeiroii* as the Tertiary Portuguese finds (Mortillet 1883, 105). However a number of dissenters ploughed through these waves of enthusiasm, amongst whom Sir John Evans was a notable and familiar figure who became known as 'the little St. Thomas' for his doubts at the international conferences where such matters were debated (Newton 1898, 66).

These early disagreements over Continental eoliths would have been familiar to the majority of those who entered subsequent arguments over the British eoliths. This brief outline also introduces certain aspects that directed the tone and character of these discussions: the influence of prior expectations on discovery and interpretation, and the importance of the social context of discussion in influencing the acceptance or rejection of interpretations.

However, it is now time to visit late nineteenth century Kent, the original homeland of the British eolith debates. A brief examination of the arguments surrounding the Kent eoliths will be undertaken first, to introduce some of the expectations that led to the discovery of what many believed was a true Stone Age industry in a little more detail. Some of the ways in which various different research areas were used in the interpretation, presentation and defence of the Kent eoliths will be suggested, and the reasons behind some of the varied responses to such discoveries will be briefly explored. The viewpoint will then turn from Kent to early-twentieth-century East Anglia, where a detailed background commentary supplied by private correspondence has enabled a closer scrutiny of the arguments that developed over 'pre-palaeolithic' discoveries.

### 3.2: The Kent eolith debates: an introduction to the broader context of Palaeolithic research

The story of the British eolith debates begins with Benjamin Harrison (Fig. 3.1) who ran the village shop in Ightham, Kent. In the early 1870s Harrison became interested in the cave-age implements of his neighbourhood. However, after meeting with a famous neighbour, he soon became absorbed in the more ancient prehistory of Kent. In the summer of 1879, a mutual friend introduced the diffident Harrison to Joseph Prestwich, Professor of Geology at Oxford, who lived nearby at Shoreham, and who is already familiar from Chapter Two. When Prestwich gestured towards the nearby valley features to explain where the equivalents of the high-level Somme gravels lay, Harrison realised that his palaeolithic finds from still higher levels suggested an even greater antiquity (Prestwich 1899, 248; Harrison 1928, 83-84).

Prestwich believed that the high-level drifts (around 100-ft. above the level of the Thames) were of fluvial origin, pre-dating current river drainage systems and found at a height above the Thames valley drifts suggestive of a great age indeed (Prestwich 1889, 273-276, 283; 1892a, 250). This was therefore a pivotal moment for Harrison. With Prestwich as friend, guide, and mentor, he now devoted time snatched from his livelihood to the search for high-level palaeoliths, hunting flints across the surface of the Downs, within temporary exposures and upon roadside stone heaps.

Harrison was soon finding what appeared to be increasingly ancient palaeoliths. In 1885, he moved his hunting territory from the high-level drifts up to the Plateau above, discovering his first Plateau palaeolith on 19<sup>th</sup> November the same year near the village of Ash on the North Downs (Harrison 1928, 110). Prestwich believed that the Kent Plateau drifts had been transported from hills a few miles away before the excavation of the intervening valleys in the glacial period (see Table 3.1 below). Harrison's discoveries therefore seemed even more ancient than his high-level finds (which he already thought earlier than the familiar river-drift types), and appeared to date to a pre-glacial or early glacial age (Prestwich 1892a, 250).



Fig. 3.1: Benjamin Harrison in 1898 (Harrison 1928, facing p. 216).

<i>Height and age</i>	<i>Geology</i>	<i>Archaeology</i>
older ↑	Chalk Plateau, covered in Red Clay-Drift of pre-glacial or early glacial age. 400-800-ft. O.D.	Plateau implements (Eolithic)
↑	High-level valley gravel drifts, c. 100-ft. above the level of the Thames. Postglacial	High-level or 'Hill' group (Palaeolithic)
↓ younger	Low-level valley gravel drifts, sloping down to Thames level. Postglacial	Palaeolithic

Table 3.1: Different age groups of Kent implements, based on physical geology, particularly different respective heights (based on Prestwich 1889, 286-289 and Prestwich 1892a, Fig. 1).

It was rather late in time that Harrison and Prestwich fully accepted the eoliths as true artefacts: Harrison in 1886 and Prestwich in 1888 (Harrison 1928, 133; see Fig. 3.2 for an illustration). Harrison's diary entry for 10<sup>th</sup> September 1890 recorded 'It was the dawn of the era of the eoliths, for on this day he [Prestwich] pressed me to take home specimens that only a few months earlier he would have regarded as too doubtful to be preserved' (in Harrison 1928, 155-156). By this time, the eolith debates of Britain had already begun.



Fig. 3.2: Three eoliths from one of Harrison's watercolour illustrations (Harrison, August 1902: PRM: Misc. Ms 11). Their respective provenances are 'S Ash', 'Pit 1898' and 'Maplescombe 795'. Reproduced courtesy of the Pitt-Rivers Museum, Oxford.

### ***3.2.1: The eolith debates in context: broader expectations of human antiquity in Britain***

The possibility of a greater antiquity for the Palaeolithic was a crucial question for the palaeolithic community around this time, and they had started to shuffle into positions of support and dissent over this question before the eoliths had made their appearance in Kent. Although Prestwich himself still maintained the postglacial age of Palaeolithic finds from river-drifts such as the Thames, he was also inspired to support an earlier pre- or early glacial arrival, having observed the geological location of these new discoveries of early palaeoliths and eoliths from Kent.

It should be of no surprise that James Geikie, supporter of interglacial and perhaps pre-glacial man (as seen in Chapter Two), was not particularly amazed at such discoveries, since he had already accepted that "Yes, palaeolithic man is *old*" (Geikie to Harrison 14<sup>th</sup> March 1892, in Harrison 1928, 175). More than a decade previously, on 2<sup>nd</sup> May 1881, Harrison's old friend and sparring partner Worthington G. Smith had received a letter from James Geikie that "staggered" him (B. Harrison in Harrison 1928, 91). He sent this letter on to Harrison, and Harrison read for himself the portentous lines:

‘They [palaeolithic implements] will yet be found in such deposits and at such elevations as will cause the hairs of cautious archaeologists to rise on end. I hope other observers will take a hint from you and search for palaeolithic implements in places which have hitherto been looked upon as barren of such relics’ (Geikie to Smith 1881, in Harrison 1928, 91).

Like Geikie, Harrison had also been stirred by Skertchly’s supposedly pre-glacial implements in the late 1870s, and went so far as to visit them where they lay on display in the Jermyn Street Museum (Harrison 1928, 98). However, upon suggesting to Evans that some of his own finds seemed older still, Harrison received the reply:

‘As to the implements found by Mr. Skertchly, I for one see no reason for attributing them to any pre-glacial antiquity. I believe them to belong to the same geological period as the others found in their immediate neighbourhood, and should give the Ightham specimens the same antiquity’ (Evans to Harrison, 7<sup>th</sup> May 1882, in Harrison 1928, 98).

When the eoliths appeared, they were interpreted in terms of the varied expectations regarding the extent of human antiquity that had developed in other areas of research. The positions taken in the eolith debates were relatively predictable, as they were drawn from the same pool of presumptions that had influenced opinions on earlier debates – indeed the ancient palaeoliths, and later the eoliths, were often used to support stances taken on other matters. But in turn, the reactions stimulated by the eoliths can provide a fertile source of information on such broader expectations, approaches, and research agendas, areas that will now be examined through a study of the presentation and reception of the Kent eoliths.

Harrison summarized the debates over the Kent eoliths in his poem *Eolith, Palaeolith – Nature or Man. That Little Chocolate Flint*, and this introduces the content of the arguments rather neatly:

'Eolith, Palaeolith – Nature or Man. That Little Chocolate Flint

How often we hear of the wrangles they have  
    over one little ochreous stone,  
How they say they can tell all its history long  
    from its chocolate staining alone,  
How some gravely proclaim it was made by a man  
    or at least by an anthropoid ape,  
While others maintain that in glacial moraine  
    it was licked by the ice into shape.

Some assert it was born on the Wealden Heights,  
    it was chipped, it was fashioned and used;  
Attached to a handle, in hundreds of fights  
    it hammered, it battered, it bruised,  
Till its owner grew tired of the Weald and removed  
    on some very remote quarter-day,  
And having no room in the furniture van  
    for his weapon, he chucked it away.

It rolled down the slope in a tertiary drift  
    from primitive Sussex to Kent,  
Its progress was slow, for a million or so  
    were the years on the way that it spent.  
But it got there at last and its troubles were past,  
    (like the days on the Wealden Heights)  
Though it bid very fair to come in for a share  
    of a second long series of fights.

For others declare that its story is false  
    that it never came northward to Kent;  
That the place where t'was found as it lay in the ground  
    was the place where its life had been spent;  
That it hadn't rolled down in a tertiary drift  
    ere the glacial period or since  
But had stayed all the time on the top of the chalk  
    in a layer of red clay and flints.

If only that Chocolate Stone could explain  
    what the dickens it did in the past  
That Sages might cease from exciting their brains  
    and the hatchet be buried at last  
Whether EOLITH, PALAEOOLITH – NATURE OR MAN  
    could they but of that question dispose  
Those eminent men might relinquish the pen  
    till a new controversy arose'

(Harrison to Lewis Abbott, 18<sup>th</sup> May 1898: BM(F): Misc. Doc Files: Eoliths;  
see also Harrison 1928, 218-219; capitals in original).

However, it is the character rather than the content of such wrangles as those described by Harrison that will provide the focus for the following brief discussion. The nature of the social and intellectual relationship between Prestwich and Harrison will be examined first, before turning to the case presented by Prestwich in the first three major papers on the Kent eoliths and their reception. Particular attention will be given to the use of archaeological and geological arguments in these three papers and subsequent interpretations on both sides. Although many researchers were involved in the eolith debates, the different approaches that they brought to the subject raise the question of how far they were actually engaging with each other's arguments.

### ***3.2.2: Prestwich, Harrison, and the presentation of their case***

By 1888 Prestwich had finished the second volume of his *Geology. Chemical, Physical, and Stratigraphical* (Prestwich 1888), and he now offered to describe Harrison's finds from Ightham. Harrison henceforth worked 'largely under his direction' (Harrison 1928, 128-130). Prestwich soon became the public face of the Kent eoliths, and although Harrison attended the most important society meetings, the papers were principally written and delivered by Prestwich. This division of roles is partly explained by their respective positions in academic society, but their relationship was also heavily influenced by personal factors.

Prestwich was held in great esteem for an unrivalled knowledge of the more recent geological strata. His was a weighty opinion: an established figure within the Quaternary research, particularly amongst geologists, Prestwich was well schooled in the cut and thrust of society debate. In contrast, Harrison, though in correspondence with some of the most prestigious of his contemporaries, such as Evans, Lubbock, and Worthington Smith, had not applied for membership of any of the learned societies (Harrison 1928, 59). Many of these were prohibitively expensive and Harrison was not a wealthy man. He could not afford to buy the latest books: his friend Worthington Smith lent him Evans's masterwork *Ancient Stone Implements* until Evans himself presented Harrison with a copy after a visit early in the 1880s (Harrison 1928, 82, 94). In addition, Harrison was hesitant of public speaking, being both diffident in personality (we have seen above how this delayed his meeting with Prestwich), and rather deaf (Harrison 1928, 59-60). It was this combination of social,

academic, and personal factors that led to Prestwich taking the lead in the presentation of the eoliths. When Prestwich read the first major paper on the palaeoliths and eoliths, Harrison, who was by his side, didn't catch a word (Harrison 1928, 143).

Prestwich presented the early palaeoliths and eoliths of Kent to his academic peers in three crucial papers (Prestwich 1889; 1891; 1892a). The use of geological and archaeological lines of argument on the part of Prestwich, Harrison, and their critics illuminate a number of differing aims and expectations which may have served to confuse the more obvious sources of disagreement still further.

Prestwich's first paper, *On the Occurrence of Palaeolithic Flint Implements in the Neighbourhood of Ightham, Kent, their Distribution and Probable Age* (Prestwich 1889), was delivered to the Geological Society where he was a prominent Fellow. For Prestwich, who fervently believed in the archaeological nature of the flints, the main aim was to establish the *antiquity* of the artefacts, both the ancient palaeoliths and those soon to be known as the Kent Eoliths, and to do so he focused on their geological context (Harrison 1928, 166; Grayson 1986, 90-91; Spencer 1988, 96). Prestwich argued that these discoveries came from drifts which predated the emergence of present river systems: they were far earlier than the postglacial Palaeolithic river-drifts of the valleys below, and hinted at a possible pre-glacial date for the arrival of the first inhabitants of Britain (Prestwich 1889, 292).

The ancient palaeoliths played a supporting role in this argument over antiquity, and emphasis was placed upon their deep brown staining, extent of wear, and rude appearance (Prestwich 1889, 286-288). In the discussion, the questions posed by John Evans and William Whitaker, the Survey geologist, followed the geological tone of Prestwich's paper. They questioned Prestwich's arguments for the age of the drifts and also queried whether these discoveries, found on the surface of the drift, were truly associated with these supposedly ancient deposits (Anon 1889, 51).

Prestwich soon bolstered his geological arguments in another paper read to the Geological Society early in 1891: *On the Age, Formation, and Successive Drift Stages of the Valley of the Darent; with Remarks on the Palaeolithic Implements of the*

*District, and on the Origin of its Chalk Escarpment* (Prestwich 1891). He now described the rude, worn Plateau group of implements in greater detail, again using their crude character as support for a great age: dominated by natural fragments of flint that had been worked at the edges, these ‘point to the very infancy of the art’ (Prestwich 1891, 134). Prestwich also supported his argument that the artefacts were indeed associated with the drifts by recording Harrison’s recent discovery of a palaeolith *in situ*, at a depth of two feet (Prestwich 1891, 133, footnote). In the discussion, Prestwich reminded the combatants that the lack of pits and excavations in the Plateau drift had restricted search mainly to the surface (Anon 1891, 37).

Now that the geological question had been put to rest, Prestwich delivered the more archaeological contents of his third paper to the Anthropological Society in 1891, *On the Primitive Characters of the Flint Implements of the Chalk Plateau of Kent, with reference to the Question of their Glacial or Pre-Glacial Age* (Prestwich to Harrison 5<sup>th</sup> February 1891, in Harrison 1928, 160; Prestwich 1892a). This was the first substantial presentation of the eoliths, and since he had little doubt that they were indeed artefacts, his main aim was ‘to enquire whether the character of the implements is in accordance with the early glacial or pre-glacial age, to which I would assign them’ (Prestwich 1892a, 249). Following a summary of his earlier geological conclusions, Prestwich indicated key differences in workmanship between the eoliths and the river-drift implements (summarised in Table 3.2 below) that supported a great age of the former (Prestwich 1892a, 250-252).

<i>River-drift implements</i>	<i>Plateau Implements</i>
Made from larger flints	Edges of fragmented natural flint are roughly trimmed into shape
Usually not very worn	Very worn
Skilled workmanship	Only slight trimming
Rude specimens belong to the same types as more finished specimens	Most show no special design and are rude / primitive in form
Pointed forms predominate	Only a few well-made specimens
Variety of colours, some are stained	Almost all are stained a deep brown

Table 3.2: Comparison of the river-drift and Plateau implements, indicating the greater age of the latter (based on Prestwich 1892a, 254-255).

Prestwich drew on the widely-held idea, explored in Chapter Two, that early tools would be crude since workmanship grew increasingly skilled over time. The older palaeoliths from Prestwich's Hill group, the precursors of his eoliths, fitted the expectations of many: they 'are not the work of prentice hands' (Hinton and Kennard 1905, 87; see also Moir 1919, 7, 30-31). He grouped the eoliths into three groups of types: those little modified; more definite forms (mostly types of scrapers); and a small third group of forms reminiscent of the valley-drift implements (Prestwich 1892a, 259-260). Harrison divided them according to possible use (Harrison 1892, 265-266). Heavy staining suggested an extensive interment within the similarly ochreous drifts; and wear suggested a long depositional history. These archaeological conclusions backed up the geological case for the antiquity of associated deposits.

The mixed reception that greeted this paper was partly influenced by positions taken on other research questions, such as the relationship of the Palaeolithic to the glacial epoch, as noted above. J. Allen Brown, author of *Palaeolithic Man in N. W. Middlesex*, and supporter of pre-glacial man (Allen Brown 1887, 33, 199) accepted Prestwich's conclusions (Allen Brown, cited in Prestwich 1892a, 274). However, they were rejected by both John Evans and William Boyd Dawkins (in Prestwich 1892a, 270, 273), neither of whom were enthusiastic about the idea of Tertiary man (Spencer 1988, 91, see also Chapter Two). Augustus Lane Fox, well known for his evolutionary views of industrial development was supportive, although he emphasised the need for *in situ* finds (Lane Fox, cited in Prestwich 1892a, 272-273).

### ***3.2.3: In summary: eoliths as archaeology and eoliths as fractured flints***

What do we learn from this brief discussion of the presentation and reception of the Kent eoliths? It appears that the questions directed at the eoliths were very similar to the criteria applied to the original case for human antiquity at the dawn of Palaeolithic archaeology: the age of the deposits, the association of the finds with those deposits, and the artefactual nature of those finds (Prestwich 1860b, 58; Lubbock 1862, 247; Newton 1898, 64). However, on each side, different personal expectations and areas of research expertise encouraged a variety of different stances. The response to Prestwich's third (1892a) paper illustrates some of the varied reasons for acceptance or rejection based on broader expectations of human antiquity and archaeological

patterning. Turning to Prestwich's own approach to these three questions, this was coloured by a fervent belief in the artefactual nature of the eoliths, and much of his discussion of the eoliths focused on their age and archaeological position, rather than a considered defence of their artefactual nature. For the opposition, however, such details were largely irrelevant.

For Prestwich, the main aim was to prove the remarkable geological age of the associated drifts. In order to support his argument that these surface-finds were of the same age as the ancient drifts upon which the majority were scattered, Prestwich emphasised the ancient character of the eoliths and called particular attention to occasional *in situ* finds. Harrison later directed a series of excavations in an effort to add more such finds to their list of eolithic discoveries (Newton 1898, 70). Although he accepted that crude implements might recur in later assemblages, Prestwich argued that the Kent forms were overwhelmingly crude, indicating a great antiquity (Prestwich 1892a, 253-254). However, the presence of crude palaeolithic tools alongside the eoliths was seen as a problem by Prestwich (1892a, 257), and by some of his peers (Harrison 1928, 193). Prestwich (1892a, 257) suggested that they were dropped by later tool-makers wandering across the Plateau. Many years later, O.G.S. Crawford disclosed a certain selectivity practised by Harrison in response to this worrying anomaly:

'undoubted hand-axes have been found on the highest points of the Kentish downs, in the deposits. I once had a fine small one but it was lost in the blitz of 1940. I have been told, I think by his son, that Benjamin Harrison found many such from the plateau gravels but concealed them because he thought they would detract from the interest of the eoliths from the same deposits' (Crawford to Oakley, 3<sup>rd</sup> October 1957: BM(NH): KPO, DF140/6; emphasis in original).

A similar selectivity was practiced by Prestwich and Harrison in their arrangement of these unstratified finds into a coherent series of groups according to workmanship, form and function, their appearance matching expectations for 'tools' of their great age. Harrison later described how he became inspired by Pitt-Rivers' work on industrial evolution, sending the renowned anthropologist, E.B. Tylor: 'some sketches of Eoliths ranging on to Palaeoliths as illustrated by my finds on the Plateau, led to do so by seeing Gen. Pitt Rivers Evolution of Cultures' (Harrison to E.B. Tylor,

14<sup>th</sup> November 1907: PRM: EBT, Harrison 11).

Harrison, Prestwich and others also drew a more general analogy between the eolithic culture stage and the crude tools made by the recently extinct Tasmanians (Prestwich 1892a, 258; MacCurdy 1910, 534), and Harrison remarked to Tylor: 'I can plainly see my rude specimens will just fit with your Tasmanians & feel sure there is plenty of material to work upon' (Harrison to E.B. Tylor, 26<sup>th</sup> April 1898: PRM: EBT, Harrison 3). Evans had enquired of Harrison:

'Has the absolute uselessness of such flints as tools never struck you, nor the fact that if the edge of a flint is chipped by hand it may just as well be made to present an acute as a right angle' (Evans, cited by Harrison to A.R. Wallace, 20<sup>th</sup> February 1898: PRM: Misc. Ms 11).

This led Harrison once again to ethnographic reports, and he suggested that some forms might be 'body stones' (see Figs. 3.3 and 3.4 below) as used in the East Indies and Australasia to 'rub the hard skin of the feet, remove scarf skin &c.' (Harrison to A.R. Wallace, 10<sup>th</sup> January 1898: PRM: Misc. Ms 11; Harrison 1904, 18).



Fig. 3.3: Drawing by Worthington Smith of a 'Combination Shin-scrapers' from Caddington, March 1906, sent to Harrison (reproduced from Roe 1981, 218, Fig. 3). The inscription reads: 'AM I NOT AN EOLITH AND A BRICKBAT'.

One of the paradoxes of the debates was that whilst the supporters of ancient tool-makers were expecting crude implements, their opponent took the crude appearance as support for their arguments that they had been formed by natural

processes. Whilst the opposition began to determine diagnostic features of flint-fracture indicative of human blows, the colith supporters tended to focus less on flaking attributes and more on the ease with which they fitted expectations and standard industrial classifications. Thus Worthington Smith was dismissive of ‘tools’ lacking a bulb of percussion and urged Harrison to concentrate instead on his Palaeolithic discoveries (Worthington Smith to Harrison, 12<sup>th</sup> April 1892, in Harrison 1928, 176), but Harrison and Prestwich promoted the archaeological nature of their tools by noting a close match to expectations based on industrial progress, reconstructing possible functions and drawing analogies to accepted industries. The interpretations of different camps were built upon different bodies of information, each following their own respective trajectories.



Fig. 3.4: Two coliths from a watercolour illustration sent by Harrison to A.R. Wallace (Harrison to Wallace 10<sup>th</sup> January 1898: PRM: Misc. Ms 11). No. 2 (above) is identified as a ‘Body Stone’ and No. 5 (below) as a ‘Double Pole Scraper’. Reproduced courtesy of the Pitt-Rivers Museum, Oxford.

One of the most significant points that has been revealed in this discussion of the Kent debates is that Prestwich and Harrison believed in the flints, so started to organise them according to their archaeological expectations, but the opposition concentrated on geology and natural fracture. Though they were arguing over the same specimens, the arguments developed by the two sides were not necessarily fired directly at the opposition – and this is one reason why the battle lasted so long. Such differences in the approaches of the two sides, particularly in the approaches to human workmanship and the perception of flint-fracture research and archaeological reconstructions, were one of the main driving forces of debate. These will be taken up in more detail in the discussion of the arguments over the East Anglian pre-palaeoliths.

### 3.3: The pre-palaeoliths of East Anglia: a case study of the complexities behind interpretations of the Stone Age

On 3<sup>rd</sup> October 1909, James Reid Moir (1879-1944), who usually spent his days helping his father run a successful tailoring business in Ipswich (Boswell, autobiography: ULLiv: PGB, D4/1, p. 37), found 'worked' flints in a stone heap of a brickfield near Ipswich. These pre-palaeoliths from East Anglia now entered the eolith debates. Like the Kent finds, their antiquity brought them great attention (see Table 3.3 below for the broader geological and eolithic context of the pre-palaeoliths). The pre-palaeoliths appeared to be far more ancient than the river-drift palaeoliths as they came from the eroded base bed of the Red Crag, beneath the Middle Glacial Sands (Moir 1911, 17-18).



Fig. 3.5: James Reid Moir (1879-1944), reproduced courtesy of the National Portrait Gallery.

<i>Geology</i>	<i>Archaeology</i>
River-drifts	Palaeolithic (Chellean, Acheulian, Mousterian)
Chalky Boulder Clay	Skertchly's Palaeolithic finds of the 1870s
Middle Glacial Sands and Gravels	
Cromer Forest Bed	Moir's 'implements', discovered in 1918: considered transitional between the pre-Crag rostro-carinates and the Chellean hand-axes (Moir 1921b, 395)
Red and Norwich Crag	The Norwich test specimen, discovered in 1911: Lankester's favourite example of the rostro-carinate type of sub-Crag 'implement' (Lankester 1914)
Suffolk Bone-Bed	Moir's sub-Crag, pre-palaeolithic 'industry', discovered in 1910
Kent Plateau drifts	Harrison's eolithic industry

Table 3.3: Summary geological section of deposits mentioned in the text (after Lankester 1913, Moir 1913b, 371 and Moir 1935, 26).

On Monday October 17<sup>th</sup> 1910, a wider audience might have learned of Moir's discoveries through his letter to the editor of *The Times* (Moir 1910, 8). Moir (Fig. 3.5 above) had already contacted the biologist and palaeontologist, Sir Ray Lankester (Lankester to Moir 15<sup>th</sup> May 1910: BLL: RL, Add 44968/1-2). Lankester had been interested in Harrison's eoliths, and was also an acknowledged authority on the Suffolk Bone-Bed, beneath the Red Crag, where Moir had found the pre-palaeoliths (Moir 1930, 140; 1935, 26). He agreed from the appearance and geological context of Moir's discoveries that these pre-palaeolithic flints were far more ancient than the Early Palaeolithic artefacts from the river-valley drifts, which at this time were widely regarded as postglacial (Moir 1935, 22). A storm of controversy blew up, initially over the age of the finds, which was soon cleared up when Moir made more discoveries from beneath undisturbed Red Crag deposits (Boswell, autobiography: ULLiv: PGB, D4/1, p. 38-39); and then over whether these were artefacts or naturally-fractured flints.

Once again, the arguments over the pre-palaeoliths involved a variety of approaches to Stone Age research. The approaches taken over the first decade of debate, between c. 1910 and c. 1920, will be explored below in some detail. This will build on the brief discussion above of the Kent debates and will contribute to a more detailed understanding of the social and intellectual context behind interpretation and debate in the early twentieth century. Discussion has centred on three main areas:

- First, flint-fracture research and experimental work (3.3.1). The approaches of sceptics such as Warren, Haward and Breuil are compared to the reaction and researches of Moir and Lankester to identify different stances and understand the reasons for such differences.
- Second, the archaeological arguments of Moir and Lankester (3.3.2). The aspects of pre-palaeolithic ‘industries’ selected by Moir and Lankester reveal deeper expectations of industrial patterning and approaches to classification, familiar from the discussion in Chapter Two.
- Third, the social strategies that were used to gain consensus for this variety of flint-fracture arguments and archaeological reconstructions (3.3.3). Interactions and tactics are evident in the selection of societies, journals, terminology, and the individuals singled out for confrontation. Particular attention has been directed to the way in which Moir and Lankester approached Boule and Breuil in 1912. This provides a fascinating contrast to the events that occurred on a later visit by Breuil in 1920.

Amongst other sources, the following analysis has drawn heavily on a valuable record of the collaboration between Moir and Lankester contained in three remarkable and edifying volumes of Lankester’s letters that Moir kept and deposited in the British Library, in which they discussed their expectations and strategies for presentation (BLL: RL, Add 44968-44970). The development of flint-fracture research has recently been examined in detail by Grayson (1986), who explores this aspect of the debate as an example of ‘middle-range’ research, and the reader is referred to his article for more information on how the task of distinguishing human from natural work became increasingly technical in the race for ever-more-convincing ‘scientific’ proofs. However, the aim below is to explore the *perception* of these debates: the expectations and reasoning revealed in the interplay between arguments based on flint-fracture research and those that focused on an archaeological interpretation of the disputed flints, and the social strategies that were used to promote the arguments of both sides.

### ***3.3.1: Flint-fracture arguments and the pre-palaeoliths of East Anglia***

When confronted with an *in situ* discovery of chipped stones from a geologically ancient deposit, the only remaining question that could be levelled by sceptics at those who claimed them as artefacts was whether these had been created by human or by natural forces. Such questions were not confined solely to the eoliths, but dated back to the dawn of palaeolithic research. They had been asked of Boucher de Perthes's discoveries in the mid nineteenth century (which did, admittedly, include natural specimens), and were still being directed towards discoveries in the gravels of the Somme and Thames Valleys in the 1860s. Even the finely finished hand-axes described by John Evans were considered by some to be the product of natural forces (see Whitley 1865, 33-39). The trade in forgeries that grew up alongside palaeolithic research also directed closer attention to flaking technology and characteristics of age such as wear, staining and patination (Falconer 1868c, 605-606 Evans 1872, 575-577). The question of human workmanship thus began as an archaeological matter (Evans 1877-78, 150), and discussion focused on the specific specimens of the disputed industries presented by their defenders. As we have seen, the Kent specimens were scrutinised by Worthington Smith and John Evans for bulbs of percussion, sharp cutting edges, and other features considered to be diagnostic of human workmanship.

However, by the time of Moir and Lankester's pre-palaeoliths, the character of this question had changed in a very interesting way (Grayson 1986, 87, 92). The problem of how to distinguish human from natural work now also embraced the study of *natural* processes of flint-fracture, due to growing suspicions that natural forces might also be capable of producing the features originally taken as evidence of human workmanship, such as the presence of a bulb of percussion (Mortillet 1883, 80-83; Abbott 1897, 91-95). In the early twentieth century, Samuel Hazzledine Warren (1872-1958), a geologist of independent means from Loughton, Essex (Fig. 3.6), called for more researchers to turn their attention to the principles of flint-fracture, explaining:

'It is not the geological position of the eoliths that is primarily in dispute, but whether they are, or are not, of human fabrication. Local field evidence cannot help us here; it is a knowledge of the fracture of flint under different conditions that we require' (Warren 1905, 337).

Moir would later take up the study of flint-fracture to assist his defence of the pre-palaeoliths, and this drew him into a revealing series of confrontations with more sceptical researchers like Warren. However, when Warren wrote the words above, his work on flint-fracture, and that of two French colleagues, Marcellin Boule and Henri Breuil, was directed towards eolithic claims from Kent and, more recently, from Belgium. Warren, Boule and Breuil would later bring their knowledge of this area to the pre-palaeolithic debates. Their early research will be introduced first before returning to Moir's work and their different approaches to the East Anglian flints.

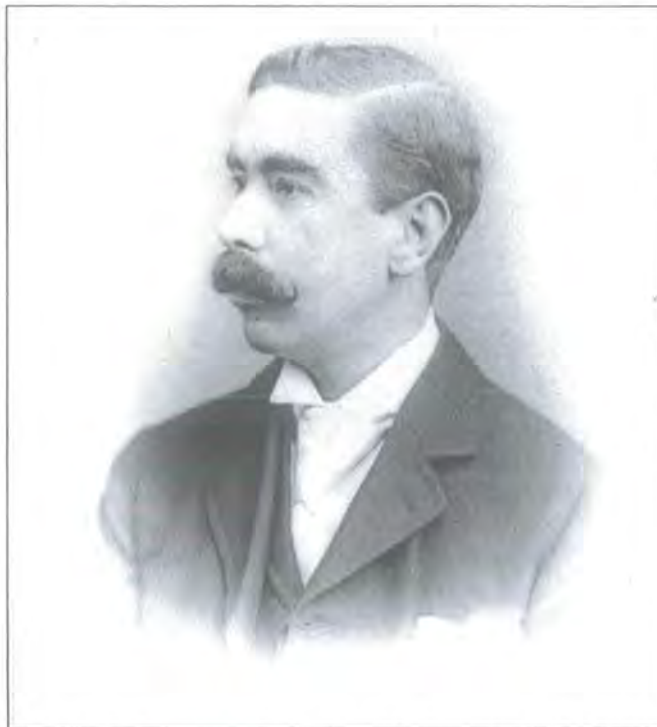


Fig. 3.6: Samuel Hazzledine Warren (1872-1958) (1907: *GSL: Portraits of Fellows of the Society*, Vol. 5, p. 35), reproduced courtesy of the Geological Society of London.

#### *Background to flint-fracture research*

In the late nineteenth century, Aimé Rutot (1847-1933), curator at the Natural History Museum of Brussels and 'chief exponent of the Eolithic cult, as it may be called, on the continent' (Dawkins 1910, 237), was promoting a series of industries from Belgium, found in what he believed were early Quaternary deposits. These ranged from 'Mesvinian' precursors of the Chellean (which we shall meet again in Chapter Four), back through the transitional Reutelo-Mesvinian (later the Mafflian) to the simpler tools of the Reutelian – and he later added Pliocene precursors (Rutot 1898;

1900; 1903; Grayson 1986, 94). With the exception of the Mesvinian (Sollas 1911, 111), these industries were regarded with suspicion. Three particularly influential papers were produced in the aftermath of the Kent and Belgian claims: Boule (1905) on natural fracture in torrents; Warren (1905) on his experiments; and Breuil (1910) on field observation of natural fracture under pressure.

In 1905, Marcellin Boule (1861-1942), geologist, prehistorian, and recently appointed Professor of Palaeontology at the *Muséum d'Histoire Naturelle*, Paris (Vallois 1941-46, 204) published an account of 'eoliths' produced by a cement works at Guerville, near Mantes, where the flint nodules from the chalk had been churned around in the water – much as torrents might have churned flints in the past (Boule 1905, 262). Boule's paper provoked Harrison to visit his own local brickyards where natural flints were ground up with the loam. However, of the few that had been flaked, although 'one or two even show bulbs', he concluded that these were different to humanly-struck flakes (Harrison to A.R. Wallace, undated: PRM: Misc. Ms 11).

More importantly, Boule's paper also stimulated Warren to publish his own flint-fracture researches (Grayson 1986, 104). Warren's (1905) paper *On the Origin of "Eolithic" Flints by Natural Causes, especially by the Foundering of Drifts* was delivered to the Anthropological Institute. He outlined six different processes by which eoliths may have been formed, described his pressure experiments, and concluded that different series of eolithic types did not represent cultures of different age, but reflected the geological conditions of the locality where they were naturally produced (Warren 1905, 342-359). Dawkins saw this publication as 'absolutely conclusive' evidence for the natural origin of Rutot's eoliths (Dawkins 1910, 237). Warren even made casts to demonstrate how the Kent eoliths were chipped by pressure. One of the specimens he attacked is illustrated below (Figs. 3.7 and 3.8).



Fig. 3.7: Cast made by Warren of one of the Kentish eoliths: 'Double Notch mounted to show how it was found', demonstrating how it was formed by sub-soil movement (PRM: Accessions Book No. X, p. 454; PRM 1940.12.161.1).

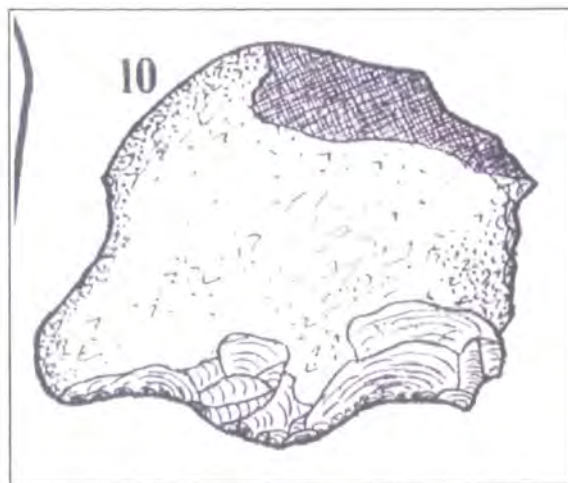


Fig. 3.8: Warren's illustration of his Kentish 'Double Notch', illustrated in Figure 3.7 (Warren 1920, 240, 243, Fig. 10).

However, it was a field observation from France that gave the final blow to Rutot's earlier Belgian industries, when the Abbé Henri Breuil (1877-1961), in 1910, announced his discovery of *in situ* natural-flaking from the base of the Eocene deposits of Belle Assise. A large number of these naturally produced flakes, some of which are illustrated in Figure 3.9 below, exhibited bulbs and fine retouch (Breuil 1910, 399-402).

Now that Boule, Warren, and Breuil had demonstrated that natural fracture could create extremely persuasive 'implements', there was no longer any excuse for complacency over diagnostic attributes. Their research aroused great interest. Breuil's Belle Assise observations stimulated William Sollas to insert a last-minute note into his *Ancient Hunters*, announcing that these had advanced investigation so far that the eoliths 'must now be regarded as the story of an exploded hypothesis' (Sollas 1911, 68). On the other hand, W. Allen Sturge, President of the Prehistoric Society of East Anglia, was doubtful of Breuil's conclusions, and referred the reader to the first article of the first volume of the *Proceedings*: Moir's first substantial article on *The Flint Implements of Sub-Crag Man* (Sturge 1908, 13; Moir 1911).

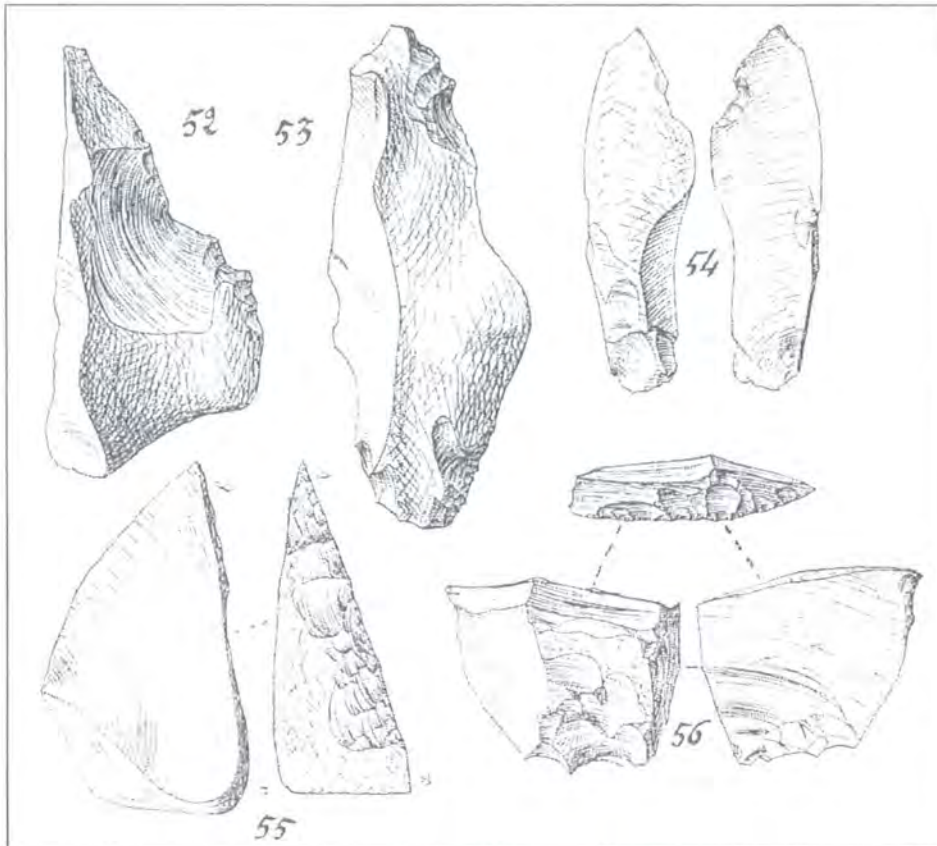


Fig. 3.9: Some of the Belle Assise flakings (Breuil 1910, 400, Figs. 52-56). Breuil particularly noted the bulb of percussion observable on Figures 54 and 55 (Breuil 1910, 399) and the retouch in Figure 55 (Breuil 1910, 401).

Moir and Lankester were aware of the potential response to their sub-Crag pre-palaeolithic finds from East Anglia, having seen the devastating effect of Boule's discoveries at Mantes and Breuil's observations at Belle Assise on the arguments of Aimé Rutot (Grayson 1986, 106). Alongside an understandable eagerness to construct archaeological interpretations of the pre-palaeoliths, Moir and Lankester also realised the importance of entering the debate on a similar level. A confrontation with Warren was almost inevitable.

*Moir, Lankester, Warren and Haward: diverse approaches to flint-fracture*

At the beginning of this chapter, an account was given of the argument between Lankester and Dawkins at a Royal Society *soirée* in 1912, where Dawkins dismissed the pre-palaeoliths on the grounds that they had been produced by natural processes (Lankester to Moir, 9<sup>th</sup> May 1912: BLL: RL, Add 44968/154). Moir suffered a similar experience at hands of Dawkins and Warren, who 'after seeing my specimens when

on exhibition at Burlington House, London, told me that they were undoubtedly the result of natural forces and nothing else' (Moir 1911, 19). Lankester was aware of the papers by Boule (1905) and Breuil (1910), describing the latter as 'a perfectly fair and good attack on reputed "Eoliths"' (Lankester to Moir, 30<sup>th</sup> May 1910; 2<sup>nd</sup> June 1911: BLL: Add 44968/5, 41-42). However, the application of such arguments to his own pre-palaeoliths by Warren and Dawkins was certainly not to his liking, and such confrontations appear to have driven Lankester into this area of research in an attempt to counter such arguments on similar grounds.

In 1912, Lankester wrote a letter to the editor of *Nature* pointing out the 'great need for a thorough study of flint', its origin, varieties and fracture (Lankester 1912b, 331), and he had long been encouraging Moir to take up his own flint-fracture experiments (Lankester to Moir, 30<sup>th</sup> May 1910; 1<sup>st</sup> July 1911; c. 1911; 12<sup>th</sup> October 1912: BLL: RL, Add 44968/7, 47-48, 102; 44969/2-3). However, when Moir finally came to publish his results (Moir 1911; 1912a), they were heavily criticised by Warren and his fellow researcher, F.N. Haward. Besides obvious differences of opinion over the origin of the pre-palaeoliths, Moir also approached flint-fracture research with different expectations to those of Warren and Haward. Grayson has assessed their respective approaches in some detail as early examples of 'middle-range' research (Grayson 1986, 106-115). However, the discussion below seeks to understand the differences in perception and intention that lay behind their work and stimulated disagreement.

Fred N. Haward (1871-1953), an engineer by profession, and Hazzledine Warren, introduced above, both became sceptical of the eoliths through observations of natural flint-fracture in the field. Warren first saw contact-flaking on the Isle of Wight and Haward in East Anglia, and this experience stimulated both to conduct their own flint-fracture experiments (Haward 1912, 185; 1919, 120; Warren 1900, 411-412; 1913, 38; 1923b, 162). It is difficult to disentangle the contributions of Warren and Haward. Friends and colleagues, they shared their ideas as well as their outlook, and came to similar conclusions independently (Haward 1919, 120). They took a rigorous approach to flint-fracture research, which they studied through experiment, observations made in the field where flakes had been produced by natural geological fracture, and a comparison of the attributes of such flakes with those

knapped by humans.

Warren's initial attacks on the Kent Plateau eoliths, described above (Warren 1905), were soon eclipsed by the sub-Crag debate. From 1913, publications of fracture experiments were pointed directly, though courteously, at Reid Moir (Warren 1913, 1914a, 1914b, 1923c, 1924b). Haward (1912, 188) introduced the concept of 'chip and slide' (chipping by lateral movement under vertical pressure, a stationary flint being flaked by a block moving over it: see Fig. 3.10 below), a principle incorporated by Warren<sup>9</sup> within his 'investigation of the elementary principles which underlie all flint chipping', published in the *Journal of the Royal Anthropological Institute* (Warren 1914b, 412).

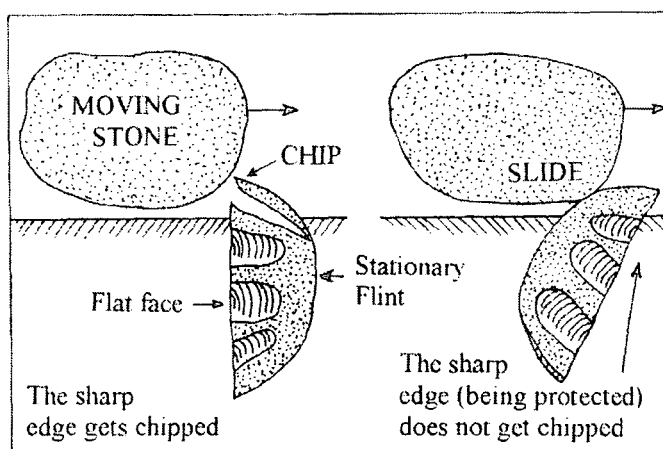


Fig. 3.10: 'Chip and Slide'. A demonstration of the conditions under which natural-flaking might occur (redrawn from Haward 1914, 352). Moving stones are forced against stationary flints by natural agencies, flaking the sharp edges of the flint.

The 1914 paper published by Warren gives an idea of the areas covered: natural edge-chipping provided the main focus, with detailed coverage of how form and condition could influence the result of natural-flaking; the various different natural agencies involved (heat, concussion, crushing, chip and slide), each of which had its own characteristics; and the general principles of fracture along planes of least resistance (Warren 1914b). Haward began his own 1914 paper *The Problem of the Eoliths* with a plaudit to experimenters: 'The fullest credit must be given to our

<sup>9</sup> Warren observed that although Haward had given a name to the principle of chip and slide, he himself had first suggested this idea in 1905 (Warren 1914b, 426).

members, Mr. Reid Moir, F.G.S., and Mr. S. Hazzledine Warren, F.G.S. (and others also) for their efforts to solve this problem by experiment' (Haward 1914, 347). But in addition to their different conclusions, these two experimenters brought different assumptions to their work.

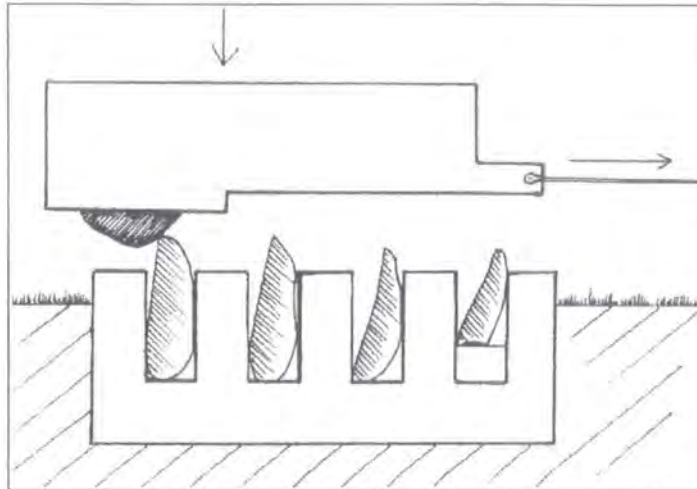


Fig. 3.11: Sled experiment (redrawn from Warren 1914a, 426). Warren dragged a weighted sled, which had pebbles attached to its base, over the projecting edges of flints held in a wooden frame.

Moir attempted to simulate natural conditions in his experiments. He countered Breuil's Belle Assise findings by 'simulating' geological conditions in a letter press filled with sand and flints, proving that a covering of sand protected flints from fracture: pressure did not act vertically through sand (Moir 1912a, 181; Moir 1911, 18-19). (Sand covered Moir's sub-Crag flint bed at Bolton and Laughlin's pit, from which he had retrieved many pre-palaeoliths.) Moir also 'simulated' a sea beach or river agencies in his sack experiment, shaking ten broken flints in a sack to demonstrate that natural forces removed flakes at varying angles whereas human blows were delivered at a constant angle (Moir 1911, 20; Moir 1912a 173-174; Moir 1919, 15-18; 1939, 6-7). Lankester himself pointed out the flaw: 'With regard to your experiment with flints in a sac [sic] – it seems to me that the conditions of a torrent or rush of sea ice-bearing water are different' (Lankester to Moir, 5<sup>th</sup> July 1911: BLL: RL, Add 44968/50). Haward was incredulous, crying 'Nature does not confine stones in a sack', and emphasising that dragging a stone across a concrete floor (see Moir 1912a, 183) was a far cry from the massive pressures of a glacier (Haward 1914, 347-348). Warren was similarly dismissive of Moir's belief that he could represent natural conditions (Warren 1913, 38; Warren 1914a, 547).

But Haward and Warren were not trying to reproduce nature. They aimed to investigate the physical properties of flint and to train the eye to recognise the characteristics of chipping produced by different methods. Haward hinted that lack of 'extensive geological experience' was the source of such misguided attempts to replicate nature: in response to some of Moir's (1912a) efforts, Haward exclaimed 'It is ridiculous to suggest that a few laboratory experiments, made under artificial conditions, are equivalent to the action of Nature, whose forces are so varied' (Haward 1914, 349).

Warren attributed the mistrust (on both sides) of experimental research to a misconception of fundamental aims (Warren 1914b, 413-414; see also Warren 1914a, 547; 1923a, 39; 1923b, 155; 1925, 302). Nature could only be observed in the field, where a convincing variety of flakings could be recovered, as Breuil had demonstrated to devastating effect with his 'weighty article' on Belle Assise in 1910 (Underwood *et al.* 1911, 36). However, Moir had a very different perception of how experiment could contribute to the central problem. He defended his sack and letterpress experiments, and continued to describe them in subsequent publications (Moir 1919, 15-18; 1939, 6-7; see Grayson 1986, 111). It seems that Moir never did fully grasp that Warren and Haward were searching for general principles rather than reconstructions of nature, an outcome that, despite Haward's suggestion, was probably tied as much to his personal dogmatism as lack of geological knowledge.

Moir now set himself the task of comparing his pre-palaeoliths and, more particularly, his experimental results to flints caught frozen in the act of fracture in the field (Moir 1914a, 397-398). Following Breuil's Belle Assise case, Moir examined another Eocene stratum, but one closer to home: in the Bramford pit in Suffolk, owned by one Mr. Coe (Moir 1914a, 399). Moir concluded that the Eocene flints were clearly the work of unguided natural forces, almost identical to his experimentally-produced ones: there were 'pseudo'-bulbs of percussion but these lacked striking platforms; there was no retouch and no definite forms, only 'fantastic shapes' that had clearly been naturally fractured and which could not be divided into archaeological types (Moir 1914a, 400-403). In contrast, when Warren searched for natural-flaking sites his aim was to use these observations in conjunction with general principles learnt from experiment: the two central pillars of his natural-flaking arguments (see

Warren 1923b).

In addition to experiments and field observation of natural fracture, Moir and Haward also came into conflict over the attributes of humanly-struck flints. Both had been drawn to the local gun-flint makers, known as the Brandon Flint Knappers, as an ethnographic analogy for their arguments over fracture attributes – recalling Worthington Smith’s (1887, 87-89) work with the Stoke Newington forgers twenty-five years earlier (see Chapter Two). Moir argued that despite the lack of bulbs of percussion on his sub-Crag specimens, these had still been struck by humans, since a similarly flat, non-conchoidal surface was produced by the particular “quartering” fracture with which the flint knappers of Brandon dressed their flints (Moir 1913a; Moir 1914a, 25; see also Lankester 1914, 7). Returning to the pre-palaeoliths, Moir surmised that ‘The men who made these implements had acquired the art of being able to fracture flints with a blow and leave a flat surface showing practically no bulb of percussion. It was needful to do this when making the rubbers for dressing skins’ (Moir 1913a, 313).

Unfortunately for Moir, Haward in 1897 had been ‘taught the secrets of the art of flint chipping by several of the Brandon “Flint Knappers” with whom I kept in close touch for years’ (Haward 1919, 120), and in combination with his observations of natural-flaking sites and his experiments, Haward made a strong case. He argued that throughout all industries of whatever age, the characteristics of bulbous fracture produced by percussion were the same, since they were produced by the same force – most flakes struck by man *did* have a bulb of percussion, although admittedly an iron hammer produced finer bulbs than a stone one (Haward 1919, 122-124). The difference in the chipping on flints from the Norfolk Basement Beds and the Ipswich Sub-Crag Beds was due to the fact that they had been fractured by great natural pressure that could remove large flakes with an almost flat bulbar swelling – and this was different to human-struck bulbs (Haward 1919, 125-127).<sup>10</sup>

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<sup>10</sup> Barnes criticised Haward’s conclusions about cones of percussion, arguing that these could be very varied in appearance (Barnes 1920, 259). The debate between the two continued through subsequent issues of the *Proceedings of the Prehistoric Society of East Anglia*.

*In summary: tracing the sources of disagreement over flint-fracture research*

Lankester had sent Moir off into the field of flint-fracture research in an attempt to attack the arguments of the opposition on similar grounds. However, for various reasons, Moir failed to hit the right tone of argument to convince the sceptics. Despite his experiments, observations in the field, and anthropological analogies, Moir never seems to have understood that Warren and Haward were trying to understand the general principles of flint-fracture. Haward put Moir's attempts to reproduce geological forces down to lack of 'extensive geological experience' (Haward 1914, 349). Regardless of the source of Moir's misunderstanding, there was evidently an interesting atmosphere of uncertainty over the respective roles of geology and archaeology in flint-fracture research, which led to doubt over who had the authority to pronounce on the question of flint flaking agencies.

Boule noted that although geologists were happy to assert their opinions on the age and context of Moir's pre-palaeolithic finds, they avoided the question of whether these flints were humanly worked, which was proclaimed mostly by archaeologists (Boule 1912b, 428). Warren, who was perceived by most of his contemporaries as a geologist (despite his contributions to Palaeolithic archaeology), also remarked on the peculiar position of research into natural agencies of flint flaking. He observed that although the study itself was a purely geological subject, its application was purely archaeological, so geologists avoided:

'this no-man's-land of unexplored phenomena. Thus the problem of distinguishing human flaking from natural that is at the foundation of prehistory "fell between two stools" and became the Cinderella of Science, casually cloaked in any rags and tatters of misleading half-knowledge' (Warren 1940, 13-14).

Judging from the arguments above, Warren may well have been thinking of Moir when he spoke above of 'misleading half-knowledge'. More importantly, this comment also summons up a perception of division between the role of geologist and archaeologist within a subject that might have been expected to foster close alliances. Boule, in his review of Moir's first (1911) paper on the sub-Crag implements, predicted:

*'chacun l'appréciera de son point de vue personnel, au moyen de son expérience en matière géologique ou archéologique'* (Boule 1912b, 428).  
'everyone one will appreciate it from their personal point of view, through their experience of geological or archaeological matters.'

However, this was not just a geological / archaeological divide. One final point, self-evident but nonetheless important, is that the differences in the perception of the pre-palaeoliths which provided each side with opposing research objectives also led to differences in the way the research was presented. Since Warren and Haward did not believe in the archaeological nature of the pre-palaeoliths, they approached flint-fracture research in an attempt to gain an understanding of general principles that would enable them to observe specific diagnostic features on any chipped flint from known geological context and decide the agency involved. Moir, however, believed in the archaeological nature of the pre-palaeoliths, and would often bring in archaeological lines of argument that were of little relevance to Warren or Haward. For example, Moir noted that naturally-fractured flints, unlike his pre-palaeoliths, did not fall into distinctive types (Moir 1914a, 402), or suggested functional advantages of features taken by the opposition as evidence of natural fracture (Moir 1913a, 313). Such lines of argument were more familiar fare for Moir and Lankester as will be demonstrated in the next section. However, later in the chapter we will return to the way in which the personal point of view blinkered one side from the concerns of the other. The picture of two sides firing past each other in a long-running debate also characterised arguments which will form the subject of future chapters.

### ***3.3.2: Industrial patterning of the pre-palaeoliths: expectations and approaches***

In addition to his attempts at grooming Moir in the art of flint-fracture research, Lankester (Fig. 3.12) also made great efforts to present Moir as a reputable archaeologist. The promotion of the pre-palaeoliths as respectable archaeological specimens was a far more enduring and attractive approach for Moir and Lankester than their defensive foray into flint-fracture research.

'You can always state what are the accepted views & pros & cons put forward by geologists as to relative ages of deposits. But you yourself are & must be more & more, an expert & critic of the worked flints themselves' (Lankester to Moir, 3<sup>rd</sup> March 1912: BLL, RL, Add 44968/123; emphasis in original).

In their archaeological interpretations of the pre-palaeoliths, Moir and Lankester were not just trying to understand how their industry fitted into the industrial succession. Their opinions were also aimed at the opposition, and were intended to promote and defend the existence of the Pre-Palaeolithic as a true archaeological entity. The following discussion of their archaeological reconstructions is intended to complement the section above on flint-fracture research, and give an idea of how believers in the eoliths saw the debate. The kinds of interpretations that they were making also give an insight into the expectations and assumptions of industrial patterning that guided Early Palaeolithic research.

For clarity, the discussion below has been split into three parts. The first part discusses Moir's application of the concept of industrial progression to the pre-palaeoliths. In the second part, Lankester's enthusiasm for the rostro-carinate has been used to illustrate the importance of the type-fossil concept to the arguments over the pre-palaeoliths. These two areas are integrated in the third part, which examines how Moir and Lankester reconstructed the evolution of the rostro-carinate, and how they connected the pre-palaeolithic line to the Kent eoliths on the one hand and the palaeolithic industries of the river-valley drifts on the other.



Fig. 3.12: Edwin Ray Lankester (1847-1929) (Moir 1935, frontispiece).

*Concepts of progression and Moir's evolving pre-palaeolithic industries*

The assumption that the very earliest implements would be crude in character and later industries would exhibit more skilled workmanship formed an important part of the arguments of those who promoted the eoliths. This is evident from the tone of discussion adopted by Prestwich and Harrison, described above (Prestwich 1891, 134; 1892a, 254-255; Harrison to E.B. Tylor, 14<sup>th</sup> November 1907: PRM: EBT, Harrison 11). Turning to the pre-palaeoliths, the attacks of the opposition stemmed from the same assumptions. Haward emphasised that '*Roughness of workmanship is [...] no proof of antiquity*', citing a number of alternative agencies: the intractability of raw material, the difference between the skill of a master-knapper and an apprentice, and the fact that in the early twentieth century, 'primitive' cultures were contemporaneous with 'modern civilization' (Haward 1914, 350; emphasis in original). However, although crudity was not proof in itself of antiquity, the earliest industries were nonetheless expected to be crude. Warren argued:

'If the eoliths were human implements it appears to me that we should expect that their characteristics would be independent of associated geological forces, but would be dependent upon the relative ages of the deposits containing them. As a matter of fact we do not find that earlier deposits consistently contain more primitive eoliths and later deposits more advanced eoliths' (Warren 1914a, 551; see also Warren 1914b, 433).

Considering the power of the concept of progression, it was not surprising that Moir saw a progressively evolving sequence of distinct tool-types in his pre-palaeoliths, their age and form successfully plugging the immense gap between the ancient eoliths of Kent and the more recent river-drift palaeoliths (see Table 3.3). Moir used the existence of such an industrial sequence as a weapon to counter the views of critics like Warren. Indeed, Moir attributed the previous absence of a convincing intermediate form as one of the main reasons for the failure of the Kent Eoliths to attract wider support (Moir 1912a, 172). However, whilst Moir devoted his energy to the creation of such a sequence based upon differences in flaking, form, patination and staining, a sceptical opposition pointed out the disturbed geological context of these finds, and the dubious basis of Moir's criteria of relative age.<sup>11</sup>

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<sup>11</sup> See Haward 1914 for arguments on why patination was no guide to the age of Kent plateau eoliths. In 1948, Warren carried out a detailed statistical survey of abrasion, patination and the relationship

In the first announcement of his discovery, Moir was already claiming to have found at least two distinct types of sub-Crag implement, which he assigned to different ages based on differences in their patination and flaking (Moir 1910, 8; Lankester 1912a, 284). Moir soon added a succession of 'implement' groups from successive geological horizons to his pre-palaeolithic series, once again based on differences in flaking, form, patination and staining. These included a few from the base of the Red Crag; at least four different groups from the Middle Glacial Gravel, thought to have been made by different races over many different periods and later mixed together; and precursors to the earliest Chellean implements from the overlying Chalky Boulder Clay (Moir 1913a, 308-311; 1913b, 369-370). Occasional discoveries of 'Harrisonian' specimens indicated a still earlier stage (Moir 1913b, 374).

Moir was fond of remarking that the variety demonstrated by these various successive 'assemblages' would not be present had they been naturally formed, since Nature would flake in a uniform manner (Moir 1913b, 370). However, Moir was once again failing to engage fully with the flint-fracture arguments of the opposition: Warren had not argued that Nature flaked in a uniform *manner*; he had observed that natural fracture followed uniform *principles*, the character of flaking varying with different kinds of raw material or geological forces (Warren 1914a, 551).

At this stage, Lankester (1912a, 330-331; 1914, 16) remained sceptical of such a sequence, although he seems later to have shifted his opinion (see Moir 1919, 41-42). He criticised one of Moir's drafts (probably Moir 1911) for suggesting a link to the much-debated Kent eoliths with the remark that 'A rough implement is often later than a more elaborate one' (Lankester to Moir, 29<sup>th</sup> December 1910: BLL: RL, Add 44968/15-16). At this stage, Lankester preferred to concentrate on establishing the human workmanship of the pre-palaeoliths:

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between abrasion and the number of flake scars in *The Basement Bed of the Red Crag at Dovercourt & its Flaked Flints*. This was an enormous undertaking. 'At one spot I was able to dig out & bring home every stone over an area of ten square feet', a total of 150 flints (Warren, undated draft, c.1948: BM(NH): DF140/7). Warren may have been inspired to make this effort in response to T.T. Paterson's revival of the question of sub-Crag man in *Nature* (Paterson 1948, 278).

'& not to trouble too much about further questions, such as the various "groups" in Mid-glacial, Boulder Clay &c although they are really of very great importance & interest. Once we have fixed the bare fact that such early worked flints really exist! That is what people still deny!!' (Lankester to Moir, 21<sup>st</sup> January 1914: BLL: RL, Add 44969/106-107; emphasis in original)

Lankester had no problem with Chellean or even Neolithic specimens of rostro-carinates, announcing:

'I am convinced that it is wrong to assume that each succeeding period had its own industry and only that industry. Why should not some men have retained the tradition of an earlier implement? It must be a question of evidence, and so far as I can make out, collectors have done great harm, in not keeping every variety of worked flint, poor ones as well as fine ones, from any given stratum examined. They have greedily seized on fine well-chipped specimens and neglected, actually thrown away, wonderful simpler tools, in the most unscientific way. I have seen in small collections here lately several undeniable rostro-carinates' (Lankester to Moir, 15<sup>th</sup> March 1913: BLL: RL, Add 44969/57-58; emphasis in original).

However, Lankester himself was guilty of selectivity of distinctive forms from amongst the ruder array of potential pre-palaeoliths. We shall see below how Lankester defended a progressive evolutionary sequence of his beloved rostro-carinate forms (Moir 1919, 41-42), which was perhaps stimulated by his association with Moir. To conclude this section, Moir's conviction that he saw evidence of progression in stones now generally recognised as natural products, and the widespread respectability of such an assumption – even if founded on fallacious material – illustrates how persuasive was the concept of progressive industrial evolution in directing Palaeolithic interpretations. If Moir and his allies were persuaded of this, what impact might such expectations have on the interpretation of true palaeolithic industries?

#### *Lankester and the rostro-carinate*

In 1913, at a meeting dedicated to the eoliths and early palaeoliths, the President of the Geological Society asked four questions of those promoting such 'implements'. The first three followed the now familiar lines of enquiry: 1) were they geologically *in situ*? 2) what was the geological age of the deposits? 3) were they humanly worked? However the fourth question was 'Could such a sequence of types of implements be

established in this country as to enable geologists to use implements as zone-fossils in the deposits of the Human period?’ (Anon 1914, ii)

The concept of the zone-fossil was very attractive for geologists, and the identification of distinct tool-types was standard practice in palaeolithic research. Lankester avidly took up this concept, and promoted a distinctive, repeated chipped stone form found amongst the pre-palaeoliths: the ‘rostro-carinate’, or eagle’s beak implement (see Fig. 3.13 below), which he exalted above associated ‘scrapers’ and ‘hammer-stones’ (Lankester 1912a, 287).

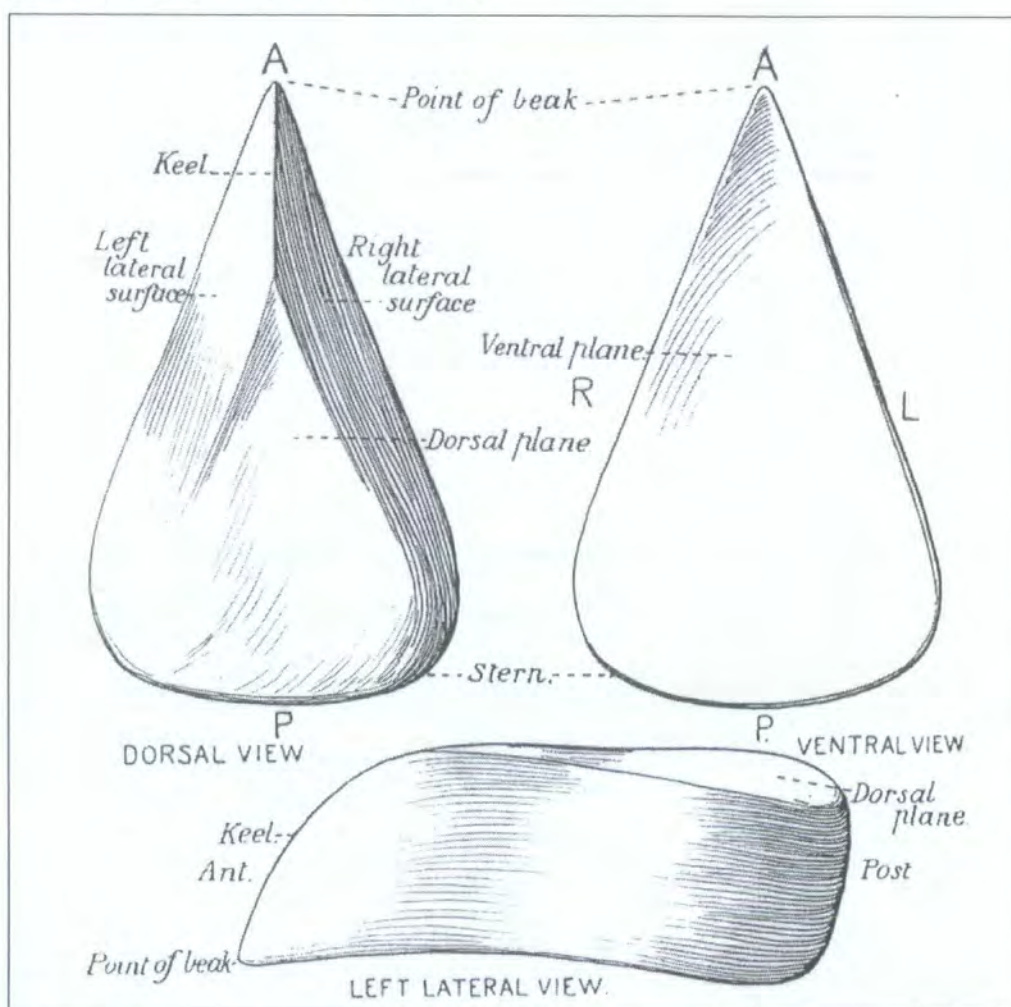


Fig. 3.13: ‘Diagrams showing the ideal form aimed at by the makers of the Rostro-carinate flint implements’; ‘A, anterior; P, posterior; R, right; L, left’ (Lankester 1912a, 294, Fig. 1).

The anthropologist, Dr. R. Marett, recorded Sir Ray Lankester’s search for earlier forebears of the palaeolithic tools in a short poem, and his enthusiasm for the rostro-carinate comes across well in the following two verses:

‘Then Mr. M . . . r took on the job for all that he was worth,  
And soon he lighted on a flint in twenty feet of earth;  
A very King of Flints it was, of Brobdingnagian bulk,  
Looking something like the fore-part of a badly battered hulk.

They lifted it in triumph, and they bore it to the Knight,  
He gazed, and suddenly his eyes glowered with second-sight.  
Then hoarse he cried, as one through whom the Voice of Time did speak:  
“I greet thee, Rostro-carinate! For I recognise thy Beak!”  
(From Marett’s *Oratio Capitalis pro Rostris*, Moir 1935, 34)

It was perhaps his biological background that gave Lankester such enthusiasm for the rostro-carinate as a distinctive type, and coloured his attempts to slip this type, like some slightly dubious new animal species, into the existing classification. However, in view of the historical debt owed by Palaeolithic classifications to palaeontological methodology, Lankester’s efforts were not particularly out of place. Indeed, this focus on a distinctive Eolithic core-form is reminiscent of the way others approached Palaeolithic hand-axes, as discussed in Chapter Two.

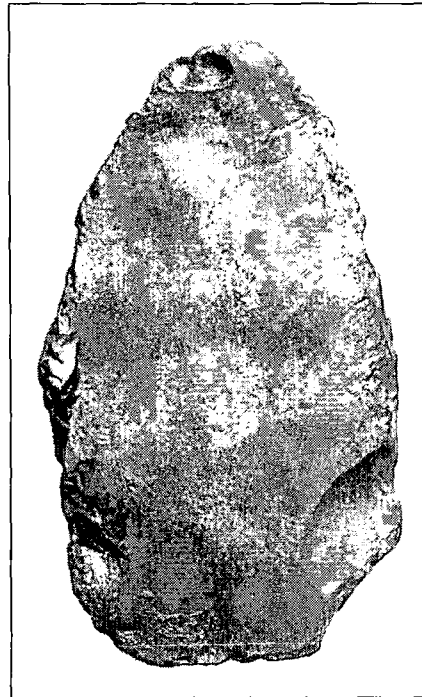
Lankester’s masterpiece was a *Description of the Test Specimen of the Rostro-Carinate Industry found beneath the Norwich Crag*, published as a Royal Society Occasional Paper in 1914. This eighteen-page treatise on the splendid rostro-carinate included fourteen high-quality figures and three plates, all devoted to a single type specimen, ‘the best preserved specimen of this class of implements’, found by W.G. Clarke, a reporter from Norwich, at Whitlingham pit and known henceforth as ‘the Norwich test specimen’ (Lankester 1914, 1). This most remarkable and historic focal point of past debate, the type-fossil *par excellence* illustrated in Figure 3.14 below, has recently been rediscovered by Roger Jacobi (pers. comm. 2003) in the British Museum collections, where it can be viewed today (BM(F) Norfolk, Whitlingham).

Lankester brandished the rostro-carinate at the opposition in defiance at their suggestions that the pre-palaeoliths had been produced by natural agencies of flint-fracture. His attempts at casting Moir into the flint-fracture debates may have been unsuccessful, but surely the opposition would see convincing proof of human workmanship in the rostro-carinate? Lankester declared to Moir:

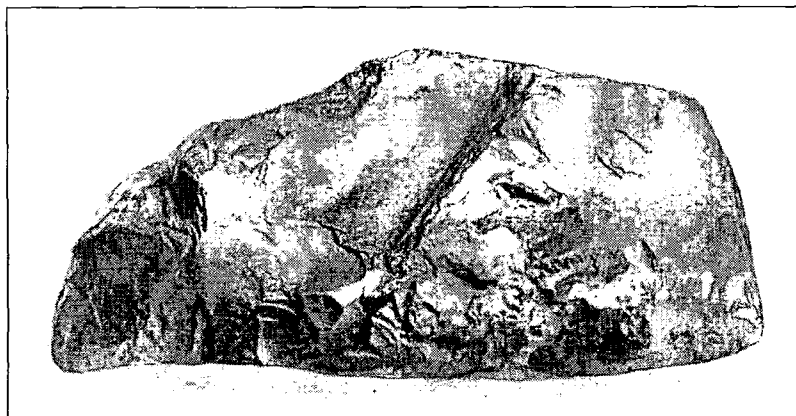
'The adherence to one type – the rostro-carinate – is a great argument for the unbeliever. I want to ram and stuff him with that one type. Once he has admitted that as human, the rest can be discussed with greater advantage' (Lankester to Moir, 18<sup>th</sup> March 1913: BLL: RL, Add 44969/59-60).



(Lankester 1914 Fig. 1)



(Lankester 1914, Fig. 3)



(Lankester 1914, Fig. 4)

Fig. 3.14: The Norwich Test Specimen: 'A Rostro-carinate from beneath the Norwich Crag' Fig. 1: dorsal view, showing the keel; Fig. 3: ventral view (Lankester 1914, Plate I); Fig. 4: left side of specimen in profile (Lankester 1914, Plate II).

The question of the human workmanship of rostro-carinates appears to have been the first task Lankester set himself before turning to a consideration of its geological age or context – a reversal of Prestwich’s strategy in his three papers discussed above. As Lankester explained to Moir:

‘In that paper [Lankester 1914] I insisted, as you & every one else can read, that the main point for me is the human workmanship of this & the other rostro-carinate implements. The occurrence of this & of others in a given geological stratum, and further the exact age & relations of such stratum are separate questions, and must be discussed by geologists on definite geological evidence. That is what I wrote & still hold’ (Lankester to Moir, 3<sup>rd</sup> September 1917: BLL: RL, Add 44970/15-16; emphasis in original).

There were several good reasons to justify such an approach. The geological context of the East Anglian discoveries was less complex than the Kent Plateau problems that had faced Prestwich, and Lankester’s discoveries were not dominated by surface finds. In addition, Lankester did not inspire the respect enjoyed by Prestwich within geological circles, being more of a biologist than a geologist. Even his early work on the East Anglian Crags was more palaeontological than geological in content (Lankester 1865; 1870).

Turning now to the reception of the rostro-carinate, few dismissed it lightly; indeed, this type-fossil merited a number of serious, considered attacks by the opposition. Haward, for example, presented a detailed interpretation of exactly how the Norwich test specimen had been naturally flaked along lines of least resistance (Haward 1919, 133-135). Lankester’s emphasis on the rostro-carinate also stimulated a number of conversions, one major name being Victor Commont, who died just when Lankester ‘was looking forward to his “coming out” on our side about rostro-carinates’ (Lankester to Moir, 21<sup>st</sup> May 1918: BLL: RL, Add 44970/44-45). Commont, as we shall see in Chapter Four, had gained an influential standing amongst British palaeolithic archaeologists, largely through his researches in the Somme Valley.

Even Lankester’s previous adversary, William Sollas, was converted. Sollas had often collided with Lankester in the earlier years of the pre-palaeoliths. At one meeting, he had dismissed the finds, saying “We have here choppers which do not chop – borers which do not bore”, to which Lankester had retorted, “You, Sir, are not a borer which

does not bore” (Moir 1935, 32). But though Sollas had also attacked the rostro-carinates at the 1913 meeting of the British Association at Birmingham (Sollas 1913, 788-790), he admitted in the second edition of his *Ancient Hunters* that rostro-carinates were humanly manufactured (Sollas 1915 119). This led Lankester to muse, rather ungratefully, that ‘Sollas is a hopeless creature. I suppose he has heard from Commont, as I have, that he accepts the Norwich type specimen’ (Lankester to Moir, 30<sup>th</sup> March 1916: BLL: RL, Add 44969/174-175).

For Warren, who was one of the reviewers of Sollas’ second edition, this change of mind left him puzzled at ‘an apparent inconsistency with our author’s attitude to the “rostro-carinates” in general’ (Warren 1916a, 79); however, Lankester was exultant, and marked out to Moir exactly how best to press forward the advantage.

‘The successful method to follow now, is to insist on this. “There are rostro-carinate implements.” The further point now to be decided & agreed upon, is “What is the age of this specimen, and of that?”’ (Lankester to Moir, 18<sup>th</sup> April 1915: BLL: RL, Add 44969/156-161; emphasis in original)

Flushed with such success on the human workmanship question, thanks more to the rostro-carinate and the persuasive type-fossil concept than to Moir’s flint-fracture experiments, Lankester seems to have become more amenable to Moir’s efforts to develop a succession of Eolithic and Pre-Palaeolithic specimens running through time until they linked into the Chellean, at the base of the accepted Palaeolithic industrial succession.

#### *The evolution of the rostro-carinate, and links to the Palaeolithic sequence*

Whilst Lankester promoted the *human workmanship* of the rostro-carinates, Moir emphasised the *progression* of the rostro-carinate tool type, which gave him another line of attack against those who argued that his ‘artefacts’ had been produced by the action of unguided natural forces. Moir also elaborated on the rostro-carinate ancestry of undeniable Chellean artefacts, perhaps partly in the hope that these reputable descendants might reflect some of their glory back onto their less respectable ancestors. He argued that the rostro-carinates from the Bone-Bed below the Red Crag evolved into more finely-made specimens in the Middle Glacial Gravel, and were becoming scarce and degenerate by the time of the Chalky Boulder Clay, when typical implements now resembled early

Chellean specimens (Moir 1913b, 373; see also Moir 1914a, 402-403). Moir then devoted two papers to this question of the evolution of the early palaeolithic hand-axes from the rostro-carinate. The first (Moir 1916) was delivered to the Anthropological Society in a paper published in 1916, and the second was delivered to the Royal Society in 1917 (Moir 1920).

Lankester initially countered Moir's arguments that the rostro-carinates evolved into the Chellean forms, arguing that although the rostro-carinates might be descended from the Kent Plateau specimens, they eventually disappeared, and 'there is no evidence of any transitional connection' of the East Anglian 'Icenian'<sup>12</sup> industry with the river-drift implements (Lankester 1912a, 330).

'The Chellian and Acheuilian [sic] and Moustierian [sic] types are essentially depressed or flattened like a leaf. The Sub-Crag type (rostro-carinate) is essentially compressed from side to side' (Lankester 1912a, 331; emphasis in original).

However, by the time Moir was about to submit his Royal Society paper (Moir 1920) late in 1917, Lankester seems to have reconsidered his previous view that there was no link between the compressed rostro-carinate and the flattened hand-axe, and ingeniously solved the question of how to link the one to the other. Lankester considered that the development from the rostro-carinate to the palaeolith of rhomboidal section echoed the evolution of the plaice, and suggested they be called 'Platessiform': plaice-like (Moir 1920, 346). Before this Royal Society paper went to press, it was pointed out that other palaeoliths were *triangular* in section, and so Moir had to add a parallel line of evolution: Lankester's 'Batiform' palaeoliths that echoed the evolution of the skate, *Raia batis* (Moir 1920, 347-348; see Fig. 3.15 below).<sup>13</sup>

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<sup>12</sup> 'Icenian' was the Roman name for East Anglia, and thus became Lankester's term for the sub-Crag industry (Moir 1935, 99).

<sup>13</sup> 'But in the giving of these descriptive names there is, of course, no wish to convey the impression that the makers of the ancient flint implements described, had a knowledge of the manner in which the evolution of the plaice and the skate took place, and fashioned their artefacts on a similar plan. It would appear to be a mere chance that the Platessiform and Batiform implements were developed along analogous lines to the plaice and the skate' (Moir 1919, 42).



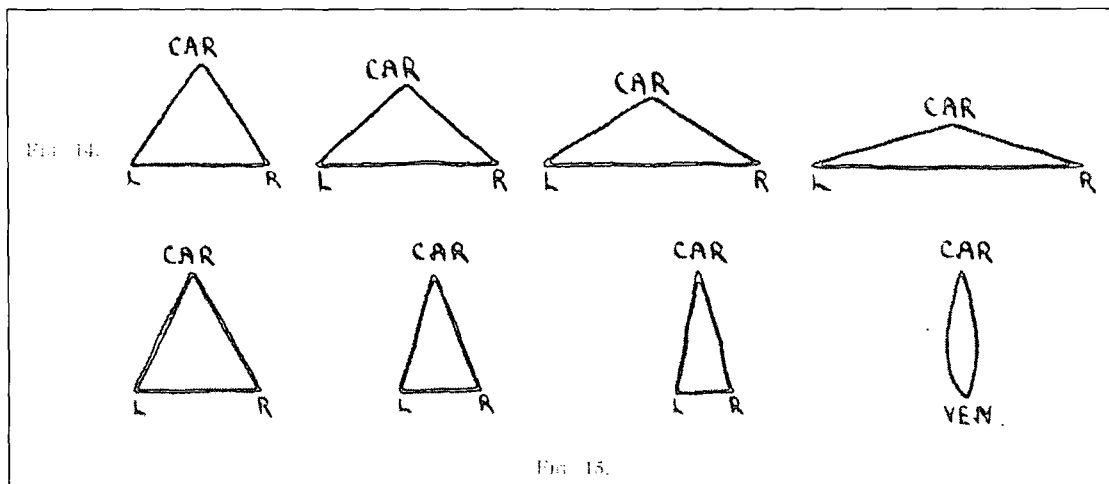


Fig. 3.15: Moir's illustration of the Batiform and Platessiform lines of development (Moir 1919, 41-42, Plate 15; see also Moir 1920, 346, Figs. 13 and 14). His Fig. 14 illustrates the depression of the keel in a 'Batiform' manner; and Fig. 15, the Platessiform progression. 'Car' indicates the position of the carina (keel), and 'ven' the ventral side of the flint.

In the line leading from rostro-carinates to palaeoliths of *rhomboidal* section (Platessiform), the cutting edge was extended from the keel on the dorsal surface to the *ventral* surface on the opposite side, the flat underside being flaked away (Moir 1919, 36-37). In the other line of evolution from the rostro-carinate to palaeoliths of *triangular* section (Batiform), the hand-axe kept to the same plane as the original rostro-carinate implement. Instead of the keel becoming prominent and extending first along the dorsal and then around the ventral surface, the dorsal keel in this case became flattened or 'depressed' and the two side edges took on the cutting role (Moir 1919, 39).

Lankester was adamant that this research into the evolution of the rostro-carinate should be presented in a strategic manner, and although he now publicly supported a progression towards the Palaeolithic hand-axes, he warned Moir to avoid mentioning the earlier, Eolithic end of this evolutionary development. Before the discovery of the pre-palaeoliths, Lankester had supported the Kent eoliths, addressing Harrison as the 'courageous and indomitable discoverer of pre-palaeolithic man' (Lankester to Harrison 15<sup>th</sup> April 1906, in Harrison 1928, 271). However, there was now a danger that the Kent eoliths might tar the pre-palaeoliths with disreputable associations that were best avoided.

‘As to your paper [Moir 1920] on the transition flints from Rostro-carinate to Chellean-type Palaeoliths, I am convinced that it is most important not to go into the separate (though related) question of the development of rostro-carinates from simplest beginnings (which I beg you not to call “Eoliths” – endless misery & confusion & lies are set rolling by that word “Eolith”). It is really necessary in a controversy like this to hammer in one point at a time. What you can (& I hope will) do now, is to show that there are most convincing intermediate forms between typical rostro-carinate & typical Chellean (or Acheullian) [sic], and that there is no explanation of their occurrence (in such number as you can produce) excepting that they were actually flaked by the prehistoric men with the intention of modelling an implement or more or less Chellean shape from a rostro-carinate by treating the carina as one of the lateral margins of a Chellean and flaking away the ventral plane to form the other lateral margin. That & all that you have to say about the necessity for a striking platform, (my dorsal plane or platform), and its retention in many completely symmetrical Chellean implements, is enough for one paper’ (Lankester to Moir, 3<sup>rd</sup> September 1917: BLL: RL, Add 44970/15-16; emphasis in original).

However, having clarified the relationship between the rostro-carinate and the Palaeolithic hand-axes (Moir 1916; 1920), Moir felt free to turn his attention to still more ancient ancestors of Palaeolithic industries. In his 1919 book, *Pre-Palaeolithic Man*, Moir attempted to trace the origin of his two Palaeolithic hand-axe lines on the one hand, and the flake-dominated Mousterian industry on the other. Both lines began with eolithic types from Kent that had been defined by Harrison and Prestwich. One line led from the simple Kentian scraper and point to Mousterian-like implements from the Middle Glacial Gravel, then to implements of more markedly Mousterian form from the Chalky Boulder clay above, before culminating in the Mousterian and Upper Palaeolithic industries. The other line led from the Kentian points ‘prophetic of succeeding rostro-carinates’ to the true rostro-carinates of the Pre-Red Crag industries, and on to both Platessiform and Batiform hand-axe dominated Palaeolithic industries: Pre-Chellean, Chellean and Acheulian (Moir 1919, 48, Fig. 7).

Moir later added his ‘Early Chellian-palaeolithic age’ Cromer finds as another link in the pre-palaeolithic succession, noting the superposition above the pre-Chellean Crag (Moir 1921a, 429-430). These were discovered on the foreshore at Cromer in 1918 from what was, in Moir’s opinion, a remnant of the lowest Forest-bed horizon (Moir 1921b, 385) where the familiar rostro-carinates appeared to be ‘in process of manufacture into the platessiform, and batiform, Chellian implements’ (Moir 1921b, 395).

*In summary: visions of archaeology and arguments over natural fracture*

Certain important expectations, introduced in Chapter Two, seemed to be driving the classification of these pre-palaeolithic industries. Moir's strenuous efforts to describe an evolving series of industries within the Pre-Palaeolithic phase of industrial development illustrate the powerful concept of progression in tool-working skill through time. The enormous amount of attention lavished by Lankester on a core-tool type which he named the 'rostro-carinate' (Lankester 1912a, 287), recalls the discussion in Chapter Two concerning the dominance of typologically-attractive artefacts like the hand-axe over less distinctive flakes. Moir also seems to have been drawing on the widespread conceptual division between flake-dominated industries and those characterised by core-tools. In addition, his attempts to trace these two lines back to a common origin in the Kent eoliths seem to have been building on contemporary ideas of parallel industrial cultures (see Chapter Four). Later chapters will explore the influence of such expectations in relation to interpretations of true industries of Early Palaeolithic age. However, their weighty influence is evident when one remembers that Moir and Lankester managed to identify progressively-improving lines of flake-dominated and core-dominated industries in a selection made from what are now widely regarded as naturally-fractured stones.

We have observed some of Moir's problems in engaging with flint-fracture arguments on the same level as Warren and Haward in the previous section. From the discussion above, it is now apparent that he was far more at ease when constructing an archaeological promotion and defence of his pre-palaeoliths. The research objectives of both sides were stimulated by a desire to understand the phenomena under scrutiny, and were also employed in defending expectations and attacking the opposition. Unfortunately, the differences in initial expectations and approaches confused communication, despite the fact that both sides were arguing about the same chipped stones. The pre-palaeolithic stones fulfilled Moir's archaeological expectations, but when opponents such as Warren looked at his finds, they saw a completely different set of attributes. It is difficult to see how Moir would ever achieve any important conversions amongst such sceptical researchers of flint-fracture agencies.

### *3.3.3: Social manipulation behind scientific debate: presenting the Sub-Crag flints to the palaeolithic research community*

Notwithstanding such differences in approach and outlook between the two sides (and often within their ranks), the choice of how to publicise discoveries and interpretations could be as important as the content of research in gaining greater academic approval for one's viewpoint. Having your idea heard by the right people in a socially advantageous context was a strategic way to win more support. Lankester's letters to Moir give a vivid insight into the strategic use of important arenas, particularly the local and national societies, their journals and meeting rooms.

From the tone of the correspondence above, it is evident that although the relationship between Moir and Lankester seems, superficially, to mirror that of Harrison and Prestwich – the local researcher and the esteemed academic figure. In fact, Lankester directed Moir's presentation of the case very closely. Moir could be confrontational and suspicious<sup>14</sup> of the opposition, hot-headed and, without Lankester's restraining hand, likely to rush heated words into print. This impulsiveness has left some valuable records that have been of great assistance to the following examination of the social strategies involved in debate, as the Machiavellian Lankester often found it necessary to explain strategies carefully in letters to Moir. These give a fascinating insight into the tactics of the time, many of which characterised later discussions on other subjects. A flavour of the varied arenas and tactics has been provided below: the use of local and national societies by Lankester and Moir, and by some of their opponents; the use of tone and terminology in printed articles; and the exhibition of specimens in museums and society rooms.

#### *Getting ahead in Society: society meetings and journals*

Moir published his first detailed paper on the pre-palaeoliths in the *Proceedings* of a newly established local society, the Prehistoric Society of East Anglia (hereafter, the PSEA), founded in 1908. The fact that Rutot was the first Honorary Corresponding Member gives some idea of the stance that would be taken by the Society towards the eoliths. After Moir had read his first paper on the sub-Crag flints to the PSEA (Moir 1911), a Special Committee was appointed to decide on the human workmanship of these flints. The members of the Committee – Col. Underwood, W. Allen Sturge, W.G. Clarke,

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<sup>14</sup> Described by Baden-Powell as 'paranoiac' (Baden-Powell to Oakley, 19<sup>th</sup> March 1944: BLO: CH, 95).

Nina Layard, and Dr. Corner – examined the flints and accepted a large number, mainly on the basis of their elaborate chipping and the presence of bulbs of percussion (Underwood *et al.* 1911, 37), and the reputation of the pre-palaeoliths (and their discoverer) within the Society was thus decided.

Moir soon gained an influential position within the PSEA, assisted by his close association with the new local pre-palaeolithic ‘industry’. He was Vice-President by 1912, and in his Presidency over the next two years was accompanied by an equally supportive Vice-President: Sir E. Ray Lankester.<sup>15</sup> Moir and Lankester seem to have negotiated their positions between them, meanwhile securing a post for Clarke, a fellow pre-palaeolithic supporter (and discoverer of the Norwich Test Specimen), at the Ipswich Museum.<sup>16</sup> Such prominence within the PSEA enabled Moir to promote the pre-palaeoliths in the journal, and for a while, the reputations of Moir, the pre-palaeoliths, and the PSEA spiralled upwards together. The majority of his papers on the subject appeared in the *Proceedings* (Moir 1911; 1912a; 1913a; 1913b; 1914a; 1914b; 1915a; 1915b; 1918a; 1918b; and more). On the local level, the pre-palaeoliths were doing rather well. Lankester had also started to introduce them into the respected national societies, forums that had been established for longer than the PSEA, and might gild the East Anglian finds with some of their prominence and prestige.

The PSEA was praised by Marcellin Boule, French Professor of Palaeontology and observer of unguided flint-fracture at Mantes, for its strong human palaeontological (i.e. Palaeolithic) content; the other society included in this tribute was the Anthropological Institute (Boule 1915, 2-3), which had been established for far longer and had a stronger national reputation. Prestwich, as noted above, had delivered his third Kent paper to the Institute, and in the early twentieth century supporters and sceptics alike still tried to publish their landmark papers on the archaeological aspect of similarly ancient ‘industries’ under the auspices of the Anthropological Institute (Lankester 1914; Moir 1916; 1921b; Warren 1905; 1914b). The journal *Man*, published under the direction

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<sup>15</sup> ‘I shall be very glad to become Vice President of the Prehistoric Society of East Anglia, and if I feel able to do so, to become President’ (Lankester to Moir, 22<sup>nd</sup> September 1913: BLL: RL, Add 44969/75-76).

<sup>16</sup> ‘I certainly would wish you to continue as President of the Prehistoric Soc. I could not attend to it at all at present – if ever. I think Clarke would be an excellent appoint. at the Ipswich Museum’ (Lankester to Moir, 11<sup>th</sup> May 1914: BLL: RL, Add 44969/121; emphasis in original).

of the Anthropological Institute, also received a number of important papers on the subject (Moir 1916; 1921b; 1922b; Warren 1913; 1922b; 1923c). It is of no surprise that Moir, who viewed himself as a prehistoric archaeologist, directed many of his non-PSEA articles to these two journals associated with the Institute. The two societies had similar interests, and even hosted a joint meeting on flint chipping on 17<sup>th</sup> February 1914, at which Warren also delivered a paper (Warren 1914b; Scrapbook of 'Papers and Notices relating to the Collections of S. H. Warren': BM(NH)).

However, with Lankester on the case, it was not long before the pre-palaeoliths appeared in an arena that was more prestigious than the local PSEA, and perhaps even the nationally renowned Anthropological Institute. Boule noted:

*'La question n'était pas encore sortie du cercle fort restreint des spécialistes quand Sir Ray Lankester la prit sous son patronage et l'introduisit dans un milieu scientifique beaucoup plus étendue et plus officiel'* (Boule 1915, 6).

'The question had not yet left the very restricted circle of specialists when Sir Ray Lankester took it under his patronage and introduced it into a much more extended and official scientific circle.'

Lankester's first paper (Lankester 1912a) was delivered to the Royal Society in the same year where it had a tremendous impact. Boule remarked: *'Naturellement, ce mémoire, magistralement composé et rédigé, fit une grande impression en Angleterre'* (Boule 1915, 8). 'Naturally, this memoir, masterfully composed and written, made a great impression in England.'

The Royal Society, founded in 1660, stood foremost in age and respect amongst all the learned societies of Britain, and Lankester used his standing in both the Royal Society and the PSEA to further the pre-palaeolithic cause. He managed to gain funding for Moir from the Committee of the Royal Society so that he could employ a collector and workman in 1912 and 1913, work that was published in the *Proceedings* of the PSEA (henceforth PPSEA) the following year (Moir 1914a). Lankester then used his PSEA standing to set up 'The Sub Crag Exploration Fund' to continue this funding for 1914, drawing up a draft proposal to be sent to men of influence – all of whom were members of the Royal Society (Lankester to Moir 6<sup>th</sup> Nov 1913: BLL: RL, Add 44969/95-97).<sup>17</sup>

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<sup>17</sup> The signatories to Lankester's 'Sub Crag Exploration Fund' document were Archibald Geikie, President

Lankester observed in one paper that a variety of researchers had accepted the human manufacture of the rostro-carinate, but stated, 'I do not intend to "count heads" nor to cite a list of the names of those well-known experts in the study of ancient flint implements'. He continued regardless with a list bloated with F.R.S; 'Such a list of names would include those of Sir Arthur Evans, F.R.S., Sir Hercules Read, F.R.S., Dr Flinders Petrie, F.R.S., and the late Lord Avebury' (Lankester 1914, 4). John Lubbock (Lord Avebury) had given the Plateau eoliths and Moir's pre-palaeoliths his seal of approval in the seventh edition of his *Prehistoric Times* (Lubbock 1913, 422-423), a most valuable convert amongst these other respected names.

Lankester's achievement in linking the pre-palaeoliths to the Royal Society name was very worrying for his opponents. Henry Howorth (1842-1923), ex-Member of Parliament for South Salford and a dedicated researcher in Palaeolithic matters following his retirement, was appalled that such material had successfully infiltrated the Royal Society. He wrote to Dawkins:

'It is scandalous that the first scientific society in Europe should publish such rubbish as Moir writes and that the Society should give it its imprimatur. The whole difficulty is the way in which the geologists are represented on the Council. The papers are sent I am told to the geological committee of the Society & rather than have a row with Lankester they pass anything' (Howorth to Boyd Dawkins: BMD: WBD, DERSB 70493/1; emphasis in original).

Dawkins begged Sollas to attack the rostro-carinates 'in the names of archaeology and geology' when they came up at the next Royal Society meeting, complaining that Moir and Lankester were 'lowering the character of the Phil. Trans' and 'misleading' those who assumed the Society gave the contents its stamp of approval (Dawkins to Sollas, 24<sup>th</sup> June, undated: UMO: Parker papers, Box 1). The stamp of approval was clearly a powerful one to raise this much antagonism, and Lankester used it to the full. In his first Royal Society paper, he made it quite clear that the geological support for the association of Moir's artefacts with the Red Crag rested upon a sound basis: the two geologists, Whitaker and Marr, had not only held the position of President of the

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of the Royal Society; Arthur Evans; Hercules Read, Keeper of Ethnology at the British Museum; Marr, of the Geological Department, Cambridge; and Henry Balfour of the Pitt-Rivers Museum (Lankester to Moir 6<sup>th</sup> Nov 1913: BLL: RL, Add 44969/95-97).

Geological Society in the past, but were also fellow Fellows of the Royal Society (Lankester 1912a, 285). Lankester's achievement in bringing this subject before more exalted societies greatly enhanced the standing of the pre-palaeoliths.

Although different topics of research were welcomed by different societies, palaeolithic research drew on a variety of research areas. Therefore the pre-palaeoliths (and palaeolithic subjects in general) formed a topic of discussion in a great mixture of societies. It is interesting to see where researchers chose to deliver their papers. Although both sides of the debate could boast both geological and archaeological supporters, Lankester and Moir tended to focus on the archaeological side of the case to promote their interpretation, as we have seen above. The PSEA Committee which accepted Moir's finds noted that their conclusion was based on the flints, although the geological support of Whitaker and Marr 'completes the proof' (Underwood *et al.* 1911, 39). F.N. Haward also managed to direct his flint-fracture counter-arguments to the PPSEA (Haward 1912; 1914; 1919; 1921), and Warren delivered his conclusions to archaeological arenas such as the Royal Anthropological Institute, as discussed above.

However, the results of Warren's flint-fracture research were also frequently delivered at the Geologists' Association (Anon 1913, 302; Warren 1923a; 1923b) and the Geological Society of London (Anon 1914, iii-iv; Warren 1920). Howorth, in an undated letter, contrasted the atmosphere in the Royal Society with that in the Geological Society, where Warren's papers seemed to be received with much support: 'I was in good spirits and let fly vigorously & was much applauded. Reginald Smith was the only one on the other side and he made a hash of his case' (Howorth to Boyd Dawkins, ?1920: BMD: WBD, DERSB 70493/1).

The decision to present a case to a particular society clearly involved much consideration. It was useful to have some standing in the society to be sure of a sympathetic reception, and the prestige of the society would be reflected back on the papers it accepted. In addition, the subject matter had to be regarded as relevant. As discussed in the section on flint-fracture above, those who were attempting to undermine the archaeological claims of Moir and Lankester did not believe in the archaeological reality of the finds. It is not surprising that they seem to have frequented more geological arenas with their unpicking of the archaeological argument based on principles of flint-

fracture, whilst Moir and Lankester wanted to share their reconstructions of a new industry with archaeological audiences. Personal factors also affected such choices considerably. The geologist, Percy Boswell, later recalled:

‘I remember that I repeatedly tried to persuade Moir, that in accordance with the established practice of science, he should set out the evidence against, as well as for, his claims. As a business man, he did not see the necessity for doing this – contending that it was for other people, who disagreed with him, to record unfavourable evidence’ (Boswell, autobiography: ULLiv: PGB, D4/1, p. 40).

*Strategies of terminology: the word ‘eolith’*

Just as Lankester and Moir were careful about where they presented their pre-palaeoliths, they were equally cautious of the words they used in describing them. Lankester was particularly concerned to maintain a distance between the East Anglian specimens and the earlier finds from Kent and Belgium, and he focused instead on the connection to the more acceptable Palaeolithic industries. This is evident from his use of terminology, particularly his reactions to the term ‘eolith’ (see Moir 1935, 93-95). The term ‘Eolithic’ seems first to have been used in *Musée Préhistorique* in 1881, a volume co-authored by the father and son team of Gabriel and Adrien de Mortillet (Mortillet 1881; noted by Grayson 1986, 84).<sup>18</sup> This term was popularised in Britain after J. Allen Brown used it to demarcate Harrison’s rude Plateau specimens from the Palaeolithic implements of the river-drift.

‘Eolithic; Roughly hewn pebbles and nodules and naturally broken stones, showing work with thick ochreous patina, found on the plateaux of the chalk and other districts in beds unconnected with the present valley drainage (Allen Brown 1893, 94).

Lankester had originally supported Harrison’s Kent Eoliths, but when Moir sent a box of his early East Anglian discoveries to Lankester, Lankester was swift to warn Moir of the dangers of the term ‘eolith’, writing ‘I think the word Eolith & even Palaeolith should be avoided. “Worked flints of the so-&-so beds or horizon” is the best sort of term’ (Lankester to Moir, 29<sup>th</sup> December 1910: BLL: RL, Add 44968/16). Moir (1911, 22) was soon defining his implements as pre-palaeoliths: ‘a stage of culture mid-way

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<sup>18</sup> There is a widespread misconception that J. Allen Brown coined the term (see MacCurdy 1907, 546; Osborn 1915, 265).

between the Eoliths proper and the river-drift palaeoliths', as they combined the characteristic surface flaking of palaeoliths with eolithic edge-chipping. Lankester's desire to avoid any negative association attached to the Kent finds explains his concern over the word 'eolith' (Lankester 1912a, 311; 1912c, 250), and his initial opposition to Moir's arguments that these crude Kent forms were ancestral to the 'pre-palaeoliths' – as discussed above (Lankester to Moir, 8<sup>th</sup> June 1910; 29<sup>th</sup> December 1910: BLL: RL, Add 44968/12-13, 15-16).

Around the time of Moir's discoveries, Sollas attacked the eoliths (in the widest sense of the word) in his *Ancient Hunters* and described how the use of this term 'has been extended to similar objects of any age earlier than the Palaeolithic' (Sollas 1911, 55, footnote 1). Lankester took great exception to the book, and more specifically to Sollas's wide-ranging use of the term 'eolith'. Lankester (1912d) castigated Sollas in the *Saturday Review* for 16<sup>th</sup> March 1912 both for applying the term to Breuil's Belle-Assise flakings and for ignoring the evidence of the rostro-carinates, declaring 'It is not the part of a man of science to sweep away all such evidence with the exclamation "Eoliths!"' (Lankester 1912d, 333-334) He had a similar grievance against Charles Dawson (of Piltdown fame) who, at a meeting of the Royal College of Surgeons, 'used the silly word "Eolith" which is always done for the purpose of making confusion and false suggestion' (Lankester to Moir, 27<sup>th</sup> February 1911: BLL: RL, Add 44968/25-26).

Lankester applied the term 'Eolith' solely to the Kentish specimens, and also deplored its desultory use 'by various continental people' – a dig at Rutot. He instructed Moir 'Keep the question of Eoliths on such flints apart. You have got some worked flints which are certainly Proto-Chellean' (Lankester to Moir, 22<sup>nd</sup> April 1911: BLL: RL, Add 44968/31-32; emphasis in original).

'That word "Eolith" must be abandoned. Whenever one says a thing is an eolith, people think one is a fool, and accepts all that anyone has ever called eolithic. I think we must use "prae-Chellean" instead, meaning "prae river valley-gravel". The Chellean is the oldest set recognised by the French, & we need not bother about the words Strepyan & Mesvinian of Rutot' (Lankester to Moir, 6<sup>th</sup> March 1912: BLL: RL, Add 44968/124-125; emphasis in original).

The 'eolith' affair leaves no doubt at all that words were extremely useful in implying a connection to more popular industries. The use of terms such as 'pre-

Chellean' or 'pre-palaeolithic' to describe the East Anglian industries helped to suggest that these were on the same respectable level as Palaeolithic industries, and were quite distinct from the more notorious 'eoliths'. It was far more respectable to ally the pre-palaeoliths to the accepted Chellean industry, although this caused anger in certain eolithic camps. Rutot appears to have written to Moir in 1912, accusing him of 'suppressing the eoliths', and arguing that the rostro-carinates *were* eoliths, in that they were not intentionally shaped, but 'accommodated' (the edges blunted) to fit the hand. Lankester's response was indignant: he asserted that Rutot was 'a vain ticklish old fellow & can't bear to see any one in the same field with himself', and, with marvellous unconscious irony, added 'the old silly is merely anxious about a word' (Lankester to Moir, 23<sup>rd</sup> May 1912: BLL: RL, Add 44968/161-162).

*Strategies in print and in person: approaching the opposition*

Besides sending the right message through careful use of individual words, the overall style of publications had to strike the right tone if the writer wished to gain the confidence of the reader. There was some variation in style between different specialist subjects, societies (local or national) and between individuals. However, there were certain widespread rules in constructing a convincing scientific case. More than a few times Lankester had to remind Moir of the benefits ensuing from an apparently detached, methodical, 'scientific' and therefore 'expert' stance in print, evidently concerned that Moir's 'pugnacious & suspicious' temperament and 'baseless suggestions' about opponents was 'not the way to learn all the facts and to become a real leader in the subject' (Lankester to Moir, 23<sup>rd</sup> January 1911: BLL: RL, Add 44968/21-22).

Lankester taught Moir the subtleties of writing slippery articles: 'Do not mind my telling you that you should not say as you often do "It is quite certain that" or "there is no doubt possible etc", but rather "It seems probable that" or "there is much reason to suppose"' (Lankester to Moir, 18<sup>th</sup> January 1911: BLL: RL, Add 44968/17-18). When McKenny Hughes attacked the rostro-carinates in his *Flints*, Lankester advised Moir on how to go about writing a letter to *Nature*:

'be very careful to treat him as a poor deluded nice old gentleman, who means no harm. Say it is to be regretted. Point out that he has written without knowledge, but speak of him in a forgiving spirit' (Lankester to Moir, 16<sup>th</sup> March 1911: BLL: RL, Add 44968/27-28; emphasis in original).

Lankester often had occasion to hold Moir back from personal confrontation, and emphasise the advantages of a precise and clinical published account:

‘I don’t see how Mr. Hazzledine Warren can do or say anything about your sub-Crag implements, and I should leave him alone. [...] They must wait now if they wish to talk about your things, until my account of them is out. There is no hurry!’ (Lankester to Moir, 4<sup>th</sup> January 1912: BLL: RL, Add 44968/106-107; emphasis in original)

A published article carried a certain authority, particularly if it appeared under the aegis of a prestigious society. The personal touch could be useful if handled with care. Moir recalled how Lankester, following the reading of Moir’s (1920) paper to the Royal Society in 1917 on the passage from the rostro-carinate to the hand-axes, ‘looked round in a threatening way at the audience and said: “If there are any present who do not accept these conclusions, Mr. Moir and I will no doubt be able to deal with them”’ (Moir 1935, 78). However, fierce disagreement in society rooms might also make the hopeful persuader look foolish, and Lankester again cautioned Moir:

‘I should strongly advise you to let Sollas alone. No one cares about these wrangles. Sollas will have to eat his words, as soon as my paper on the Wittlingham [sic] flint is out. It is far best to let people like Sollas jaw. They know nothing about the subject. The more you argue with them, the more importance they get. Leave him alone’ (Lankester to Moir, 31<sup>st</sup> July 1913: BLL: RL, Add 44969/71-72; emphasis in original).

It was also important that the reader was not put off the pre-palaeoliths by poor quality illustrations. Lankester’s own papers were full of superb illustrations (see Lankester 1912a; 1914), and he often warned Moir: ‘I don’t think you realize how much weight of conviction is carried by a first-rate set of drawings’ (Lankester to Moir, 2<sup>nd</sup> March 1922: BLL: RL, Add 44970/207; emphasis in original; see also Moir 1935, 58-64).

#### *Strategies of direct confrontation with the evidence: exhibitions*

Another useful strategy was to confront wavering sceptics directly with the physical evidence itself. This could succeed in winning them over, particularly if presented in an authoritative manner in an environment that was conducive to acceptance. When leafing through old society journals, brief references to past exhibits can occasionally be found interleaved between lengthy published papers, an elusive indication of an aspect of debate

that had once provided the focus for so much energy.

The exhibition of specimens in society rooms or museums, to illustrate papers or to promote general discussion, was standard practice, as will be recalled from the earlier discussion of Lankester's confrontation with Dawkins over flint-fracture at the Royal Society *soirée*. Warren exhibited the results of his flint-fracture experiments (see Figs. 3.16 and 3.17 below) at a variety of arenas, mostly to geological audiences, for reasons outlined above. He displayed specimens illustrating the origins of eoliths to the Geologists' Association in November 1900, gave a lecture on pressure chipping to the same body in 1906, and displayed his pressure-chipped notches to the Geological Society on 24<sup>th</sup> March 1909 (Anon 1909, cxxv).



Fig. 3.16: Cast made by Warren of one of the Kentish eoliths: 'Fractured flint with 2 flakes replaced as found' (PRM: Accessions Book No. X, p. 454; PRM 1940.12.161.3).

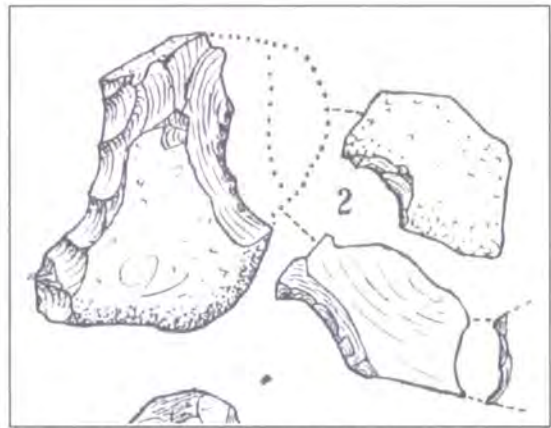


Fig. 3.17: Illustration of the Kentish eolith cast shown in Fig. 3.16, demonstrating how the two larger chips refit (Warren 1920, 240, Fig. 2).

Warren continued to attack the pre-palaeoliths with exhibitions of 'Specimens illustrating the Experimental Chipping of Flints' to the Geologists' Association on 1<sup>st</sup> November 1912 (Anon 1913, 302), and he also presented the results of his experiments alongside those of other supporters and adversaries at the Geological Society meeting of 19<sup>th</sup> November 1914 (Anon 1914, iii-iv). Moir's sub-Crag specimens made their first substantial debut in the Geological Society on 7<sup>th</sup> November 1910 (Lankester 1912a, 285), but thereafter enjoyed a more varied career.

In 1912, Lankester (1912c) informed the readers of *Nature* that Moir's specimens could be seen at the British Museum; Moir later suggested that they might view the results of his experiments at the same prestigious location (Moir 1912b, 463). It must have helped that Reginald Smith at the Museum had a certain sympathy for the pre-palaeoliths, although Moir could not prevent Warren from visiting the exhibit and publishing critical remarks (Warren 1913, 37-38). When the rostro-carinates were later on display at the museum, Moir was very particular about *how* they should be displayed (Fig. 3.18), writing to Sir Hercules Read:

'May I suggest that when exhibited, the specimens should be posed in the rostro-carinate manner [sketch] not as most palaeoliths are shown [sketch], as then their significance is more easily recognized' (Moir to Read, 22<sup>nd</sup> November 1919: BM(F): Misc. Doc. Files, Moir).

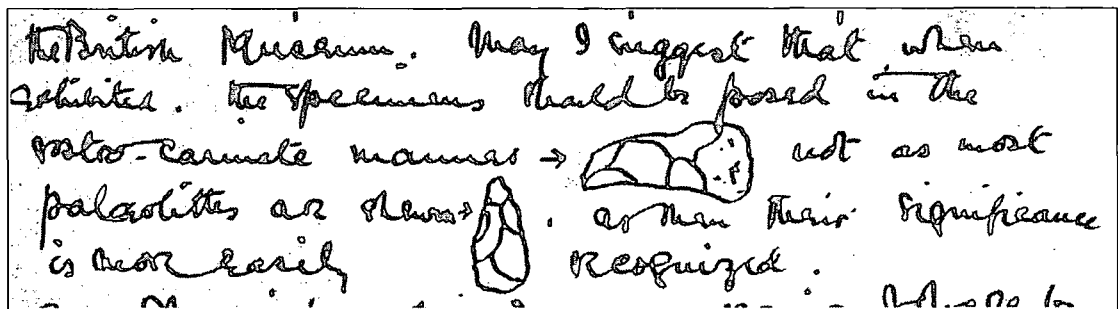


Fig. 3.18: Moir's suggestion to Sir Hercules Read about the ideal arrangement of his rostro-carinates in the British Museum exhibition (Moir to Read 22<sup>nd</sup> November 1919: BM(F): Misc. Doc. Files, Moir).

The choice of where to display exhibitions followed similar reasoning to decisions about where to present papers. In a letter written during the later years of the debate, Lankester made it clear to Moir exactly where he could and should exhibit his finds. There was the Geological Society, 'the place where you have most *claim* to exhibit since you are a fellow'. There was also the Royal Society, where Lankester had 'obtained a promise from the Secretary [...] that I shall have a large table'; and Lankester also outlined plans for cases in the Jermyn Street (Geological Survey), British, and Natural History Museums, the latter requiring 'careful coaxing of Harmer & also Smith Woodward'. He continued, 'Once you can place the things on view, you can send people (through a special note in "Nature") and get one of the officials to keep the key & show the things to visitors' (Lankester to Moir, 7<sup>th</sup> February 1921: BLL: RL, Add 44970/143-146; emphasis in original).

This process involved personal standing in societies, knowing the right people, and approaching them correctly. It also required access to some form of reputable advertising space, for which *Nature* did very nicely. Moir and Lankester had previously used *Nature* as a prestigious platform from which to announce their early discoveries, and thereafter used it as a field for more general announcements and sniping (for example, Lankester 1912b; 1912c; 1921, Moir 1912b; 1912d; 1916; 1921c).

The presentation of the Ipswich discoveries was carefully managed, and Lankester kept a careful eye on Moir's activities. Moir gained a firm foothold in his local society, the PSEA, whilst Lankester approached the national societies with a combination of cautiously written articles, and a belligerent personal presence. Both favoured more archaeological arenas for their promotion of what they saw as an archaeological case, whilst the details of flint-fracture experiments were often brought up before geological societies. Meanwhile exhibitions displaying 'artefacts' and the results of experiments were set up by both sides. Each gathered together a network of supporters, winning them over in person, through papers and correspondence, or direct confrontation with exhibits; and the same arenas were used to attack the opposition.

*'Those tricky Frenchmen': 'intelligent men & experts'*<sup>19</sup>

Personal confrontation was a common strategy in society-room debates, as discussed above. When the opponent in question commanded widespread respect, the value of their conversion was greater as this might well initiate an extensive shift in opinion, and the effort that went into securing their conversion was consequently magnified. In 1913, Lankester reminisced to Moir, 'Those tricky Frenchmen have not yet published any notice of my paper or of their visit to England' (Lankester to Moir, 18<sup>th</sup> March 1913: BLL: RL, Add 44969/59-60). He was referring to a visit by Marcellin Boule and Henri Breuil to Ipswich in September the previous year, when he and Moir had made every effort to secure the conversion of these tricky, but influential, Frenchmen who had published such devastating attacks on the eoliths in the past.

The events of 1912 were particularly remarkable for a number of reasons. First, there were the obvious stratagems used by Lankester and Moir to control the social and

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<sup>19</sup> Lankester to Moir, 18<sup>th</sup> March 1913; 19<sup>th</sup> September 1912: BLL: RL, Add 44969/59-60; 44968/193.

intellectual context of the visit of Boule and Breuil in the hope of manipulating the opinions of the visitors. Second, there was the clash between two groups who approached the question from two different perspectives: as an archaeological industry, and in the light of recent papers on flint-fracture (Boule 1905; Breuil 1910). Third, the reaction of Breuil is of particular interest in the light of his response to certain pre-palaeoliths shown him by Moir some years later – of which more below.

Around the time of this visit, there was a widespread feeling in Britain that the French were taking the lead in palaeolithic questions (Sturge 1908, 9; Boule 1915, 1-2). They certainly had an influential hold over the question of natural *versus* human flaking, writing the two famous anti-eolithic papers described above in section 3.3.1 (Boule 1905; Breuil 1910). In addition, Boule had also recently become the Director and Breuil a Professor of Prehistoric Ethnography at the prestigious new *Institut de Paléontologie Humaine* in Paris, established in 1910 by Prince Albert of Monaco (Boule 1912a; Burkitt 1925, 14; Broderick 1963, 65-68).<sup>20</sup> The opinion of these two individuals was much respected in Britain, and their possible conversion to the pre-palaeolithic cause had the potential to alter the attitude of many significant British opponents.

Lankester had already demonstrated an interest in gaining Continental support for the pre-palaeoliths before their visit, urging Moir to go to Paris and Brussels armed with his letters of introduction and show researchers such as Boule and Rutot the best pre-palaeoliths and illustrations (Lankester to Moir, 4<sup>th</sup> January 1912: BLL: RL, Add 44968/106-107). Casts had been sent by Lankester to the *Musée de Saint-Germain* via Boule, who was invited to come to England to see the originals and their associated geology (Boule 1915, 9). Moir was eager for Breuil to come and see the sites: Lankester sent his address and advised Moir to send casts of his 'best & most convincing specimens' (Lankester to Moir, 29<sup>th</sup> May 1911: BLL: RL, Add 44968/37-40; emphasis in original). Lankester was hoping, even more audaciously, to welcome Prince Albert of Monaco (1848-1922), benefactor of the Paris *Institut* and patron of Breuil and Boule, to Ipswich in September 1912. In the end the Prince found himself unable to attend, but

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<sup>20</sup> Broderick notes that Boule and Breuil never got on particularly well with one another: Boule was secretive, and Breuil never turned in satisfactory expense accounts to Boule who directed the *Institut* (Broderick 1963, 152).

Boule and Breuil were still expected.

News travelled fast and the prospect of such distinguished visitors caused great excitement amongst British Palaeolithic researchers, but Lankester was determined to keep the event under his thumb. He informed Moir, 'there must be no fuss & intrusion of strangers or even local geologists etc. They want to go over the thing with you and me' (Lankester to Moir, 2<sup>nd</sup> July 1912: BLL: RL, Add 44968/172). Sollas was determined to attend the September party, however, and Lankester's attempts to prevent this illustrate the importance of the visit, and the value attached to a careful orchestration. His private opinion to Moir on the Sollas problem is worth quoting in full: 'Sollas wants to come. He will only be a nuisance, but I shall try to manage him. He will inevitably try to draw their attention off, on to *other* & irrelative problems' (Lankester to Moir, 5<sup>th</sup> September 1912: BLL: RL, Add 44968/189; emphasis in original). Lankester managed Sollas with the following explicit missive:

'with regards to Boule & Breuil. They are as you know coming over expressly to see for themselves the sub-Crag implements and the actual conditions in which they are found. I have given up my holiday & have gone & shall go to expense & trouble in various ways to ensure their seeing what I wish them to see, and I do not wish to have any chance of their attention being turned from the evidence.'

'If they should be able to go to Oxford after they have done with me – well and good. But this (here) is my special and, as I think, very urgent affair and I must beg you to leave me un-embarrassed and to leave them also free to give their attention to the matter for which they have undertaken to come over. I do not want any assistance in explaining to them the geology of this district – nor do I wish to have other audience & witnesses to distract my attention & theirs. I do not know how long it may take or how long they may be able to give, & I desire that no unnecessary limit shall be put to their enquiry here, or to my guidance of their movements' (Lankester to Sollas 19<sup>th</sup> September, undated, ?1912: BLO: Eng. lett. d. 329/14; emphasis in original).

Even the Cambridge geologist, Marr, who had been so geologically supportive in Lankester and Moir's publications, was not allowed to come, and when Moir approached Lankester to ask if he could attend, the response was apoplectic.

'I most emphatically object to any one coming to take part in our inspection or to "look up" the French geologists whilst they are with me. No one ought to know when or where they are coming, excepting you and me. This is a serious job and not a pic-nic: and if any one tries to join us or take part in our visitation, I shall decline to let them do so. it would be too fatiguing, full of delays & jaws & altogether a failure. [...] What we have to do is to bring Boule & Breuil

face to face with the facts and, since they are intelligent men & experts, let them draw their own conclusions. Whatever information they want, you & I can give them and no one so well' (Lankester to Moir, 19<sup>th</sup> September 1912: BLL: RL, Add 44968/192-193; emphasis in original).

Lankester also took the opportunity to make it absolutely clear to Moir in the same letter what subjects were permitted within the hearing of the Frenchmen, and which were forbidden:

'Pray do not (unless there is time after we have seen everything) raise the question of fracture by sand-pressure, or any other story than that of these sub-Crag flints. Once that is dealt with, all other hares may be let loose for them to hunt. But not before. [...] I do not wish any one to be told what day we expect B&B, nor where they will stop or go or when they will leave' (Lankester to Moir, 19<sup>th</sup> September 1912: BLL: RL, Add 44968/193).

This correspondence emphasises the importance attached to the opinion of the Frenchmen, both 'intelligent men & experts', and the necessity of bringing them within a controlled environment where the maximum amount of influence could be brought to bear. Even Moir had to be warned against distracting them from the main point: the sub-Crag flakings, or of attacking them – the question of fracture by sand-pressure above was a reference to Breuil's (1910) Belle Assise paper, the usual argument of the eolith supporters being that the flints could not have been broken by pressure transmitted through sand.

*'Those tricky Frenchmen': 'two hasty foreigners'*<sup>21</sup>

However, as events turned out, the Frenchmen were not impressed, and remained unconvinced. Lankester and Moir were furious, and Lankester certainly never forgave Breuil even after his later conversion.

'My dear Mr. Moir,

Pray keep a calm attitude about Boule & Breuil. A matter of inference based on tangible evidence, can not be dealt with by the mere opinion of two hasty foreigners who deliberately chose not to consider the specimens & other facts which are opposed to their fanciful notions.

Who cares if Sollas or any one else is rejoiced? Don't allude to it, or take any notice of it. By & bye I shall have to write more on the subject, but note this –

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<sup>21</sup> Lankester to Moir, 18<sup>th</sup> March 1913; 15<sup>th</sup> October 1912: BLL: RL, Add 44969/59-60, 7.

neither Breuil nor Boule made any close observation of the really best and convincing specimens: Breuil talked like a conceited school-boy who had just learnt some terms about flints & wanted to show it. His manner was impudent & arrogant (that of the priest) and he absurdly said, abruptly of the Lakenheath specimen of Sir John Evans "That is simply a chisel." Of course it is not. But he kept trying to make "assertions" showing his shrewdness. I don't consider that either he or Boule have any adequate knowledge of flints, especially older palaeolithic ones – Remember Breuil's absurd notion that the weight of sand could fracture the flints of Belle Assize. Boule was very anxious to assert that the striae are not due to glacier action. Who cares whether they are or not? Breuil wanted to trace early fracture to one of the scratches. Silly!

Yours sincerely,

E. Ray Lankester' (Lankester to Moir, 15<sup>th</sup> October 1912: BLL: RL, Add 44969/7-8; emphasis in original)

So the 'intelligent men & experts' (Lankester to Moir, 19<sup>th</sup> September 1912: BLL: RL, Add 44968/193) became 'two hasty foreigners' (Lankester to Moir, 15<sup>th</sup> October 1912: BLL: RL, Add 44969/7) who rapidly dismissed Lankester's selection of convincing specimens. Far from offering themselves as willing converts, the Frenchmen seem to have attempted to convert Moir and Lankester using very similar strategies. In the field they appear to have taken advantage of the situation to explain their natural-flaking sites. As Lankester recalled:

'The more I think it over the more I am disgusted by Boule & Breuil. They were and behaved as absolutely prejudiced persons – not fair-minded enquirers. Boule with his "histoire de Mantes" & Breuil with his "affaire de Bel assize" wish in the most arrogant & childish spirit to bring everything concerning such flints, as are not of the most obvious & orthodox character & history, under one or other of these two explanations.

From the first they started on this line and never even looked carefully at the best specimens which are so definite in shape & in chipping as to render their "Mantes and Belle assize" theory absurd' (Lankester to Moir, 12<sup>th</sup> October 1913: BLL: RL, Add 44969/79-80; emphasis in original).

Possibly the most hurtful rejection was their dismissal of the Lakenheath specimen of Sir John Evans (see Figure 3.19 below), Lankester's *piece de resistance*. This 'rostrum-carinate' had also come from Suffolk, and, more significantly still (for Lankester and Moir at least), it had been discovered in the collection of the late Sir John Evans himself (Lankester 1912a, 295-299). However, this attempt to gather the name of a hero from the past to support a present cause cut no ice with the French, and would probably have been received indignantly by Evans himself.

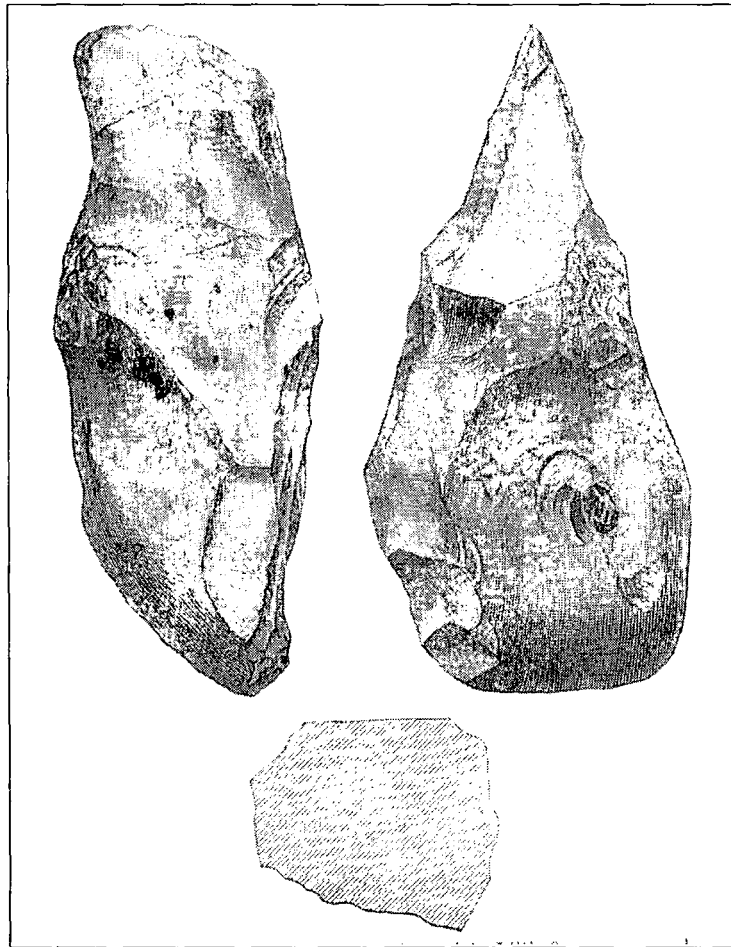


Fig. 3.19: The Lakenheath implement (Evans 1897a, 567, Fig. 444).

A few months later, Breuil echoed Lankester's opinion that the other side was absurdly prejudiced, remarking to Haward:

'as with all their "Eolithic-loving" confreres, it is difficult to discuss with these gentlemen. They affirm their opinions with too much enthusiastic conviction which prevents them from appreciating the rights of others to doubt' (Breuil to Haward, 27<sup>th</sup> February 1913, translated typescript: BM(F): WAS, 'Cromer, Norfolk', F.N. Haward coll.).

Just as the English pair had earlier sent him their most convincing specimens, it appears from this letter that Breuil had also, at some unspecified point in the past, shown Moir and Lankester 'the best flints from Belle Assise', but 'M. Reid Moir would not say that these were not made by Man. Sir Ray Lankester was more prudent: he said "that they were not due to Pressure"' (Breuil to Haward, 27<sup>th</sup> February 1913, translated typescript: BM(F): WAS, 'Cromer, Norfolk', F.N. Haward coll. (trans.); emphasis in original).

The strategy of taking an opponent to the field or showing them a specimen could be more persuasive than printed argument, but in this case it seems to have developed into a face-off between the two pairs of men. All four participants saw themselves as experts in their field and each had a number of reputable publications safely behind them which made them loath to change their opinions. Boule and Breuil tried to make Lankester and Moir see evidence of natural-flaking like that at Mantes (Boule 1905) and Belle Assize (Breuil 1910); Moir and Lankester showed them their best specimens and were surprised that the Frenchmen could not see past the specifics of the flaking to the archaeology.

However, it was a clash of personalities as well as approaches that resulted in the failure of Lankester's careful scheme. Lankester was clearly annoyed that Breuil had not been more deferential, describing him as a 'conceited little Jesuit' (Lankester to Moir, 18<sup>th</sup> April 1915: BLL: RL, Add 44969/159-160), and Moir records that 'his feelings on the matter were not by any means concealed during our tour' (Moir 1935, 143). Moir's own hot-headed nature must also have been a great handicap to diplomacy.

*The second visit: Breuil's conversion at Foxhall Hall in 1920*

Although the arguments employed by Lankester failed to strike the right chord with Breuil, it is clear from subsequent events that he was on the right track in his estimate of the response to Breuil's conversion. In 1919, in an old Crag pit at Foxhall Hall, Moir discovered two layers of finely stratified flakes (Fig. 3.20) *within* the Red Crag deposits (Fig. 3.21). These flakes were found in sand, were not in contact with each other (Moir 1921a, 399-401), and appeared to represent convincing evidence of an ancient *in situ* working floor with no possibility of natural fracture (Moir 1924b, 647).

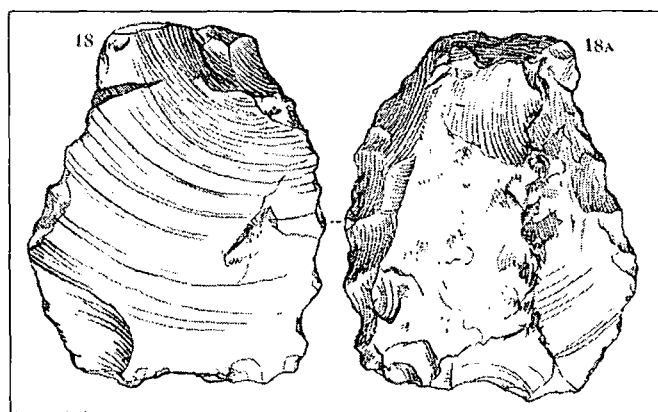


Fig. 3.20: 'Two views of *racloir* from pit at Foxhall Hall' (Moir 1921a, 409, Figs. 18 and 18a).

Moir took particular care to point out that the Foxhall Hall pit excavations (see Fig. 3.21) had been conducted in such a way as to preclude any possibility of post-Crag specimens falling down into the lower horizons (Moir 1921a, 398). Henri Breuil, on a second visit to Ipswich in 1920, now changed his mind and accepted Moir's Foxhall Hall flints. The situation in which they had been found seemed to preclude the possibility of natural fracture, and Breuil saw evidence of human workmanship in the fracture characteristics of these flints.<sup>22</sup> Driving back with Miles Burkitt, his old pupil, Breuil quietly observed, '*aujourd'hui a beaucoup vieilli l'humanité*' (Burkitt 1944, 369) ('Today has greatly aged humanity').

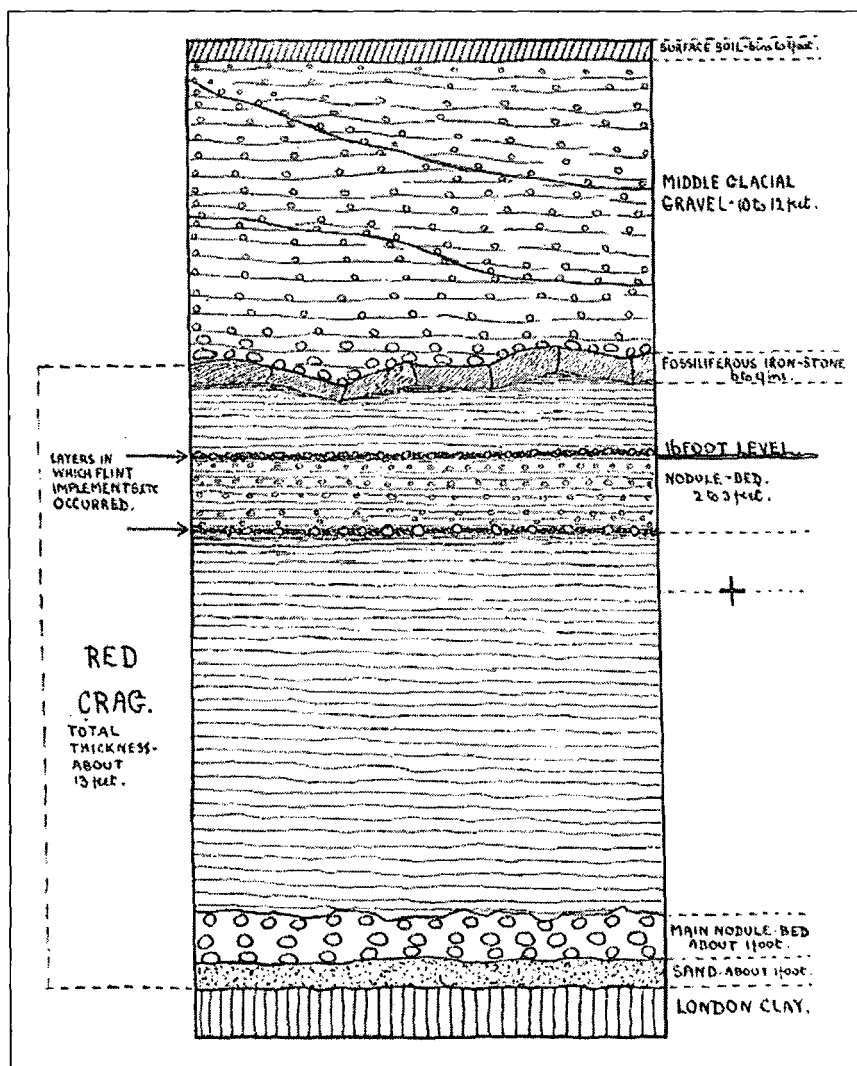


Fig. 3.21: 'Diagrammatic section of western face of pit just south of Foxhall Hall' (Moir 1921a, 400, Fig. 3).

<sup>22</sup> However, Mark White, who has examined the Foxhall Hall specimens more recently, cannot see any evidence of human workmanship in these flints (Mark White, pers. comm. 2003).

The news spread rapidly. Sollas, whom Moir angrily described rejoicing at Breuil's earlier rejection of the pre-palaeoliths, now added a postscript to his 1920 paper in the *Proceedings of the Prehistoric Society of East Anglia*, congratulating Moir on a 'well-deserved triumph' (Sollas 1920, 267).<sup>23</sup> Sollas also wrote to Dawkins to explain why this event had catalysed previous doubts into a full turnabout, and his letter, which is worth quoting in full, is full of 'Breuil':

'Dear Sir Boyd Dawkins,

Things have happened since the Abbé was working in your laboratory! He has been to Ipswich & admitted many of Moir's flints from the Crag to be human implements. With this recognition on the part of Breuil the one objection which I made to the artefact nature of the specimens I described before the Geol Soc falls to the ground. So I am joined to Breuil, Marr, Birket [sic] and the rest over this momentous issue. Nevertheless I went to London to listen to what Ray might have to say, this more especially as the rostro-carinates are not among the forms which Breuil accepts! But Ray was not there. Paper taken as read.

Nothing has done more to retard the acceptation of Moir's implements than Lankester's insistence on the merits of this one type; it is this which has diverted attention from much more impressive documents.

I may add that Moir has found a true working floor in the Norwich Crag with hearths I believe & goodness knows what. Breuil went to see it.

Nothing is published yet. Breuil thinks the Piltdown skull is probably the same age as the Crag things.

Ever truly yours

W. J. Sollas' (Sollas to Boyd Dawkins, 27<sup>th</sup> June 1920: BMD: WBD, DERSB 70494).

Soon afterwards, Moir sent Sollas 'a series of sub Crag flints all of which were specially favoured by Breuil' (Moir to Sollas, 26<sup>th</sup> ?September ?1920: BGS: GSM1/445). Sollas's letter to Boyd Dawkins reveals why these flints from Foxhall Hall were so much more convincing to Breuil than the specimens he had seen in 1912. On that occasion, Lankester showed Breuil selected types, as we have seen above. However, considering that Breuil was steeped in scepticism and flint-fracture experiments, he was more interested in ascertaining evidence of human workmanship. Therefore, an insignificant flake with a bulb of percussion from a geological context where natural fracture seemed unlikely was far more persuasive than any number of rostro-carinates – which, though as

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<sup>23</sup> This paper entered the proceedings having been rejected by the Geological Society of London, and described a particularly convincing sub-Crag specimen of Moir's that Sollas seemed almost to accept before concluding that he suspended his judgement (Sollas 1920).

impressive to Lankester's eyes as any Acheulian hand-axe, were still not accepted as true artefacts by Breuil (Breuil 1921a, 357).

Burkitt gave explicit details about the factors taken by Breuil as evidence for their human workmanship in a letter to Sollas:

'Breuil at Ipswich paid no attention to a trimmed edge alone nor a wavy flake, even if there was a bulb. These latter are common in the boulder clay and Breuil considered them probably natural. What he considered most was the presence of fairly long flat flakes with bulb etc. and more so if these had a trimmed edge, the face of the flake showing no striae which could be associated with the trimmed edge.

He pointed out to me that the trimmed edge alone; or the flake alone; even the presence of striae did not preclude the possibility of human workmanship. But the association of (I) flake (II) trimmed edge (III) absence of striae made the human agency almost certain. I always feel too that the presence of small chipped awl-like forms is significant for surely nature would have broken anything so fragile!' (Burkitt to Sollas, 10<sup>th</sup> October, undated: BGS: GSM1/445)

Breuil's attention was drawn to the character of the flaking, which he believed was not explicable by any natural processes – to the bulbs of percussion, retouch, and absence of striae. He was also taken with the geological context of the finds, which not only supported a great age but had also appeared to have preserved the finds *in situ* where they had been dropped after manufacture (Breuil 1921a, 357). In contrast, Moir's earlier pre-Crag finds were distanced from any primary context that suggested recognisably human activity, and many of his pre-palaeolithic groups had been selected from a single deposit of unstratified flints on the basis of differences in patina, form and flaking of pre-palaeoliths.

### 3.4: Conclusions

Breuil's conversion cut a swathe through the opposition, just as Moir and Lankester had hoped. The impact was extensive in Britain. Indeed, the quantity of prestigious reversals alarmed Hazzledine Warren, who delayed giving his opinion on the Foxhall Hall flints, in part because 'the change of opinion of so many of the highest authorities has been a severe shock, which gave one pause to think twice' (Warren 1922b, 87).

In France, Breuil seems to have persuaded almost everyone apart from Boule in a series of meetings of the *Institut international d'Anthropologie* in 1921 and 1922 (Burkitt 1921, 457; Breuil 1922, 226-229; Lohest and Fourmarier 1923, 56; Capitan 1923, 67). A commission selected by the *Institut* set off for Britain in September 1922, visited Moir's sites (including Foxhall Hall), and a number of collections including Warren's pressure flakings from the sub-Tertiary Bullhead Bed at Grays, Essex. This was Warren's finest discovery of a natural-flaking site; it had yielded a series that Breuil thought approached the Belle Assise flakings, and even included rostro-carinate forms (Warren 1920, 248). However, the commission members concluded that these were very different in character from Moir's sub-Crag specimens (Hamal-Nandrin and Fraipont 1923, 58). The tables had turned since Breuil's dismissal of Lankester's Lakenheath rostro-carinate in 1912.

Inspired by the Foxhallian and the older pre-Red Crag industries, the American palaeontologist, Henry Fairfield Osborn, was also led to make great changes to his prestigious correlation table that he had published in his 1916 book, *Men of the Old Stone Age* (Osborn 1916; Osborn and Reeds 1922, 471). Osborn, who was also an old friend of Lankester's, saw Moir's Foxhall Hall finds adding humans, whether *Homo* or *Eoanthropus*, to the Tertiary fauna (Osborn 1922, 440). Commenting on Osborn's paper in the *American Naturalist*, Lankester remarked:

'One would think from what Osborn writes that the Foxhall flint really started your discoveries or made a great difference in their value – That seems to me to be an erroneous view of the case.' (Lankester to Moir, 6<sup>th</sup> March 1922: BLL: RL, Add 44970/209-212; emphasis in original)

But was this an erroneous view of the case? It seems that Lankester once again had not fully appreciated the perspective held by Breuil. The eventual collapse of Breuil's opposition does indeed seem to have been triggered by Moir's flints from Foxhall Hall. However, this was a conversion on Breuil's own terms, based on an approach that was largely alien to the perspectives of Moir and Lankester. Breuil was convinced neither by Lankester's rostro-carinate nor, in the first instance, by Moir's progressive evolutionary sequence of pre-palaeolithic industrial groups. The social context of research formed an undeniably important part of academic perception and procedure, and considerable advantages could be accumulated through strategic social manipulation. Nonetheless, fundamental differences in expectation and outlook meant that the two sides often never engaged fully with each other's arguments. The corpus of what passed for accepted knowledge amongst fellow workers in palaeolithic archaeology might have appeared self-evident on the surface but, digging deeper, it seems that many individuals could not see past their own approaches and expectations, and were persuaded by different aspects of the case.

## CHAPTER 4

### **Early Palaeolithic research in the early twentieth century: institutional collaborations and complex industrial sequences**

'I believe [...] in the fundamental interconnectedness of all things' (Dirk Gently, in *Dirk Gently's Holistic Detective Agency*, Douglas Adams 1993, 118).

During the early twentieth century the identification of a number of ancient flake-dominated industries of Early Palaeolithic age led their discoverers to ask how such industries were to be classified and interpreted within the confines of the Eolithic-Chellean-Acheulian-Mousterian sequence popularised by de Mortillet (Mortillet 1873, 436-437; 1883, 131; 1900, 21; see Chapter Two). Some suggested that this classic sequence would have to be adapted to incorporate the new variety of industries, although there was little consensus as to how this should be carried out. The confusion that such industries caused offer a revealing insight into the reasoning and perceptions behind British Palaeolithic research before Henri Breuil came to dominate the scene in the 1930s (a subject that forms the focus of Chapter Five).

Two such flake-dominated industries, both discovered in Britain in the early twentieth century, provide the focus for this chapter. One was recovered by Reginald Smith (1874-1940) and Henry Dewey (b.1876) in the Thames Valley during the 1910s; the other was found by Samuel Hazzledine Warren at Clacton-on-Sea, Essex. Both industries later became more widely known as the 'Clactonian' and continued to arouse much debate (see Chapter Five). At the time, they were dubbed respectively the 'pre-Chellean' (or 'Strépyan') and the 'Mesvinian'. This chapter explores the events which led to these discoveries and the kinds of interpretations they stimulated, and provides a picture of the social practice of British Early Palaeolithic research in the early twentieth century. These 'anomalous' industries also stimulated some important changes in perception of the Early Palaeolithic record which are discussed in detail below.

This chapter has been divided into three sections:

- The first section (4.1) explores the social and institutional context of British Palaeolithic research in the early twentieth century and its influence on the content and character of possible interpretations. This analysis complements the details provided in Chapter Three on the social manipulation behind debate and provides a fuller picture of the social and institutional context of British Early Palaeolithic research. Fortunately, much of the official correspondence between the respective employers of Smith and Dewey has survived in the Geological Survey archives, which offers a detailed insight into their collaboration. The restrictions and motivations facing non-professional researchers, such as Hazzledine Warren, are also assessed.
- The second section (4.2) examines the development of the classic Thames Valley sequence by Smith and Dewey in 1912 and 1913, and suggests some of the areas of inspiration for their expectations and interpretations.
- The third section (4.3) inspects the changes that were made to the old linear Chellean-Acheulian-Mousterian framework as more contemporaneous industrial diversity became recognised, both on the Continent and in Britain. The various threads of argument are finally brought together by a discussion of the changing interpretations of the flake-dominated industry from Clacton-on-Sea between the early 1910s and the early 1920s.

1860	Prestwich and Evans establish the antiquity of man (Prestwich 1860a; Evans 1860)
1861	Fourfold palaeontological classification of the Palaeolithic by Edouard Lartet (1861)
1864	Early distinctions between palaeolithic industries (Lartet and Christy 1864)
1872	Evans's first edition of <i>Flint Implements</i> (Evans 1872)
1869, 1873, 1881, 1900	De Mortillet and the French sequence (Mortillet 1869; 1873; 1881; 1900)
1880s	Work by Spurrell and Worthington Smith on the British Palaeolithic, including technology and refitting (Spurrell 1883; 1884; Smith 1887)
1898	J.W. Kenworthy collects flakes from Clacton-on-Sea (Warren 1932b, 20)
1890s-1900s	Rutot promotes the pre-Chellean industries from Belgian (Rutot 1898; 1900; 1903)
Early 1900s	Hugo Obermaier begins his researches
c. 1906 – mid 1910s	Commont outlines the Early Palaeolithic sequence of the Somme (Commont 1908; 1909; 1910; 1911; 1912a; 1912b)
1906, 1908	Obermaier's (1906b; 1908) suggestions of parallel flake industries in the Lower Palaeolithic
1908	Warren starts collecting and observing on the Clacton foreshore
1911	Sollas's (1911) first edition of <i>Ancient Hunters</i>
1912-14	Collaboration of the Geological Survey and British Museum in the Lower Thames, downstream of London, results in the excavations at Swanscombe which form the foundation of the British industrial sequence (Smith and Dewey 1913; 1914)
1915	Sollas (1915, 134) announces the re-dating of Rutot's Belgian industries (Commont) in his second edition of <i>Ancient Hunters</i>
1919	Obermaier publishes his theory of the hand-axe and non-hand-axe geographical zones within Europe (Obermaier 1919)
c. 1921	Breuil visits Warren and identifies his industry as 'Mesvinian' (Breuil 1930, 221)
1922	Warren's first major paper on the Mesvinian industry from Clacton-on-Sea (Warren 1922a)

Table 4.1: Time chart: key events in Early Palaeolithic research from the late-nineteenth century to the early-twentieth century.

#### 4.1: Institutions, individuals, and impediments: the social and institutional context of palaeolithic research in the early twentieth century

In the early twentieth century, as Victor Commont worked away in the Somme Valley, James Reid Moir began to publish on the pre-palaeoliths of East Anglia, and Hazzledine Warren scouted the Essex shores for crude palaeolithic flakes, some British researchers began to feel nostalgic for the glorious decades of the past when respectable British giants such as John Evans and Joseph Prestwich dominated the field of Palaeolithic research (Sturge 1908, 9). And in 1912, Henri Breuil remarked at a luncheon party in Cambridge that no one in England knew anything about prehistory (Burkitt *c.* 1960, undated typescript notes, p. 14: ULC: Add 7959, Box 3).



Fig. 4.1: Nels Nelson, Paul Wernert, Hugo Obermaier, Miles Burkitt and Teilhard de Chardin in Spain, 1913 (postcard from Burkitt to his mother, June 1913: ULC: Add 7959, Box 1). Reproduced courtesy of Cambridge University Library.

Continental workers had recently been confronted with groundbreaking work in the Somme (by Commont), rich hauls from Upper Palaeolithic cave sites in the Dordogne (by Capitan) and Spain (by Cartailhac, Breuil, and Obermaier; see Fig. 4.1), and Breuil's own modification of de Mortillet's Upper Palaeolithic succession – one of the highlights of the 1906 Monaco Congress. France could also boast certain institutional advantages, such as the *Institut de Paléontologie Humaine* in Paris

mentioned in Chapter Three, which had been set up in 1910, and where Boule, Breuil and Obermaier enjoyed prestigious posts (Boule 1912a; Burkitt 1925, 14).

Returning to Breuil's comment, it was true that only ten British delegates had attended the 1912 International Congress of Prehistoric Anthropology and Archaeology, in contrast to over thirty-five French delegates. Amongst the small British contingent were William Sollas and Reginald Smith (Anon 1912). Smith (Fig. 4.2), one of the main characters in this chapter, was also concerned at the state of British Palaeolithic research (Smith 1912, 137-138). However, early in 1912, he had taken an important step towards remedying the situation.



Fig. 4.2: Reginald Smith (1874-1940), reproduced courtesy of the Society of Antiquaries of London.

Reginald Smith was Assistant Keeper in the Department of British and Mediaeval Antiquities and Ethnography at the British Museum. He was described by a colleague as 'a shortish man, bald, little moustache, pince-nez, stove-pipe collar, dark-coated' and 'an austere vegetarian', but who concealed a kindly nature and 'was

always ready to talk about flowers and robins and suchlike, and [...] liked to be told of outstanding displays in florists windows' (Kendrick 1971, 2, 5). The Keeper of his Department, Sir Hercules Read (1857-1929), shared Smith's concern over the current state of Palaeolithic knowledge (Smith 1912, 137-138; Read to Strahan, 11<sup>th</sup> February 1914: BGS: GSM2/544: H).

For his part, Smith observed that collectors of stone tools were ignoring the dating benefits of stratigraphy, whereas geologists often took little notice of the time-specific nature of palaeolithic artefacts. He concluded that 'Collaboration is obviously needed to straighten out our pleistocene geology' (Smith 1912, 138), and put these words into action only a week after he had made this observation, writing to Hercules Read to suggest a collaboration with the Geological Survey of Great Britain at 'one of the finest gravel-pits in the world': the Thames Valley exposures of Barnfield Pit, Swanscombe (R. Smith to Reid, 19<sup>th</sup> February 1912: BGS: GSM2/544; Smith and Dewey 1913, 177).

This was a propitious time for such a proposal, since the Geological Survey had just started an extensive re-survey of the London area. Although the Survey was interested in such temporary exposures for the glimpse they provided at underlying strata, as yet it had no methodical way to exploit such ephemeral opportunities. The usual strategy was to work over the ground systematically, sheet by sheet, but the Survey had not yet arrived at the Swanscombe area (Survey Sheet 271). Smith wanted to ascertain the succession of palaeolithic implements at Barnfield Pit whilst the work of the Associated Portland Cement Manufacturers progressed; they were planning to remove the palaeolithic gravels over the next two months to reach the chalk beneath. A reliable geologist would prove an excellent and valuable companion, and Smith's efforts to secure Henry Dewey, a Survey Geologist, for this role led, in 1912 and 1913, to the Geological Survey / British Museum joint venture at Swanscombe. A little later this collaboration produced co-authored publications by Smith and Dewey (1913; 1914), which would become classic works (Kennard 1916, 253; Bury 1923, 39; Oakley 1939, 357).

What was the relation between these institutions and Palaeolithic research in the early twentieth century? The Geological Survey has already made a brief

appearance in Chapter Two as one of the major employers of Quaternary researchers in the second half of the nineteenth century. In the early twentieth century the Survey was still one of the largest official institutions concerned with Pleistocene geology in Britain. Although the universities were starting to attract a solid core of researchers devoted to palaeolithic matters, many of whom had been plucked from the Survey, it would be several decades before they came to dominate this field.

The British Museum supported palaeolithic research and collaboration with the Survey for a number of reasons. It was trying to shrug off a reputation for being more an old-style collection of artefacts than a contributor to knowledge (Read to Teall, 24<sup>th</sup> June 1913: BGS: GSM2/544). Sir Hercules Read, Smith's superior, was worried that Britain was lagging behind the Continent, and this made him very eager to exploit the opportunities offered by new exposures. An assured geological opinion would certainly strengthen any archaeological interpretations offered by Museum staff. It was factors such as these which led to the Swanscombe excavations; the histories of these institutions and the motives of their members would colour the conduct of their collaboration and the portrayal of their conclusions.

#### ***4.1.1: The Geological Survey of Great Britain***

Since its foundation in 1853, the work of the Geological Survey had been devoted to mapping the geological deposits of Great Britain. A scientific purpose was fostered alongside the more practical matters of agriculture, mining, or water-supply, and the Survey officers gradually amassed much information on the context of palaeolithic finds. As an institution, the Survey provided one of the few professional opportunities for those interested in the geology of Britain, and employment was not restricted to gentlemen of means but was dependent on ability and a Civil Service exam. However, increasingly poor pay meant that the Survey was swiftly becoming a fast track to university postings, where more intelligent, ambitious, or less devoted Survey men seeded the chairs that would later lead palaeolithic research. William Boyd Dawkins, William Sollas and James Geikie had all occupied Survey positions before taking up their university chairs, as noted in Chapter Two.

Around 1910, the Survey was revising its maps for the district around London and the south-east, an area under the eye of the District Geologist Clement Reid (1853-1916), pictured in Figure 4.3 below (Flett 1937, 161). As always, the main concern of the Survey was geological stratigraphy, and the maps, memoirs, and collections of type specimens produced by Survey Geologists provided an unrivalled source of information for Palaeolithic researchers, many of whom kept a few battered Survey publications close at hand for quick reference. The variety of Quaternary researchers contributing to Palaeolithic research has been mentioned in Chapter Two, and Henry Dewey, who had joined the Survey as a Clerk in 1903 and became a Geologist in 1906, now joined this diverse band.

As we shall see, his employers could be defensive of time spent on projects that were perceived to be lying outside official Survey business, and Dewey's work on the borderland subject of Palaeolithic research would later become fettered with greater restrictions. But in 1912, thanks to the foresight of Clement Reid (who was himself an interested contributor to the subject) and the flexibility of the Assistant Director, Aubrey Strahan, Dewey would begin his work at Swanscombe.

Table 4.2: The Geological Survey in 1912.

*Jethro J.H. Teall*, Director of the Geological Survey (retired 1914)  
*Aubrey Strahan*, Assistant Director of the Survey of England and Wales  
*Clement Reid*, District Geologist (retired 1913)  
*George Barrow*, District Geologist  
*Henry Dewey*, Geologist

#### **4.1.2: The British Museum**

The British Museum, where Reginald Smith held the position of Assistant Keeper, was one of the few institutions to offer professional employment in Palaeolithic research. Although Smith, like Dewey, had little time for independent work, he was fortunate in having an understanding superior: Hercules Read.

Table 4.3: The British Museum in 1912.

Director of the British Museum  
*Sir C. Hercules Read*, Keeper of the British and Medieval Department  
*Reginald Smith*, Assistant Keeper of the British and Medieval Department

However, posts were rare, and to fill one required luck or influence. Reid Moir had written to Ray Lankester very soon after making his famous Pre-Palaeolithic discoveries on the possibility of starting a career in prehistoric archaeology (around the time that his father was pressurising him to spend more time on the family business) but received a dismal reply:

‘I am really very sorry that you think of giving up business & taking a post connected with prehistoric archaeology, because there are no such posts. [...] You could not get into the British Museum or any of the larger museums, since they require men who have trained when young, and even then they get little opportunity for independent work. You are far better off – free and independent, giving your leisure to prehistoric work – and making an income in quite a different way. But in any case, there really is nothing for which you could apply – so you should, I think, dismiss this notion altogether’ (Lankester to Moir, 12<sup>th</sup> August 1911: BLL: RL, Add 44968/53; emphasis in original).

Swanscombe was an interesting experiment in collaboration on the part of these two institutions, each with entirely different aims and responsibilities and peopled by individuals with their own particular interests. Reginald Smith and Hercules Read were fired by the desire to develop British Palaeolithic research, and the British Museum would also benefit from any collections that were made. However, Henry Dewey, though equally interested, seems to have received rather less support from his Survey employers, who perhaps stood to gain less from the project. Despite Dewey’s personal interest in the sites of the Lower Thames, it would be no surprise that the Survey was the first to break off the official collaboration.

#### ***4.1.3: Collaboration in action: the British Museum and the Geological Survey at Swanscombe***

In February 1912, Reginald Smith suggested to Hercules Read that the Barnfield Pit exposures might repay a collaborative geological and archaeological investigation.

Smith was eager to promote a closer alliance between the Museum and the Survey, and wrote to Clement Reid, the respected Survey District Geologist in charge of the revisions of the London area:

‘Mr. Dewey & I are ready to take vacation if a private fund has to be raised for the purpose, but I sincerely hope that it will be recognised as proper & desirable work by both institutions, and lead to a fuller cooperation in the future. Though much time & money has been expended officially for excavating ancient sites in Egypt, Assyria, Asia Minor, Cyprus, etc., I do not think this Museum has ever undertaken similar work in England, & the time has now come to advance beyond mere collecting from workmen & others, & to ascertain the sequence of the gravels by reference to the implements & faunistic remains’ (R. Smith to Reid, 19<sup>th</sup> February 1912: BGS: GSM2/544).

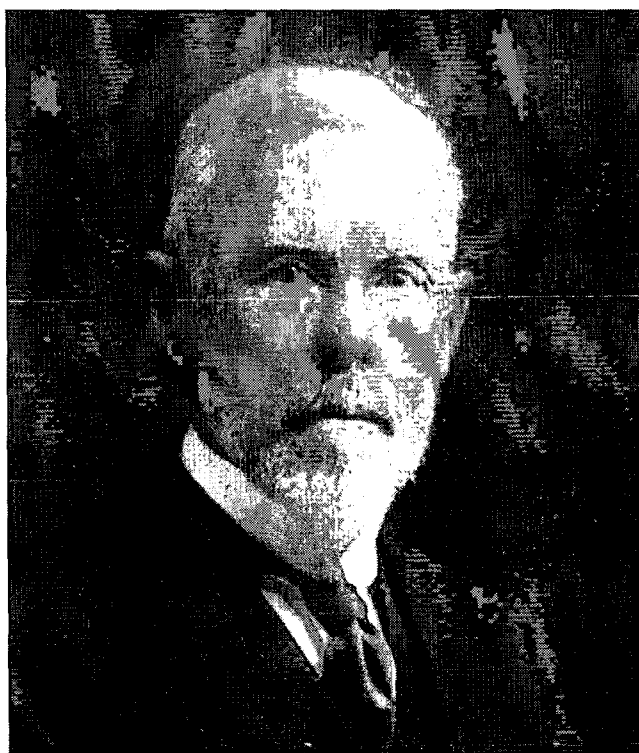


Fig. 4.3: Clement Reid (1853-1916), reproduced courtesy of the Geological Society of London.

Clement Reid immediately contacted his superior, Dr Aubrey Strahan (1852-1928), the Assistant Director of the English Survey, requesting that Dewey be allowed to observe the excavations at Barnfield Pit, a request that was granted. In March, the cost of hiring labourers was split between the British Museum and the Geological Survey. Both institutions benefited in terms of information and specimens for their respective museums, and it seemed a happy union. Hercules Read ‘borrowed’ Dewey (often at Smith’s instigation) from the Survey on several further occasions for help on

the geological aspects of other exposures in the London area such as the shell bed at Greenhithe (Ingress Vale), the Dartford Heath exposures (Wansunt Pit) where Chandler and Leach had been working, and Baker's Hole, Northfleet. The Survey acceded to Read's requests for expert geological assistance so long as this did not cut too much into Dewey's official duties.

However, by the middle of 1913, Jethro J.H. Teall (1849-1924), Director of the Survey (Fig. 4.4), began to become more concerned about the way in which Dewey was spending his time. He argued that two days would be more reasonable than a whole week at the Dartford Heath section which he feared would constitute 'a rather serious interruption of Mr. Dewey's work' (Teall to Read, 23<sup>rd</sup> June 1913: BGS: GSM2/544).

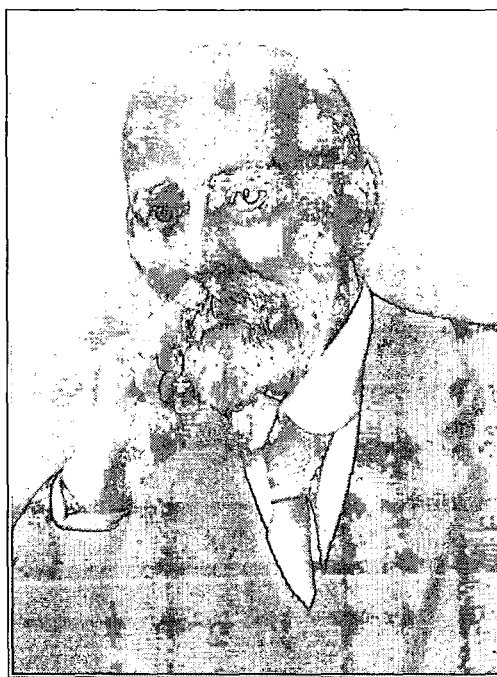


Fig. 4.4: Jethro Justinian Harris Teall (1849-1924),  
Director of the Geological Survey 1901-1914 (Flett 1937, Plate VI).

Read felt it necessary to re-emphasise their mutual gain:

'I quite understand your point of view on the obvious impropriety of your allowing Mr. Dewey to spend his official time in any matter that has no relation to his work. My ardent desire, however, in this matter, is to make it quite clear that you and we are working together in this determination of the sequence of the deposits where human remains exist. I need not enlarge upon this to you, as you know as well as I the difficulty of persuading people that our attitude is unprejudiced whether from a geological or archaeological

standpoint. If therefore, you will allow Mr. Dewey to collaborate as much as you think justifiable, that will serve my purpose, which, after all, is not ultimately confined to the acquisition of flints for the Museum, but a determination of much wider issues' (Read to Teall, 24<sup>th</sup> June 1913: BGS: GSM2/544).

This letter establishes three important points: first, Read wanted to establish the Museum as an active participant within wider research issues and counter criticism that the institution was merely interested in collecting; second, geological support was important in enhancing the credibility of archaeological claims; and third, the Museum perhaps stood to gain more from this collaboration than the Survey.

As mentioned above, the main concern of the Survey at this time was to complete the new maps of the London area and, as a long-established institution, its directors had fixed and rather military ideas on what constituted proper Survey work and how this work should be carried out. The area allotted to each Geologist to survey was comparable to a territory, and their masters could be jealous guardians of the results. When Aubrey Strahan (Fig. 4.5) took over from Jethro Teall as Director of the Geological Survey in December 1913, Swanscombe and Northfleet came within the area to be surveyed by Officer C.E.N. Bromehead (1885-1952).

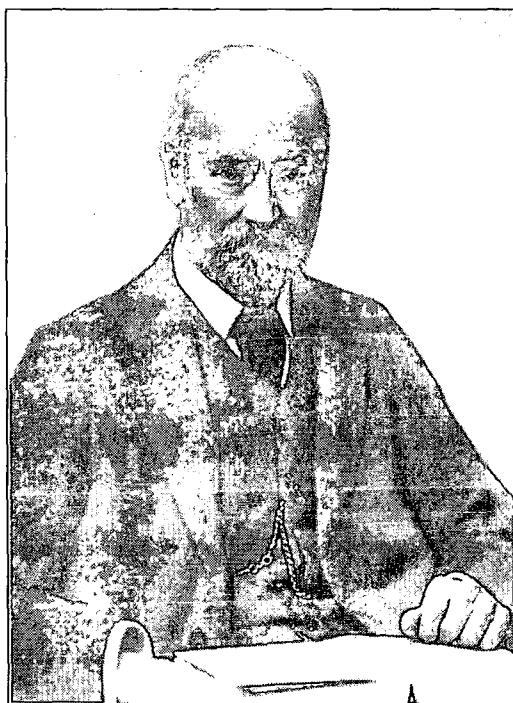


Fig. 4.5: Aubrey Strahan (1852-1928),  
Director of the Geological Survey 1914-1920 (Flett 1937, Plate VII).

Read wrote to offer his congratulations to Strahan on his appointment, hoping that the old collaboration would continue. However, since the areas that Read and Smith were interested in no longer fell within Dewey's official territory, and since Dewey's immediate superior, District Geologist George Barrow (1853-1932), was adamant that Dewey could no longer infringe upon the territory of another officer (Barrow to Strahan, 19<sup>th</sup> December 1913: BGS: GSM2/544), Read was now referred to Bromehead for information. Dewey, despite his personal interest, had to relinquish the area, and his contributions to future publications dwindled. It was unfortunate for continuity that the Survey would often acknowledge their staff to be 'experts' only on the areas they had been officially detailed to survey. Dewey might have had sound knowledge of the Wansunt pit / Swanscombe deposits, but it was difficult for him to contribute fully in the Palaeolithic question without sympathetic superiors.

The rigid structure of the Survey created many such restrictions. Survey staff had long found it difficult to publish outside the official Survey remit, a problem that was now to dog Dewey as he worked up his part of the *Archaeologia* articles co-authored with Smith. Dewey's Survey findings entered Survey literature along the usual route, appearing in the Summary of Progress for 1912, and information being passed on to the officer surveying the area. On the Museum side, *Archaeologia* was a natural choice for Read: this journal was the organ of the Society of Antiquaries, which was officially connected to the British Museum and Read was also President of this Society and wanted it to play a more active part in such research (Read to Teall, 2<sup>nd</sup> January 1913: BGS: GSM2/544; Smith and Dewey 1914, 187).

Early in 1913, Read asked Teall if Dewey could contribute to the *Archaeologia* volume. Teall agreed, and the result was *Stratification at Swanscombe* (Smith and Dewey 1913). But the Survey was less supportive the following year when Read asked the new Director, Strahan, if Dewey could spend two or three days preparing his contribution to a second paper that drew more general conclusions about the palaeolithic succession of the 100-ft. terrace, also based on sites that both Smith and Dewey had observed (Greenhithe and Crayford (Wansunt pit), as well as Swanscombe). This would become *The High Terrace of the Thames* (Smith and Dewey 1914).

However, Dewey's superior, George Barrow once again complained to Strahan, suggesting this time that the proposed paper was 'the result of a compact between Dewey and Smith' (Barrow, notes sent to Strahan, 27<sup>th</sup> January 1914: BGS: GSM2/544). Dewey had asked Barrow for a week to write up the Greenhithe and Dartford Heath work of the past year. But Barrow argued that Dewey should have no more than two or three days since he had carried out much work outside his official remit, work that should be written up outside Dewey's official time. Barrow considered that the content should be purely descriptive, covering the geology and organic remains, leaving the implements to Smith. He went on: 'the references to the area shortly to be surveyed by his colleagues & outside the pits examined should be as brief as possible' (Barrow to Strahan, 27<sup>th</sup> January 1914: BGS: GSM2/544). Barrow disapproved of the style of Smith and Dewey's former publication in *Archaeologia*, saying that:

'it is largely on things in general & other men's area all over. The Title is "Stratification at Swanscombe" etc etc

But quite a small portion of this is devoted to the real subject of the paper & the object for which he was sent by the Survey. It is largely a series of incursions into areas not allotted to him & with which he has at present nothing to do. His disquisition on things in general will not meet with general acceptance.

Further, if a man wants to specialize on work, there is a limit of time that he can be allowed' (Barrow, notes sent to Strahan, 27<sup>th</sup> January 1914: BGS: GSM2/544; emphasis in original).

Barrow clearly did not regard the Survey as a nurturing environment for Palaeolithic research. Following these discussions over Dewey's use of official time, the Director, Strahan, withdrew a little from the collaboration although he agreed that Dewey could write up his geological notes for the second *Archaeologia* article. He informed Read:

'With regard to future work I should hardly feel justified in continuing to assist in making excavations for the purpose of finding implements. To do so would endanger the carrying out of our programme of field-surveying. At the same time I should be prepared to send one of our staff to see any excavations which you may make with a view to assisting you in any geological questions which may arise' (Strahan to Read, 28<sup>th</sup> January 1914: BGS: GSM2/544).

Read's reply is most informative:

'In view of your decision for this year, we must be content with a minimum of geological assistance, but shall continue the field-work, as very little is being done privately and for the credit of the country an organised effort must be made to keep pace with continental progress. We have as good a field as anyone abroad, and if our own countrymen neglect the work, foreigners will take the opportunity, and reveal to us our own resources. There has been a good deal of stagnation in recent years, and we cannot afford to give up the undertaking, even if we did not think it as important as any other branch of departmental work. If implements can be regarded as fossils, this seems the only way of classifying the gravels and throwing light on recent geological changes' (Read to Strahan, 11<sup>th</sup> February 1914: BGS: GSM2/544).

Read had tried his best to persuade the more hostile geologists of the value of Palaeolithic research, particularly the use they might make of implements as zone-fossils. However, when Smith and Dewey carried out work in 1914 around Rickmansworth (Croxley Green and Mill End), although Smith was granted leave of absence by the Museum, 'Assistance from the geological side was given unofficially by Mr. Dewey' (Smith 1915a, 195). Dewey's contribution to the publication (Smith 1915a) consisted only of an appended geological report.

These events demonstrate that the restrictions placed upon individual efforts by their employers had tremendous consequences for the practice of Palaeolithic research. It seems that it was not necessarily an advantage to be employed by an institution with intellectual links to Palaeolithic studies. Smith was certainly luckier than Dewey. His superior, Hercules Read, shared many of his opinions about the need for this kind of research, and the British Museum also stood to gain much more than the Survey from this collaboration in the Lower Thames – both materially, in the collections it could gain, and intellectually, from collaboration with the established science of geology and its reliable Quaternary chronology.

Dewey's situation, on the other hand, illustrates the many different perceptions held by higher-ranking staff within the same institution over what kind of work the institution should support. Disagreements over this point, possibly magnified by personal differences, led to restrictions being enforced on his activities. The Survey was traditionally territorial over regional districts and intellectual property. Dewey

had always had to go through official routes for permission to publish geological information relevant to the sites and, once the new revisions had reached the Swanscombe area, he was also warned against using his official time for Palaeolithic research in areas allotted to other officers.

The Swanscombe collaborations neatly encapsulate some of the advantages and disadvantages of the larger institutions. Hierarchies and traditions could severely limit freedom to research and publish – although Reginald Smith and Henry Dewey were happy to do the work in their spare time if necessary. This brings the discussion round to the largest group of Palaeolithic researchers in the early twentieth century: those who had no choice but to carry out their studies in their spare time.

#### ***4.1.4: Warren at Clacton-on-Sea, and the research constraints facing non-professionals***

The term ‘amateur’ has attracted a certain stigma over the years. However, as explained in Chapter Two, non-professional workers of this era were welcome and industrious members of the community of palaeolithic researchers. Long into the twentieth century, the division between the collector and those who undertook detailed observation was a far more important distinction than the contrast between ‘amateur’ and professional. There was, in any case, precious little professional training or employment in Palaeolithic or in related Quaternary subjects.

Unlike Reginald Smith of the British Museum or Henry Dewey of the Geological Survey, Hazzledine Warren of Loughton, Essex, the eolith sceptic we met in Chapter Three, had no paid employment. Warren’s geological research, and his work on the eoliths, had secured for him the reputation of a cautious and careful observer, and his work at Clacton-on-Sea will be discussed in section 4.3. However, Warren had the time to devote to his researches when many of his less fortunate (non-professional) contemporaries relied on fragments of time snatched from the more monotonous task of earning a living (although Dewey might have added that this was also the case for some professionals).

William J. Lewis Abbott (1863-1933), for example, neglected his jewellers business to pursue the eoliths and was later reduced to selling his small library of archaeological works (Lewis Abbott to A.S. Woodward, May 6<sup>th</sup> 1929: BM(NH): WLA). Martin A.C. Hinton, an early specialist in Quaternary voles and lemmings, initially worked as a clerk and wrote to Geikie in 1900 to try and secure a geological position so he could devote more time to the gravels which had fascinated him from his early teens (Hinton to A. Geikie, 3<sup>rd</sup> June 1900: ULE: Gen 521/3). It was to be more than twenty years before he gained employment at the Natural History Museum (Savage 1963). Alfred Santer Kennard (1870-1948), the mollusc expert who co-authored a number of crucial papers on the Thames Valley with Hinton managed this in between time spent working for a London firm. It was only after retirement that he became a Research Specialist at the Survey Museum (Warren 1949). Reid Moir's father was angry about Moir's neglect of their tailor's and outfitter's business for the hobby of prehistory, and Moir could not devote much time to this activity until after his father's death in 1912 (Keith 1944, 740).

However, Warren was luckier than most. He was an only child, born into a family of wholesale provision merchants, a business that he left in 1903 (when he had not long turned thirty) and came to live in Essex (Oakley 1959, 144-146). Warren was not plagued by the necessity to earn a living, and was dogged by few of the restrictions imposed on those lucky enough to secure employment in the Survey, the Museums, or the universities. In his will, he left an estate of around £25,000: Warren had enough financial independence to enable him to devote his life to Pleistocene geology and archaeology. However, he was only one amongst many non-professional researchers, many of whom had to make a greater personal sacrifice to carry out their research.

#### ***4.1.5: The impact of the Great War (1914-1918)***

Before turning to the discoveries and interpretations of the various Early Palaeolithic industries recovered during these formative years in the Thames Valley and at Clacton, it is worth pushing this social history ahead just a little further in time. The wider political situation was changing as Warren, Hinton and Kennard, Smith and Dewey, worked away. Palaeolithic research would soon be shadowed by the Great

War, which made matters very difficult for certain individuals and institutions. Although this subject has received very little attention, the war had a significant impact on many individuals and on the practice of research at the time.

In 1914, Warren had become President of the Essex Field Club, a society that nurtured a variety of research areas, including prehistoric and geological subjects, and he was soon also a Commandant in the Voluntary Aid Detachment in Essex. Military restrictions on Essex forced members further afield for their excursions, and air-raids required their meetings to be held earlier to let members get home before dark (Thompson 1930, 18). As a known and respected local figure, Warren could carry on his work with impunity within the limits imposed by military restrictions, but the Survey staff had a more troubled time.

Many of the Survey officers had volunteered for military service; others helped the war effort through work in industry and commerce, and special reports on mineral resources (Flett 1937, 164). Second Lieutenant 'Rocks' King (who was later to co-author an important paper with Kenneth Oakley on the geological succession in the Thames Valley, as we shall see in Chapter Five) was sent to France to advise on water supply (Shotton 1963, 172). As the remainder of the Survey staff wandered across the tracts of British countryside allotted to them, they met with suspicion from locals, farmers, the police, and the military; they were mocked, threatened, questioned, followed, and sometimes arrested. One officer, Sherlock, was even apprehended as a German spy whilst he surveyed in Hertfordshire, where the police were suspicious of his 'foreign' (Lancashire) accent (ULLiv: PGB, D4/1, p. 107). Many complained that the Director's authorisation was next to useless in the field where few had heard of the Geological Survey (Dewey to Lamplugh, 27<sup>th</sup> June 1916: BGS: GSM2/414).

Other war-time activities by geological and palaeolithic researchers had their humorous side. William Sollas, by then in his mid-sixties, joined the Volunteer Labour Force, and:

'was sent down with a party to Didcot where they were loading high explosives into trucks. Sollas was found smoking a cigarette, surrounded by notices or posters saying, "DANGER, NO SMOKING," and he was sacked on the spot' (J.A. Douglas 1976: UMO: WJS, Box 10/17).

However, as war continued, mounting casualties thinned the ranks of talented young geologists; Warren wrote sadly of the death of Lewer, the geologist son of one of his neighbours (Warren 1916b). Despite efforts made by older scientists such as Sollas to lobby the Royal Society and the War Office for the recall of the most talented young men, the toll was heavy, and Lewer's obituary joined dozens of others in the pages of *Nature*. Sollas's stepson, the gifted young physicist Harry Moseley, died at Gallipoli in 1915 just as the War Office was initiating the recall procedure (A. Sollas to W. Sollas, 13<sup>th</sup> October 1915: BGS: GSM1/445).

More devastating for the field of prehistoric archaeology was the death of Victor Commont, whose influence on the interpretations of Smith and Dewey, and on perceptions of the Palaeolithic in general, will be discussed in section 4.2. Commont, depressed by the war, was forced to abandon his house, books and collections during a bombing raid. His death was hastened by the trauma of the subsequent evacuation of Abbeville (Boule 1918-19, 162). Sollas bitterly recalled: 'he died a victim to the organised robbery with murder which was the German war. I mourn a friend and Science one of her most gifted sons' (Sollas 1924, 140, footnote 5). The researches of Hugo Obermaier, the influential German prehistorian and geologist (whose contributions are described in the final section of this chapter) were also disrupted by the war, which cost him his post at the prestigious French *Institut* in Paris (Breuil 1946, 272).

Peace did not bring an immediate return to pre-war conditions and many experienced considerable financial difficulties following the rise in the cost of living, particularly food prices and income tax. Although Warren had enough financial independence to devote all the time he desired to archaeological and geological research, the paltry Geological Survey pay became even more of a problem than it had been in the past (see Chapter Two). Staff increases, which had been building up since the previous century, had further diminished prospects of promotion, and many were leaving for university posts.

Those who remained faithful to the Survey now held an even bigger grievance against the Survey for not improving their pay and conditions. Survey officers were highly trained professionals with university qualifications, but received far less than other, similarly experienced, members of the Civil Service. The question of pay and promotion had reached a critical point, and Dewey was prominent amongst those pressing for better conditions. He complained to Charles Dinham, a Geologist on the Survey staff:

‘It is not only the fact that we are underpaid but the want of proper respect for our work that causes indignation. It is within the knowledge of many of us that men of about our own age who hold positions as lecturers or professors at the Universities are regarded by our Superior Officers with more respect than we are.

In some cases they are men who were formerly on the Survey & who left us for their own advancement so that the irritation is increased by the thought that our loyalty is regarded as either timidity or stupidity’ (Dewey to Dinham, 17<sup>th</sup> November 1917: BGS: GSM1/291).

Having gathered information from fellow Geologists, Dewey prepared a petition for better wages and sent a Memorandum to the Director in 1918. This document clarified rising discontent about the use of the Survey as a fast-track to university employment:

‘the truth is, not only that chances of promotion are few, but that for most (where several men are approximately of the same age and standing) such chances are non-existent. There is normally little prospect, for four out of every five Geologists, of rising beyond the initial scale of salary, except occasionally for a year or two at the end of their careers. For these reasons the Survey is apt to be regarded less as a career in itself than as a stepping-stone to more remunerative appointments’ (final draft of Memorandum sent back to Dewey by Dinham, 18<sup>th</sup> March 1918: BGS: GSM1/291).

#### ***4.1.6: In summary***

Universities, museums, and the Survey were the main sources of employment for those interested in the Palaeolithic. Although some non-professionals were desperate to enter such institutions, there were often as many restrictions inside as out. Financial problems led Survey men to jump for university posts, where there was also more flexibility in the choice of research topics. But even in universities and museums, palaeolithic research, if it was on the agenda at all, formed a very small part of official

work and had to be pursued in one's spare time. Palaeolithic research was not yet fully established within such institutions, and it would not be for a number of years that specialist palaeolithic research posts appeared in the universities. Even when Miles Burkitt, Breuil's old pupil, began lecturing on prehistoric archaeology at Cambridge after the war, he did this on a voluntary basis until the early 1920s, and even then he only received £10 a year until he was officially appointed to the post in 1926 (Burkitt, typescript notes, undated, pp. 16-17: ULC: Add 7959, Box 3).

In these early years of Palaeolithic research, professionals and non-professionals alike, from a number of different backgrounds and with interests in a number of different disciplines, converged upon the study of the British Palaeolithic. The discipline today owes much to the attitude of Reginald Smith and Henry Dewey, who were 'ready to take vacation if a private fund has to be raised for the purpose' in order to observe the exposures at Barnfield Pit (R. Smith to Reid, 19<sup>th</sup> February 1912: BGS: GSM2/544).<sup>24</sup>

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<sup>24</sup> Many professionals still undertake excavations *gratis* and most research excavations, performed in the spirit of the Swanscombe excavations of the early 1910s, depend on such goodwill.

#### **4.2: Classification and expectation: Swanscombe, the Somme Valley sequence, and the Strépyan**

The complexities and restrictions of the institutional context within which Smith, Dewey, Warren and others were working was matched by equally complex and restrictive perceptions about what they expected to find. In the early twentieth century, around the time of Smith and Dewey's research, the most widely accepted Palaeolithic classification described a progressive line from the Chellean industry to the Acheulian to the Mousterian. However, certain researchers were already finding the French Early Palaeolithic subdivisions too simplistic to encompass the industrial variety present in British deposits (Sturge 1908, 13-14; 1911, 61, 65; Abbott 1911, 460). There was a great desire to understand more clearly how the different industries were patterned through the British deposits, and how this compared to other findings. Although Smith and Dewey provide an important focus for this chapter, their work was accompanied by many other attempts to solve this problem (see Hinton and Kennard 1905; Chandler and Leach 1912; Bury 1913; Kennard 1916).

A similar atmosphere had enveloped Continental research. Aimé Rutot was defending the existence of flake-dominated industries from Belgium which he thought pre-dated the Chellean. Victor Commont, until his death in 1918, was working on an influential sequence of deposits in the Somme Valley, building on and elaborating upon de Mortillet's Chellean-Acheulian-Mousterian scheme (see Chapter Two). Hugo Obermaier (1906b; 1908) had recently found it necessary to insert some new stages within the old sequence, which would invoke considerable interest. A variety of industries had been recovered in the course of such research which seemed anomalous in terms of the classic scheme popularised by de Mortillet: they appeared to have the wrong industrial character for their age. Some of the attempts to adapt the classic sequence to incorporate such industries would invoke a significant shift in the perception of Palaeolithic industrial patterning and in the interpretations of the ancient activities that such patterns reflected.

Certain important influences must first be introduced before exploring this complex shift in perception. The work of Smith and Dewey at Swanscombe, their

discoveries, and the reasoning that lay behind their interpretations, give an idea of the prominent industrial sequences that influenced research around this time, and the more general expectations that directed their interpretations. Some of these have already been raised in Chapters Two and Three: the dominance of typologically-attractive retouched tools such as the hand-axe; the pervasive assumptions of progression; and the distinction between core and flake tools. However, in this case, the use of these structuring principles is particularly interesting. At Swanscombe, Smith and Dewey discovered an Early Palaeolithic industry that lacked distinctive type-fossils and was dominated by flakes: an industry of similar character (although apparently not of similarly anomalous age) to the growing number of problematic industries recognised in Britain and on the Continent.

#### ***4.2.1: The Swanscombe sequence: a type section for the British Early Palaeolithic?***

De Mortillet's Chellean-Acheulian-Mousterian sequence was by now commonly used in Britain, although there were a number of suspicions as to its reliability. In 1909, in his Geological Survey Memoir on *The Geology of the London District*, H.B. Woodward made particular mention of the zealous researches of Prestwich, Evans, W.G. Smith, Allen Brown, and more recently of Warren, Hinton and Kennard, and remarked upon their introduction into Britain of the nomenclature used by de Mortillet. He also noted that the Early Palaeolithic of Britain was characterised by successive Chellean, Acheulian and Mousterian industries (Woodward, H.B., 1909, 78). However, Woodward added that Hercules Read of the British Museum had expressed the opinion that "no chronological order such as the French scheme implies has yet been established in this country". Soon afterwards, Read put his wholehearted support behind Smith and Dewey's research at the Swanscombe sites. Their conclusions appeared to confirm the essence of the French scheme.

#### *Discoveries at Swanscombe*

By the end of the 1912 season at Barnfield pit, Swanscombe, Smith and Dewey had recovered a large and convincing Chellean assemblage with hand-axes from the Middle Gravel, and a flake-dominated industry from the Lower Gravel below (Smith and Dewey 1913, 182-186). However they had failed to find any Acheulian hand-axes above the Chellean-rich deposits at Barnfield pit (Smith and Dewey 1913, 192). As

we have seen in Chapter Two (section 2.2.4), the Acheulian was characterised by hand-axes of finer craftsmanship than the older Chellean. At the time when Smith and Dewey were working at Barnfield pit the Acheulian was thought to be dominated by ovates, in contrast to the cruder pointed forms of the Chellean (see Table 4.4 below). Workmen recalled that some patinated Acheulian hand-axes had been found at these levels in the past, and Smith and Dewey had recovered others from neighbouring pits (Smith and Dewey 1913, 192-193). They also knew that such artefacts had been collected from Barnfield Pit in previous years, but no record had been made of their exact horizon (Smith and Dewey 1913, 189-190).

The exact stratigraphical position of these distinctive white-patinated, ovate Acheulian implements therefore afforded the main object of the 1913 season of investigations (Smith and Dewey 1914, 187). This time, they emerged from the expected stratigraphical level at Craylands Lane pit, just across the road from the Barnfield pit. Further authenticity was added to the observation by the fact that 'One of the authors was present for the extraction of one such implement' (Smith and Dewey 1914, 190). A large number of flakes were obtained from the overlying deposits, and these resembled the Levallois flakes of the Le Moustier period, well known from the site of Northfleet (Baker's Hole / Southfleet pit) (Smith and Dewey 1914, 190; see also 1913, 195). In conclusion, after two seasons' work:

'The earlier palaeolithic sequence seems therefore to be completely represented in the gravels of the 100-ft. terrace of the Thames, the two pits bordering Craylands Lane being complementary to each other in this respect, and the deposits ranging from pre-Chelles to Le Moustier times' (Smith and Dewey 1914, 190).

These two seasons had been very successful, and with their publication of these findings, Smith and Dewey appeared to have established a firm foundation for the French sequence in Britain (Dewey 1915, 112; Kennard 1916, 253). Smith continued to promote this industrial succession in his papers (Smith 1915a; Dewey and Smith 1924) and Dewey's position in the Survey gave their conclusions an *entrée* into the standard Survey series of Memoirs (Dewey and Bromehead 1921). However, this classic Thames sequence was founded upon a more complex basis than the observations summarized briefly above and the simplistic old French sequence. The

expectations and conclusions of Smith and Dewey were also derived from the recent conclusions of prominent contemporary researchers and from old traditions of classification.

#### ***4.2.2: The Swanscombe sequence in a broader context***

Like Smith and Dewey, Continental researchers had also been working on the old French Early Palaeolithic sequence with its three successive Chellean, Acheulian and Mousterian industries. Over the past few years, more detailed industrial analogies had recently become available, and these had an important influence on British Palaeolithic research. Amongst the industrial sequences most frequently mentioned in Britain were those formulated by Victor Commont, who was working on the classic Palaeolithic sections of France in the Somme Valley; and by Aimé Rutot, who had reported some industries of very different character from Belgium earlier in the century (Dewey 1931, 147). Rutot's Belgian researches featured in Hinton and Kennard's early synthesis of the Lower Thames industrial succession (Hinton and Kennard 1905), and would be drawn upon by Smith and Dewey. However, unlike Hinton and Kennard in 1905, Smith and Dewey were working at a time when they also had the advantage of Commont's recent publications (Boule 1918-19, 163). The industrial stages outlined by Commont appear to have had an immense influence, not only on the expectations of Smith and Dewey (McNabb 1996, 35), but also on Early Palaeolithic research in general.

#### *Victor Commont, the Somme Valley sequence, and Thames Valley analogies*

A few years before Smith and Dewey began their collaborative venture in the Thames Valley, Victor Commont (1866-1918) had been working on the Palaeolithic deposits of the Somme Valley in North France. Commont, a self-taught geologist, palaeontologist and prehistorian, carried out his researches in time spared from his work as professor of sciences at the *École Normale* at Amiens (Reinach 1919, 197). His attempts to unravel the Palaeolithic sequence of North France, published in a number of papers from 1906 onwards, were widely respected in France and Britain alike. Commont managed to confirm, clarify and add further detail to the old French Chellean-Acheulian-Mousterian sequence popularized by Gabriel de Mortillet (see Fig. 4.6 below), and the succession of industries that he traced in the river-terraces of

the Somme Valley inspired much British research (Sturge 1911, 99, footnote; Underwood 1912, 138; Dewey 1913, 163; 1919, 49).

Reginald Smith had noted a close match between the French classification and known facts in Britain only a week before he wrote to Reid in February 1912 to suggest the collaborative venture at Barnfield Pit, at a time when the Chellean-Acheulian-Mousterian sequence was still obscure in the Thames Valley (Smith 1912, 137, 141-142). The connections between Commont's conclusions about the Somme succession (Commont 1908; 1910; 1911; 1912a; 1912b), Smith's (1912, 138) expectations of the British sequence, and his interpretation of the Swanscombe sequence, are summarized in Table 4.4 below.

<i>The industrial succession of the Somme Valley (Commont 1908; 1910; 1911; 1912a; 1912b)</i>	<i>Smith's expectations (Smith 1912)</i>	<i>The Swanscombe sequence (Smith and Dewey 1913; 1914)</i>	
		<i>Industry / level</i>	<i>Description</i>
'Acheuléen supérieur': Lanceolate hand-axes with glossy white patina	St. Acheul II: Small oval and slender pointed hand-axes	St Acheul I and II: Possibly from the base of the Upper loam	Unstratified finds from Barnfield Pit (finely worked ovates), mostly with white patination, from old British Museum collections (Smith and Dewey 1913, 189). Stratified finds from Craylands Lane pit (Smith and Dewey 1914, 190)
'Acheuléen inférieur': Various hand-axe forms, dominated by oval types ('limandes') flaked all the way round the edge, often twisted. Also varied and specialised small tools	St. Acheul I: Limande dominates, finely flaked, cutting edge all round, often twisted		
'Chelléen évolué': Finely-worked triangular hand-axes (Montières type)	Chellean: Pear-shaped or flat ovate hand-axes. Coarsely flaked, often retaining cortex. 'Ficrons' are succeeded 'limandes' later in the Chellean	Chellean: Middle Gravel	This yielded 'the finest implements of Chelles types, that is practically all but the ovate specimens'. Pointed, roughly chipped, more or less pear-shaped, with little secondary working. Rare ovate forms were described as heralding the later St Acheul types (Smith and Dewey 1913, 185-186)
'Chelléen typique': Large hand-axes, thick butt, coarsely flaked, often retaining cortex. The elongated 'ficron' is the characteristic form. Also a variety of small tools			
Pre-Chellean: Crude hand-axe prototypes and many other smaller instruments	Strépyan: Nodules, flaked, generally at the point	Strépyan / pre-Chellean: Lower Gravel	'The "industry" consisted almost exclusively of thick flakes' 'implements are exceptional ... hand-axes of the ordinary type are entirely wanting'. A few nodules, possibly worked, 'correspond to the Strépy culture' (Smith and Dewey 1913, 182-183)
	Eolithic: Mesvinien, Mafflien, Reutelian. Nodules, not designedly chipped		

Table 4.4: A comparison of the industrial succession developed by Victor Commont from his Somme Valley researchers and the expectations and interpretations of Reginald Smith before and after the Swanscombe excavations.

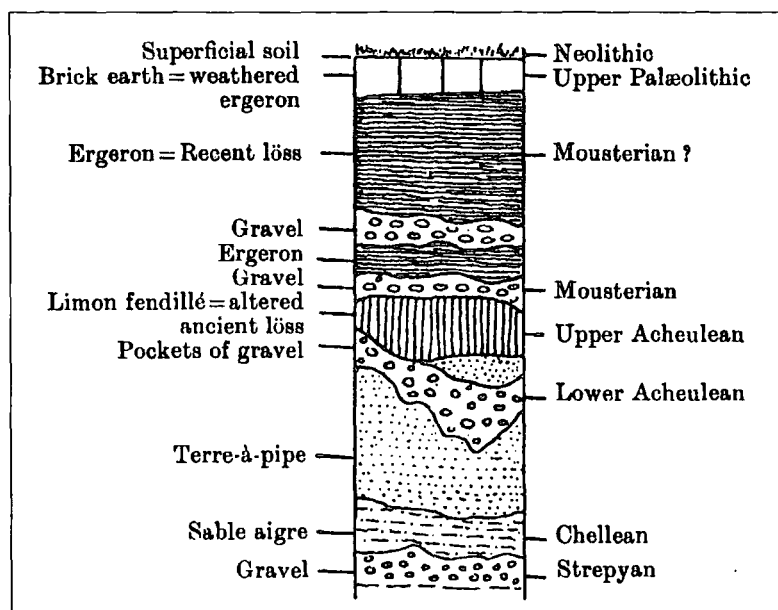


Fig. 4.6: 'Palaeolithic deposits at St. Acheul' (Sollas 1911, 102, Fig. 41).

Smith appears to have been determined to find a reliable British parallel to the findings of Commont in North France (Smith and Dewey 1913, 197, 200). The position of the flake-dominated industry from the Lower Gravel beneath the enormous Chellean assemblage of the Middle Gravel at Barnfield pit was enough to fulfil the stratigraphical criteria for a 'pre-Chellean' industry (the use of the term 'Strépyan' for this industry, a term that was rarely used by Commont, will be explained below). Despite the almost wilful absence of an Acheulian element over the 1912 season, we have seen above how Smith and Dewey doggedly added this industry to the first two on the basis of previous discoveries, suggestions by workmen, and conclusions drawn the following years from neighbouring pits (Smith and Dewey 1913, 185, 189, 191-192; 1914, 190).

The perception which went down in history was that a satisfactory line of Strépyan-Chellean-Acheulian-Mousterian industries had been established in the Thames Valley, and that a full section had been recovered, centred largely upon Barnfield pit. It seemed that, at last, Britain had a reliable equivalent to Commont's *feted* Somme succession. As Dewey later said: 'Here [Swanscombe] the result of the researches undertaken on behalf of the Geological Survey and the British Museum was the substantiation of the classification adopted on the continent' (Dewey 1926, 1433).

It was no coincidence that a *résumé* of Commont's work, including a direct parallel between the sequence of deposits and industries of Abbeville (Carpentier pit) and the Thames Valley (Barnfield pit), appeared in Smith and Dewey's 1913 paper just before they drew their own conclusions. Reginald Smith certainly seems to have expected to recover such a sequence in the Thames Valley, and this expectation was fulfilled. The British scheme was now part of a wider Continental pattern. Although Smith and Dewey protested that this was a local sequence and the evidence had not 'been twisted into agreement with supposed parallels elsewhere', they admitted: 'We owe much to his inspiration and example' (Smith and Dewey 1913, 196, 197).

However, it is interesting to see that although this sequence became accepted by many of their colleagues in Britain, Commont himself did not agree entirely with their interpretation. In a private letter, written around the time of the first Swanscombe publications, Commont made the following fascinating, but unqualified remark to Sollas:

*'à Swanscombe, à Barnfield, peut-être que M. Smith a des tendances à établir trop de subdivisions dans les alluvions de la terrasse au 100ft.'* (Commont to Sollas, 1<sup>st</sup> January 1913: BGS: GSM1/445).

*'at Swanscombe, at Barnfield, perhaps Mr. Smith has a tendency to establish too many subdivisions in the deposits of the 100ft. terrace.'*

Regardless of its origin, lying somewhere between expectation and observation, this close similarity between the accepted Thames Valley sequence (and soon the British sequence in general) and the Somme Valley succession as articulated by Commont was to have important historical consequences. Jumping ahead into a topic of the next chapter, Henri Breuil's dominance over interpretations of the British Palaeolithic in the 1930s, and his success in introducing a far more detailed and rigid Palaeolithic framework, must have been connected to the fact that both he and many British researchers had built their expectations upon the common ground of Victor Commont's industrial sequence.

#### ***4.2.3: Interpretations of the flake industry from the Lower Gravel at Barnfield pit: analogies and expectations***

Another more general problem also directed the interpretations of these researchers as they tried to build up reliable and comprehensive industrial sequences. How were Early Palaeolithic industries dominated by flakes to be classified in a research tradition that was dominated by the hand-axe form, and where the credibility of an industry was partly dependent on the presence of distinctive type-fossils? In the classifications of Commont, and of Smith and Dewey (see Table 4.4 above), the distinctive hand-axes bore a heavy typological burden, and took a prominent place alongside the smaller tools, flake-implements, and flakes. Similarly, in Chapter Three we have seen how Moir and Lankester emphasised the rostro-carinate core-tool in their promotion of a Pre-Palaeolithic period.

Such approaches are not particularly surprising considering the value of distinctive tool forms in identifying different industries, distinguishing between them, and placing them in order. It is also worth noting that further to the east of Europe, hand-axes were rare or absent. However, the dominant Palaeolithic research tradition had emerged in western Europe, an area once peopled by the makers of Palaeolithic hand-axes and occupied more recently by some influential French prehistorians. Before turning to the way in which Smith and Dewey approached the flake-dominated industry from the Lower Gravel of Barnfield Pit, it is worth taking a brief look at Commont's use of hand-axes in his industrial succession from the Somme Valley first, to see just how useful these were in defining different industries.

Commont described a slow evolution of hand-axes from pre-Chellean prototypes to the summit of the Acheulian (including transitional forms), each exhibiting more skill than the last (Commont 1908, 571; 1909, 47).<sup>25</sup> The crude prototypes of the '*pré-chelléen*' (Commont 1912b, 246; see Fig. 4.7) were succeeded by the hand-axes of the '*Chelléen typique*' amongst which the long 'ficron' was characteristic, particularly at the site of St. Acheul (Commont 1912b, 246; see Fig.

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<sup>25</sup> Terminological note: in the following chapter, which focuses on Breuil's version of the industrial succession, Commont's pre-Chellean became Breuil's Chellean; his Chellean, Breuil's Early Acheulian; his Acheulian, Breuil's Upper and Final Acheulian; and his warm Mousterian, part of Breuil's Levalloisian (Breuil and Koslowski 1931, 465, 467; 1932, 32).

4.8).<sup>26</sup> Then came the more finely-worked triangular hand-axes of Montières (Fig 4.9), Commont's '*chelléen évolué*', that he had initially suspected might reflect a contemporary tribal or functional *facies* of the 'ficron' form (Commont 1909, 66; 1910, 206; 1911, 72). By the time of the '*Acheuléen inférieur*' the oval '*limandes*', which had existed in the Chellean (Fig. 4.10), were dominant (Fig. 4.11). These were followed by the lanceolate forms of the '*Acheuléen supérieur*' (Commont 1912b, 245).



Fig. 4.7: A primitive hand-axe from the pre-Chellean levels of Saint-Acheul (Commont 1908, 537, Fig. 8; this would become the Chellean of Breuil: see Breuil and Koslowski 1931, 465, Fig. 5).

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<sup>26</sup> The term 'ficron' derived from a similarity in form to the iron point of a punt-pole (Smith 1912, 142).

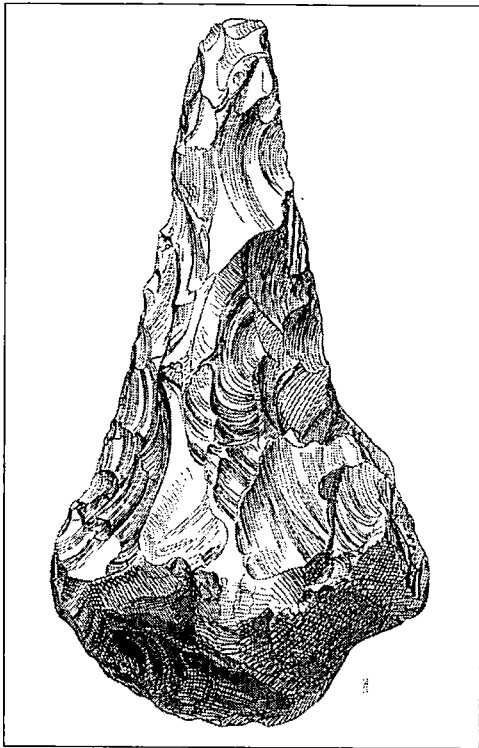


Fig. 4.8: A 'ficron', characteristic of Commont's '*chelléen typique*' (Read 1902, 17, Fig. 5; Smith 1912, 139, Fig. 12).

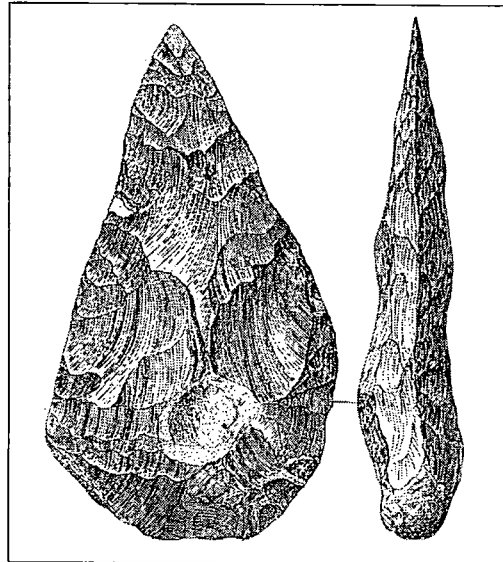


Fig. 4.9: Triangular hand-axe from Montières: Commont's '*chelléen évolué*' (Commont 1911, 74, Fig. 3).

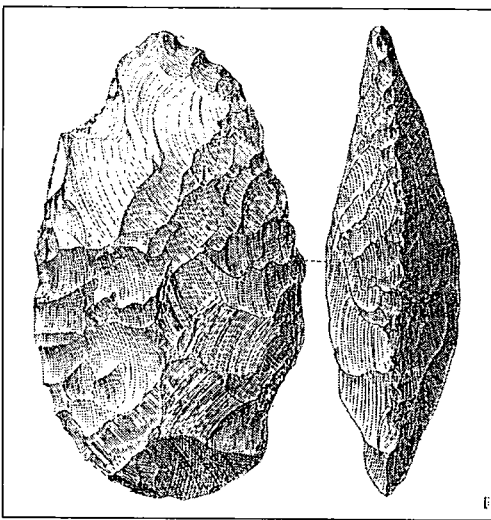


Fig. 4.10: A Chellean limande, from an industry otherwise dominated by elongated and pointed forms of hand-axe (Commont 1908, 548, Fig. 32).

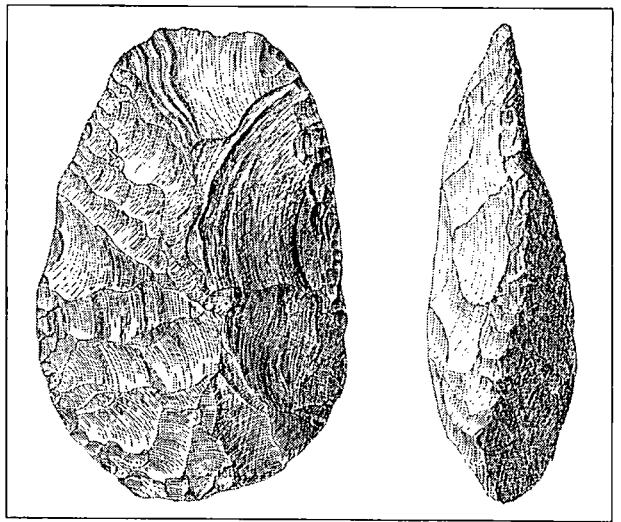


Fig. 4.11: An Acheulian limande, now the dominant hand-axe form within Commont's '*acheuléen inférieur*' (Commont 1908, 559, Figs. 57 and 58).

The idea that workmanship grew more skilled over time comes across very clearly in this catalogue of hand-axes, and the great value of such distinctive but varied retouched type-fossils in developing an industrial sequence is evident. However, the industry of the Lower Gravel of Barnfield pit contained none of these useful implements, and this made it more difficult to decide on their cultural affinity. In the words of Smith and Dewey this industry ‘was rather a surprise both as to its quantity and quality’ (Smith and Dewey 1913, 182). ‘The “industry” consisted almost exclusively of thick flakes, with prominent bulbs of percussion and a minimum of flaking, due to use or shaping, on the edges’ (Smith and Dewey 1913, 182); ‘implements are exceptional at this horizon, while handaxes of the ordinary type are entirely wanting.’ (Smith and Dewey 1913, 182-183; see Fig. 4.12).

The traditional reliance on certain distinctive core-implements caused much confusion for those attempting to interpret such industries. The artefacts from the Lower Gravel did not even qualify as ‘implements’, a term that was usually used to refer to well-known retouched artefact types, and sometimes only to retouched tools made from a nodule, and not from a flake (Hinton and Kennard 1905, 91) – a use of words that must also have reinforced the conceptual division between core and flake artefacts.

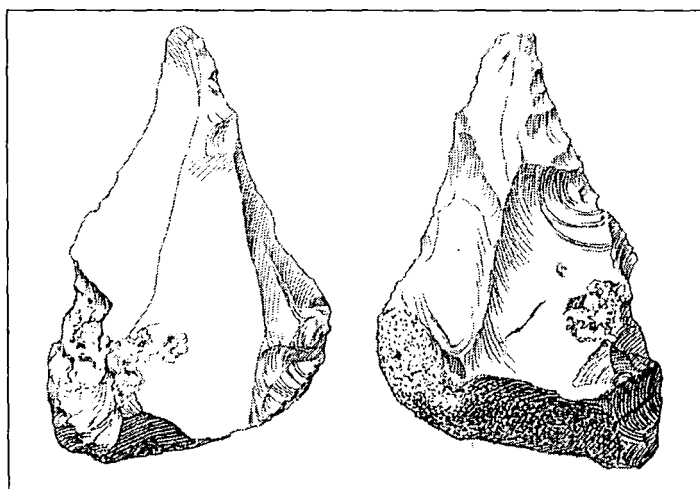


Fig. 4.12: This was the nearest approach to an implement made by any of the flakes from the Lower Gravel (in this context, ‘implement’ meaning a hand-axe). This specimen was ‘largely shaped by natural fractures’. The message was that there were no hand-axes in this level (Smith and Dewey 1913, 183, Fig 9.).

*Rutot, the early flake industries from the Belgium, and the Strépyan of Swanscombe*

Nonetheless, there were certain guides that could be followed in the attempt to provide this surprising industry with a place in the industrial sequences of the day. The position of this industry beneath the Chellean levels meant that it could be described as pre-Chellean on stratigraphic grounds, following Commont's scheme and terminology. However, Smith and Dewey also referred to this industry as the 'Strépyan', drawing an analogy to the early Belgian flake industries popularised by Rutot in the late nineteenth and early twentieth centuries (outlined in Table 4.5).

As mentioned above, Rutot's work, like that of Victor Commont, had a great influence on British research, and his industrial terminology was often invoked to describe other assemblages where the classic hand-axe forms were absent. Rutot worked further to the east of the traditional lands where the classic Palaeolithic sequence had been developed, and had recovered flake-rich industries that he believed were Eolithic and early Palaeolithic in date.

<i>Industry</i>	<i>Description</i>
Chellean	Hand-axe dominates, also scrapers
Strépyan	Many coarse scrapers, a few nodules with pointed tips (primitive hand-axes)
Mesvinian	Comprised almost solely of scrapers ( <i>grattoirs</i> and <i>racloirs</i> )
Mafflian	Many scrapers, some hammer-stones
Reutelian	Dominated by hammer-stones, a few scrapers made on broken hammer-stones

Table 4.5: The industrial sequence of Aimé Rutot, as it was seen c. 1911, based on Rutot (1900) and Sollas (1911).

These flake-rich Belgian industries have already made a brief appearance in Chapter Three in connection with the eolith debates, and Sollas described the general flavour of their reception in his remark that Rutot's Reutelian and Mafflian belonged 'to the nebulous region of "eoliths"' (Sollas 1911, 109). However, Rutot's two later industries, the Mesvinian and Strépyan, made a more successful *entrée* into discussion. Sollas rated Rutot's observations at Helin, where this Mesvinian-Strépyan-Chellean sequence had been observed in section (Sollas 1911, 108), in the same class as the contributions of Commont towards elucidating the Early Palaeolithic succession:

‘The order in which the Lower Palaeolithic stages succeed each other in time has been determined [...] most fully by the study of the sections at St. Acheul on the Somme and of that at Helin near Spiennes in Belgium’ (Sollas 1911, 100).

‘The sections at St. Acheul carry us down as far as the Strépyan stage only; to discover the relative position of the Mesvinian we must visit the remarkable section at Helin, near Spiennes’ (Sollas 1911, 107).

Although Commont himself rarely referred to these Belgian industries, a number of British researchers referred to Commont’s ‘pre-Chellean’ (i.e. the industry from the lowest levels at the site of St. Acheul) as the ‘Strépyan’ (Smith 1912, 141; Sollas 1911, 107, quoted above). This terminological equivalence between the pre-Chellean and the Strépyan explains the labelling used by Sollas in Fig. 4.6 above, and also provides an explanation for Smith and Dewey’s comparison of their industry from the Lower Gravel with the Strépyan. They also drew tentative attention to the presence of a diagnostic type-fossil amongst all these nondescript flakes (see Fig. 4.13 below): ‘A few nodules trimmed at the point and squared at the butt [...] which, if accepted as human work, would correspond to the Strépy culture’ (Smith and Dewey 1913, 183). These ‘artefacts’, which were similar to the nodules with pointed tips described by Rutot (Table 4.5), were the only objects that they could possibly describe as implements, even though they were unprepossessing specimens (Dewey and Smith 1914, 93).

Rutot’s Strépyan (originally the Mesvino-chellean) had been discovered in the early 1900s, when it took over the burdensome position of ‘precursor to the Chellean’ which been assigned originally to the earlier Mesvinian (Rutot 1900, 728). The Strépyan was rich in flakes and scrapers, but also included crudely chipped nodules that Rutot believed were prototypes of the Chellean hand-axe (Rutot 1903, 434; MacCurdy 1905, 452). For both Rutot, and for Smith and Dewey, these crude hand-axe-like Strépy nodules provided a closer match to their expectations of a Chellean precursor than the more enigmatic flakes which dominate both assemblages. Two decades later it was still held that ‘the cylindrical nodules trimmed to a point belonging to the Strépy stage are more suggestive of a first link in the chain of evolution of implements than are the eoliths’ (Woodward 1922, 67). However, the

nodules from Barnfield Pit, like the eoliths, would later prove to be naturally flaked (McNabb 1996, 35).



Fig. 4.13: 'Nodule chipped at point, perhaps of Strépy type' (Smith and Dewey 1913, 183, Fig 10).

#### *4.2.4: In summary*

British research and the British sequence was part of a wider European tradition of discovery and expectation, where research was carried out within a tradition of classification based on the concept of progression and distinctive type-fossils, and where a few prestigious regional sequences dominated interpretation. Smith and Dewey had worked within these restrictions in their interpretation of the Swanscombe sequence, and such constraints became particularly evident in their approach to the flake-dominated industry from the Lower Gravel of Barnfield pit. They were soon satisfied that their industry slotted into place at the base of the accepted Early Palaeolithic sequence, and had even identified a possibly diagnostic implement analogous to those from the Belgian sequence.

However, other researchers working within the same restrictions had identified some even more surprising flake-rich industries, which will be examined in the next

section of this chapter. Like Smith and Dewey's pre-Chellean (Strépyan), hand-axes were again rare or absent, but these industries also appeared to date to the time traditionally allotted to the hand-axe dominated Chellean and Acheulian cultures. This chronological position had important implications for old perceptions of the Palaeolithic and for the neat linear pre-Chellean – Chellean – Acheulian – Mousterian sequence that had been refined by Victor Commont.

### **4.3: Early Palaeolithic flake-dominated industries and the breakdown of the classic Chellean-Acheulian-Mousterian sequence**

Around the time when Smith and Dewey were busy in the Thames Valley, other researchers were faced with the problem of how best to incorporate non-hand-axe industries, which seemed to belong to neither the pre-Chellean industry at one end of the scale nor the Mousterian at the other, within the traditional industrial sequence of three, successive industrial blocks: the Chellean (characterised by crude hand-axes and flakes, now preceded by the pre-Chellean), the Acheulian (with finer hand-axes and flakes), and the Mousterian (dominated by flakes, a time when the hand-axes finally disappeared). Such anomalies had also been recognised in the previous century and had been used as ammunition against de Mortillet's distinct industrial epochs (see Reinach 1899, 88, and the arguments of D'Acy in Chapter Two, section 2.2.4).

The confusions of the 1910s were magnified by the adaptation of the *same* traditional terminology to describe *different* interpretations of industrial overlap. The terms 'Chellean', 'Acheulian' and 'Mousterian' were horribly flexible, and could imply a period of time or an industrial culture – or both – and this confusion had been both moulded and masked over the decades by the use of the archaeological type-fossil as both a chronological and a cultural indicator. The identification and interpretation of such 'anomalous' industries, and the attempts to twist the old framework into a new and more elaborate form, provide the focus for this section of the chapter.

#### ***4.3.1: Greater industrial diversity within the Chellean-Acheulian-Mousterian sequence***

Researchers like Comont had managed to add greater variety to the classic Chellean-Acheulian-Mousterian sequence. However, the limitations of the traditional industrial framework come across more clearly in the work of another important researcher, Hugo Obermaier, who was also trying to use the old terminology to describe a more varied pattern of Early Palaeolithic industries. In Obermaier's scheme, hand-axes could come and go within a stretch of time previously occupied solely by hand-axe dominated Chellean and Acheulian industries. However, this

scheme provoked a strong reaction from those who believed in a more simplistic succession of industries. A flexible industrial terminology, which had enabled Obermaier to adapt the old framework in the first place, added further confusion. Obermaier and his detractors all used the same industrial terms, but these had a double meaning, and could refer to both the cultural and the chronological patterning of Palaeolithic industries.

Father Hugo Obermaier (1877-1946), a Bavarian priest who hailed from hand-axe-poor Germany, began his career as a field-assistant to Albrecht Penck, the glaciologist, in the early 1900s (Osborn 1916, 444), and soon came to join Boule, Breuil and Cartailhac as one of the prestigious figures of French prehistoric research. Cartailhac himself spoke glowingly of Obermaier in a letter to Sturge:

‘He is marvellously familiar with Austria and Germany; he has studied in the field with Penck. In brief, I have full faith in his science and in his clear-seeing spirit. We have had very instructive discussions together. He is a good sort’ (Cartailhac to Sturge, probably 1904: BM(F): Archives Box 4).

By 1908, Obermaier had spent six years comparing the French Quaternary with that of Central Europe (Déchelette 1908b, 461), and so had experience of a Palaeolithic record that provided an interesting contrast to the French sites. Obermaier, like Commont, accepted the broad subdivisions of the Early Palaeolithic that had been set out by de Mortillet (Obermaier 1908, 44-45; Reinach 1908, 305; Boule in Déchelette 1908b, 461, footnote 1). However, his version of the French Early Palaeolithic sequence, outlined in 1906 and elaborated in his 1908 paper, *Die Steingeräte des französischen Altpaläolithikums*, described an alternation between industries with, and those without, hand-axes (Obermaier 1906b; 1908). Within the Chellean and Acheulian outlined by Obermaier (summarised in Table 4.6 below) hand-axes could come and go. This scheme would provoke some revealing responses.

<i>Industry</i>	<i>Description / site</i>
Younger Acheulian	Classic level of La Micoque and the Levallois industry with hand-axes
Older Acheulian	No hand-axes (base-level of La Micoque and Le Moustier)
Developed Chellean ( <i>Hochchelléen</i> )	Primitive hand-axes
Early Chellean ( <i>Frühchelléen</i> )	No hand-axes (includes Commont's 'pre-Chellean: Obermaier 1908, 55; Déchelette 1908a, 162)

Table 4.6: Obermaier's classification of French Early Palaeolithic industries preceding the Mousterian (based on Obermaier 1908, 125).

For a while, the classic sequence of three successive and discrete industries (a Chellean, Acheulian, and a Mousterian) was defended against such suggestions of greater industrial diversity. A number of researchers had linked these industries to different races, each of which were thought to have brought their respective cultures to Europe in a series of successive migrations (Dawkins 1880a, 232-233; Allen Brown 1893, 95). This link between race and culture, and the concept of successive migrations, not only provided explanations for discontinuity between successive industries, but could also be used to explain industrial overlap. The earlier races that migrated into Europe would be succeeded by later races, but might well carry on producing their (now degenerate) industries, either within small surviving pockets within Europe, or in their original homelands. This argument neatly retained the concept of a progressive succession of industrial stages, but permitted some industrial variety. It was taken up avidly by William Sollas in the earlier twentieth century, who argued that 'after a sufficient interval of time' all Palaeolithic industries would exist simultaneously across the world (Sollas 1911 vii, 120). Sollas (drawing on the ideas of Lane Fox, outlined in Chapter Two) saw the Australian aborigines as the survivors of Mousterian times, and described how these 'Mousterians of the Antipodes' were contemporaneous with present-day Western civilisations (Sollas 1911, 162, 170).

However, the existence of contemporaneous races presented certain difficulties for the use of type-fossils as time-markers. Sollas explained how the most recent artefact had to be selected from this variety to give an indication of date:

‘The duration of each of the several epochs may be defined on the one hand by its first appearance, and on the other by the first appearance of that next succeeding it. Thus with the advent of the Acheulean in any locality, the Chellean epoch may be regarded as closed; nevertheless the Chellean industry may have continued to exist elsewhere, a fact which may be expressed by the statement that the Chellean industry survived into Acheulean or even later times. Thus the industries overlap the epochs’ (Sollas 1911, 120).

However, few researchers made their use of terminology this clear, and the twin cultural and chronological aspects of industrial terminology would lead to argument over Obermaier’s new version of a more varied industrial sequence. For Sollas, the hand-axe heralded the arrival of the Chellean industry – the chronological and cultural aspects were firmly connected until the arrival of the next industry. This led him to disagree with Obermaier’s use of the term ‘Early Chellean’ (‘fruh-Chellean’ below) to describe industries that *lacked* hand-axes:

‘Dr. Obermaier [...] assigns the Mesvinian to an horizon immediately below the Chellean and speaks of it as “fruh-Chellean”. In the light of our present knowledge this nomenclature can scarcely be maintained; the Strepyan intervenes between the Chellean and Mesvinian, the Mesvinian implements are ruder than the Chellean, and the characteristic boucher is lacking’ (Sollas 1911, 111).

The attempts to adapt the old framework were assisted by a flexible terminology that could withstand manipulation of chronology and culture. Conversely, such flexibility also caused much confusion, as the same terminology could mean very different things to different researchers. Sollas could not accept an industry lacking hand-axes as even a typological variety or *facies* of the Chellean; it had to have a distinct, different name and be placed in the correct position in the industrial sequence: either before the Chellean or after the Mousterian. Other researchers, like Obermaier, were using the term ‘Chellean’ in a looser sense in an attempt to squeeze more industrial variety into these three major Palaeolithic industries. Obermaier used the term ‘Chellean’ to refer to a temporal phase which encompassed two different (though successive) cultures, only the latter of which was characterised by hand-axes (see Table 4.6). This difference of opinion between Sollas and Obermaier also illustrates the variety of ways to define the beginning of an epoch: Sollas began the Mousterian epoch with the first Levallois flake, which he saw as a technological advance (Sollas 1911, 124, 130-132). However, Obermaier included the

Levalloisian within the Acheulian since it first appeared in association with Acheulian hand-axes (Smith 1911, 530).

The recognition of contemporaneous industrial variation was not a new concept, and the idea that different groups living at the same time might make different kinds of tools formed part of the ongoing search for a reliable industrial succession. But moving away from these arguments over concepts and terminology and returning to the archaeological record, it was only once classic sequences of these successive Chellean, Acheulian, and Mousterian industries had been established, elaborated, and applied to other areas that major anomalies could even be identified as such. Although Commont and Obermaier had attempted to develop and elaborate the old sequence in the early twentieth century, the expanding numbers of anomalous industries were stretching the old framework to its limits.

For example, Commont observed that the sequence from Central Europe was very different to that of North France, and lacked the typologically valuable hand-axes (Commont 1909, 38). Commont also noted a puzzling industry of Mousterian appearance in the Somme Valley associated with a warm fauna (the 'warm Mousterian') which seemed to intervene *between* his two Acheulian industries (Commont 1909, 38-39; 1912b, 245; 1912c, 299-300). Kendall recorded a similarly anomalous industry from British deposits at Knowle Farm Pit (Kendall 1915, 138). Obermaier pointed out that the upper Acheulian and the Mousterian were contemporaneous between northern France and the caves of southern Europe (Obermaier 1906b, 306; Déchelette 1908a, 84-85).

A number of solutions were put forward to describe such variety, which led to the creation of new classificatory distinctions and a re-thinking of the use of type-fossils as chronological indicators. The development of such frameworks reached a peak of activity in the 1930s, and this will be examined in Chapter Five. However, these constructions of the future were inspired by ideas developed in these earlier decades, and by the deconstruction of the old linear sequence which was about to come to a head.

#### *4.3.2: Re-dating the Strépyan and Mesvinian of Rutot*

Returning once again to Rutot's Belgian industries, these were about to undergo a process of re-dating which would drag them up from their pre-Chellean position at one end of a single line of Palaeolithic industries to a position that corresponded in time to the period when Acheulian hand-axes were being made in France. The re-dated Belgian industries would join an expanding heap of anomalous industries that threatened to burst the old framework apart, and contributed to attempts to understand the temporal and geographical position of Early Palaeolithic flake-industries. Since they had attracted so much attention in the earlier years of their career, this process of re-dating exposed many of the uncertainties and confusions outlined above: the problems of culture and chronology; type-sequences and type-fossils; progress and contemporaneous variety.

When Smith and Dewey drew their analogy with the Strépyan, they assumed that Rutot's industries pre-dated the Chellean. However, in line with some of the other cases remarked above, some suspected that Rutot's industries had been wrongly dated and were in fact more recent than the Chellean. Warren noticed that Rutot seemed to be using a scale for his industries that differed from the usual industrial terminology – for example, his 'Chellean' was the 'Acheulian' of most authors – which meant that his Mesvinian was no earlier than accepted Early Palaeolithic industries of Britain (Warren 1905, 341, footnote).

Commont also added his quiet voice to the doubters, informing Sollas, in confidence, that Rutot would do well to leave his imagination aside, adding that he had long considered Rutot's system with its supposedly ancient Strépyan and Mesvinian to be flawed but had not come out with this earlier as he liked Rutot very much and hoped to find some points of agreement with him (Commont to Sollas, 23<sup>rd</sup> January 1913: BGS: GSM1/445; see also Commont 1912a, 168-170). It was stratigraphy and palaeontology, not typology, that brought about this change: Commont argued that the deposits which Rutot had identified as older loess were in fact younger loess, and this made the series of associated industries far more recent (Commont 1912a, 169-170; Sollas 1915, 134).

*'Quant au Strépyan il n'existe pas à mon avis. Sans doute il y a une industrie plus vieille que le chelleen typique français à Chelles, mais cette industrie préchelléenne ne peut correspondre au niveau qualifié de Strépyan par M. Rutot. Son Strépyan est imaginaire, il serait d'ailleurs post Würmian, des alluvions des basses terrasses belges était Würmiennes'* (Commont to Sollas, 1<sup>st</sup> January 1913: BGS: GSM1/445).

'As for the Strépyan, it does not exist in my opinion. There certainly is an industry older than the typical French Chellean at Chelles, but that pre-Chellean industry cannot correspond to the level designated as Strépyan by M. Rutot. His Strépyan is imaginary and as a matter of fact it would be post-Würmian [the Würm was regarded as the last glacial episode, widely associated with the Mousterian], the deposits of the Belgian low terraces being Würmian.'

Sollas now had to change the proud account in his first edition of *Ancient Hunters* of 'the remarkable section at Helin, [...] so well described by M. Rutot' (Sollas 1911, 107), to a 'discordant note' that 'requires reinvestigation' in the next edition (Sollas 1915, 132), where Commont's opinion on this matter took a prominent place. The Mesvinian was still occasionally referred to as a pre-Chellean industry (Osborn 1916, 128), but it was now gradually moving towards a more Mousterian time-frame (Sollas 1915, 136; 1920, 266). As we shall see below, the industry from Clacton-on-Sea experienced a similar shift from a pre-Chellean to a pre-Mousterian position. Smith and Dewey's term 'Strépyan', incidentally, still continued in use regardless, long after the Strépyan of the type-site had been re-dated (Kennard 1916, 253; Woodward 1922, 67; Marston 1937, 340).

We have already observed the gulf separating the vision of Sollas from that held by Obermaier. Before 1915, Sollas's main interest in the Mesvinian has been based on its great age; for him, the re-dating of Rutot's industries meant that they 'have thus lost their chief claim upon our interest' (Sollas 1915, 136). But views more similar to those held by Obermaier were becoming popular, and, by 1924, the Mesvinian was back in the limelight. When the term 'Mesvinian' was applied to the industry from Clacton-on-Sea in the early 1920s, which was very similar to the industry of the Lower Gravel of Barnfield pit, this term would invoke a different set of expectations from the preceding era when Sollas could remark that 'Each stage is transitional to the next, and there is a gradual passage from the Strepyan to the summit of the Acheulean' (Sollas 1911, 124).

### *4.3.3: The Mesvinian industry of Clacton-on-Sea*

Despite Smith's neat match between the Swanscombe sequence and Commont's pre-Chellean – Chellean – Acheulian – Mousterian scheme, there was evidently much variety that this pattern could not encompass. Certain anomalous industries had been identified which seemed to indicate that a flake-rich Mousterian industry might have existed in the pocket of time previously occupied by the hand-axe dominated Acheulian. Those described by Commont and Obermaier have been briefly noted earlier in the chapter, and British workers had made similar observations (Kendall 1915, 138; Kennard 1916, 256; Bury 1916, 176-177; Moir 1918c, 508). As mentioned in Chapter Three, Moir had even described two parallel lines of evolution emerging from the Kent eoliths, one line moving towards the Acheulian via the rostro-carinate; the other evolving into the Mousterian via primitive flake implements from the sub-Crag, Middle Glacial Gravel and Chalky Boulder Clay (Moir 1918c, 512, 514, 518), a view later supported by Smith (Smith 1934, 168-169).

However, such observations were mostly asides, tucked away in a few lines. It took longer for the idea to become more generally accepted that flake-dominated 'industries' no longer needed to be incorporated within more *linear* perceptions of an Early Palaeolithic rich in hand-axe implements – they could exist *alongside* the old line. It would not be until the late 1920s and early 1930s that such concepts became prominent enough to arouse significant changes to the old industrial framework. However, this will be discussed in Chapter Five. For now, it only remains to introduce the changing interpretations of Warren's industry from Clacton-on-Sea through the era explored in this chapter, culminating in the early 1920s when Warren made one of the most explicit early statements about contemporary races of hand-axe and non-hand-axe tool-makers in Britain during Early Palaeolithic times.

#### *Early perceptions of the industry from Clacton-on-Sea: a pre-Chellean industry*

The honour of discovering the industry at Clacton-on-Sea went to the Rev. J.W. Kenworthy and his finds of 1898 (Warren 1932b, 20). Warren began collecting the flint flakes from Clacton-on-Sea around 1908 (Warren 1951, 108), and Smith and Dewey were working at Swanscombe when his first brief account of the *Palaeolithic Remains from Clacton-on-Sea* was being published (Warren 1912a). He characterised

his finds as a flake industry; but unlike Smith and Dewey, Warren had little difficulty in identifying 'characteristic flint implements': trimmed flakes, and 'rude forms of side-choppers', although he emphasised that 'not a single example of the usual ovate or pointed Palaeolithic types has yet been found' (Warren 1912a, 15).

Despite his implements, Warren was also faced with the problem of finding a label for this flake-dominated assemblage. The industry was not accompanied by the mammoth that characterised the cold Mousterian times and Warren observed that it lacked the characteristic Mousterian technique, although it is unclear at this time whether he was referring to the finer retouch of the true Mousterian, or the Levallois technique of the industry commonly referred to as the 'Early Mousterian' (Warren 1912a, 15). Initially Warren, like Smith and Dewey, seems to have favoured a pre-Chellean age for his finds. He thought the closest affinity to the 'implements' came from the 'ruder surface implements of the Chalk Downs of the South of England' (Warren 1912a, 15), by which he seems to have meant Prestwich's Hill group of pre-river-drift palaeoliths (not eoliths) from Kent (Warren 1902, 98; see Chapter Three, Table 3.1).

In retrospect, it is interesting that Prestwich's Hill group had already been linked to the Mesvinian by Hinton and Kennard at a time when Rutot's Mesvinian from Belgium was still regarded as a pre-Chellean industry (Hinton and Kennard 1905, 91, 98-99), another example of the widespread use of Rutot's flake-rich industries to describe Early Palaeolithic non-hand-axe industries. Warren himself, however, made no connection to the Mesvinian industry. Like many, he was cautious about some of Rutot's industries (Warren 1902, 99), although he had accepted that some of the Mesvinian implements might well be humanly worked (Warren 1905, 341, footnote). However, Warren was wary of typology and too cautious to assign any pre-Chellean label to his discovery.

In addition, no analogies seem to have been drawn by either Warren or Smith between the Clacton-on-Sea and the Swanscombe industries. There was apparently little personal communication between Warren and Smith. In the British Museum, Smith kept firmly to a small circle of 'approved friends'; his assistant, Kendrick, remembered how in the 1920s Smith disapproved of any staff who were friendly

towards those 'with contrary views to ours. I think immediately of Hazzledine Warren and my visits to his anti-eolith collection at Loughton. And there were friends I made who had displeased Smith in other ways' (Kendrick 1971, 4-5).

*The pre-Mousterian industry from Clacton-on-Sea and parallel industrial cultures*

Some time shortly before 1922, Henri Breuil had his first sight of the Clacton-on-Sea industry. His first impression was that it was close to the Mesvinian of Belgium (Breuil 1930, 221), and it was he who gave the Clacton industry its first name: the 'Mesvinian'.

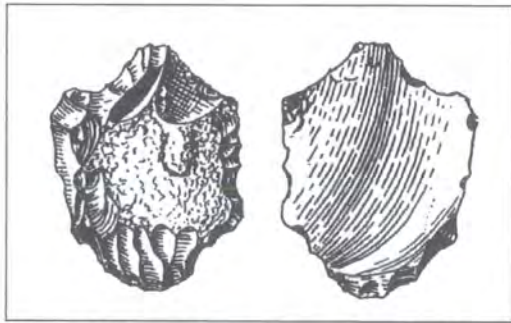


Fig. 4.14: Mesvinian 'grattoir' (Rutot 1900, 727, Fig. 12).



Fig. 4.15: Mesvinian 'pointe-racloir' (Rutot 1900, 727, Fig. 14).



Fig. 4.16: Flake collected by Warren from Clacton-on-Sea (PRM 1940.3.26).

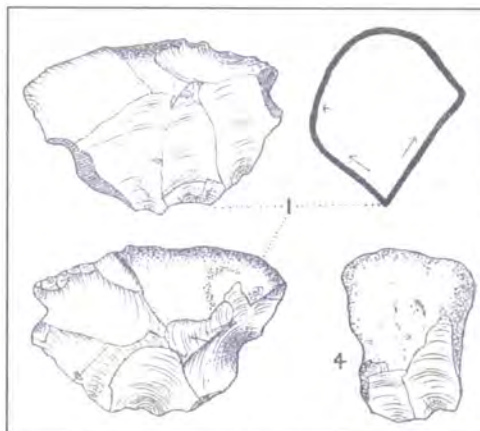


Fig. 4.17: One of the side-chopper group of Mesvinian implements from Clacton-on-Sea (Fig. 1) and a small flint nodule with an axe edge at one end (Fig. 4) (Warren 1922a, 600).

Rutot's earlier description of the Mesvinian certainly resembled Warren's discoveries. Rutot described his Mesvinian industry as dominated by end-scrapers and side-scrapers (see Figs. 4.14 and 4.15), as well as flakes with concave notches, and natural or waste flakes, used as multi-purpose tools (Rutot 1900, 714). Warren's industry from Clacton-on-Sea was dominated by crude flakes (Fig. 4.16); it also contained side-choppers (Fig. 4.17), which Warren insisted were true implements rather than cores; and some scrapers (Warren 1922a, 597-602), so it fitted Rutot's Mesvinian rather well.

Warren accepted Breuil's designation of his industry as Mesvinian, describing this view as 'obviously correct, and it has since been confirmed by several continental authorities who are intimately acquainted with the Mesvinian of Belgium' (Warren 1922a, 597). Warren had an industry that was contemporary with the Chellean or Acheulian. Indeed, he suggested that the crude pointed implements might 'perhaps be considered as unsuccessful attempts to copy the Chellian implement' (Warren 1922a, 598). However, the trimmed flakes 'appear to be the immediate precursors of the Mousterian types' (Warren 1922a, 599).

Palaeontologically this industry was also pre-Mousterian; stratigraphically it was of the Middle (50 ft.) Terrace group (which ranged from Late Chellean to Mousterian); and, taken together, this suggested a date contemporaneous with 'some part of the Acheulian stage, or possibly a little earlier. This is in agreement with the present continental dating of the Mesvinian industry' (Warren 1922a, 597, 602; see also Rutot 1921, 55). The crude nature of the industry was misleading – this was not pre-Chellean after all, but pre-Mousterian, and this led Warren to make the following tentative suggestion:

'On taking a general survey of the Mesvinian industry, as so admirably displayed at Clacton, one cannot help feeling that it might well be the precursor of the Mousterian industry, but that it has no cultural connection with the Chellian and Acheulian stages.

As knowledge of the Palaeolithic period increases, we are realizing more fully the divergence of races and cultures which were living contemporaneously together. I believe it is in this light that the Mesvinian industry is to be understood. But we have yet so much to learn that no conclusions can be regarded as more than tentative' (Warren 1922a, 602).

As we have seen, Hugo Obermaier had already published a paper in which he built on his earlier case for alternating hand-axe and non-hand-axe cultures (Obermaier 1908) and outlined different geographical provinces for groups of hand-axe-makers and for contemporary groups who did not make hand-axes (Obermaier 1919; see Fig. 4.18). Obermaier saw the 'Chellean' as a Western European / Mediterranean culture, parallel to the 'Pre-mousterian' (lacking in hand-axes) of central and Eastern Europe (Obermaier 1919, 146-147).

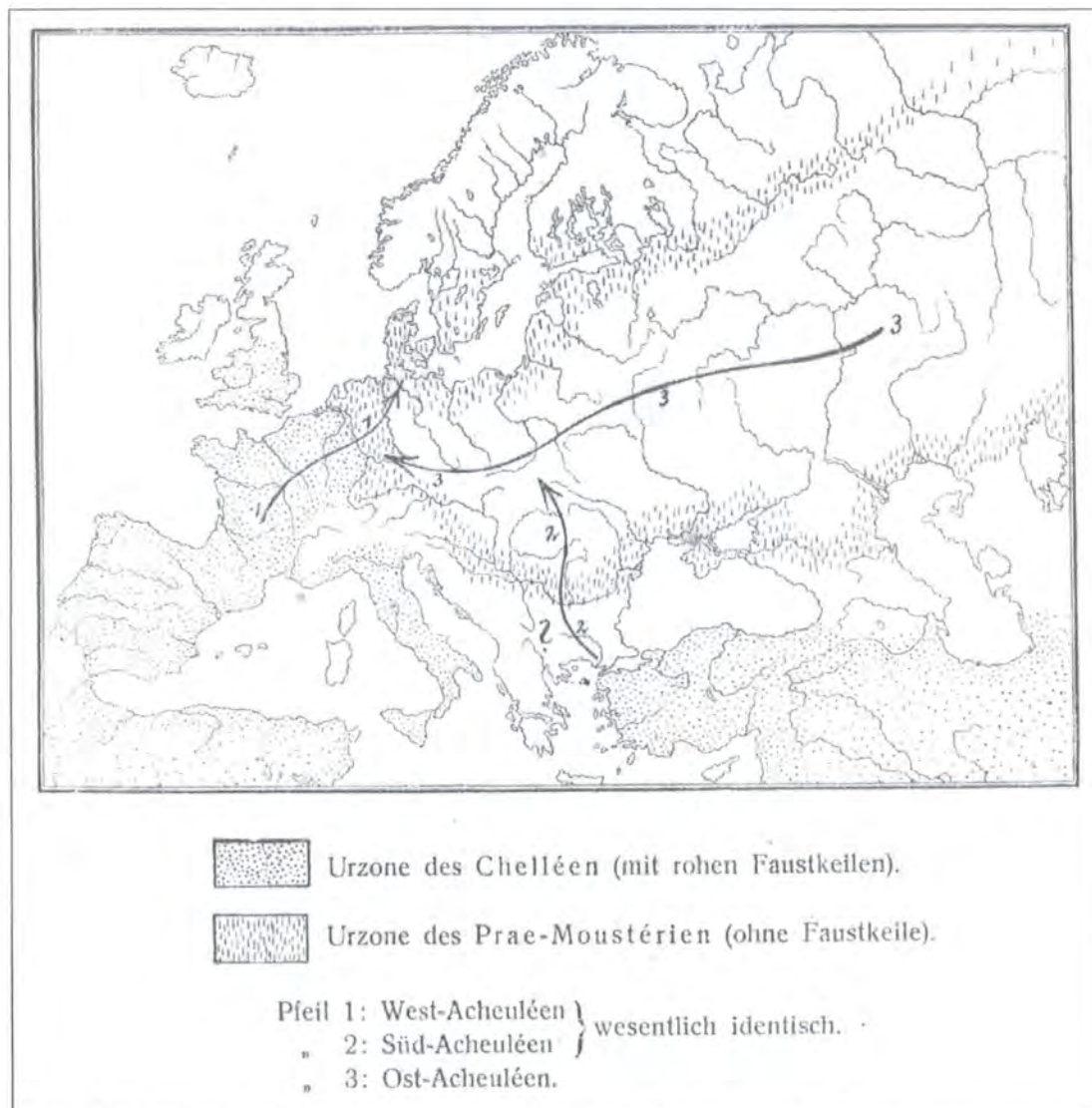


Fig. 4.18: 'Urzonen des europäischen Chelléen und Prämoustérien samt den Verbreitungsbahnen des Acheuléen', 'Zones of the European Chellean and Pre-Mousterian including the spreading routes of the Acheulian' (Obermaier 1919, 147, Fig. 1). Chellean Zone (with crude hand-axes), Pre-Mousterian zone (no hand-axes). 1. West Acheulian, 2. South Acheulian (substantially identical), 3. East Acheulian.

In 1923, Warren gave stronger voice to his opinion of a succession of flake-rich, non-hand-axe, industries. His Mesvinian developed into the proto-Mousterian tortoise-core (Levallois) industry, which in turn developed into the Mousterian. Warren placed all three flake industries on a separate line to the Chellean and Acheulian hand-axe industries, and suggested that the two industrial lines were associated with two different racial streams (Warren 1923d, 614).

However, the cases made by researchers like Warren, from his detailed perspective of a regionally specific sequence, and Obermaier, from his wider outlook and knowledge of the Central European sequence, seem to have had remarkably little effect on the general perception of the British Early Palaeolithic in the early 1920s. This was probably because the expectations of other researchers were still firmly tied to more traditional views based on a general West European sequence. No framework had yet been formulated to enable easy discussion of these new views (Garrod 1928, 261). Sollas, for example, who was present when Warren read his 1923 paper, *The Elephas antiquus Bed of Clacton-on-Sea (Essex) and its Flora and Fauna* (Warren 1923d), referred to the 'anomalous position' of the Clactonian industry beneath a late Acheulian horizon (Sollas, cited in Warren 1923d, 634), and W. Johnson remarked:

'With respect to the Mesvinian implements, they seemed to represent types which are found in several periods, and unless associated as a group, they would require further investigation before the date assigned could be accepted' (Johnson, cited in Anon 1923, 56).

In the margin of a copy of this paper held in the Institute of Archaeology, Warren has underlined 'unless associated as a group', and written in the margin 'certainly they are so associated at Clacton – that is the point' (Warren, marginalia, undated: IAL: DAA 410.E/7 WAR).<sup>27</sup> This was a time of transition, and Warren's conception of how to order the past was different to that held by Johnson, who seems to have found it difficult to reconcile the Clactonian elements with his idea of unilinear Palaeolithic classification, and therefore assumed that this assemblage comprised a mix of elements from a number of different ages. Warren, on the other hand, was convinced that this was not a fortuitous mixture of types from several periods; it was a discrete industry, and deserved to be treated as an entire assemblage and not pigeonholed into slots. Flakes might be present,

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<sup>27</sup> Many thanks to John McNabb for indicating this source.

but flakes did not always indicate a late period, and these were not Mousterian flakes. Bifacial forms reminiscent of hand-axes were also present, but Warren believed (and later insisted emphatically) that these were *not* hand-axes and did not justify the glib label 'Acheulian'.

#### 4.4: Conclusions

During the early twentieth century, an array of individuals tackled the problem of how to order and interpret the Palaeolithic record, whilst working under a variety of social, institutional and intellectual restrictions. Some of these researchers would have witnessed great changes in the perception of the Early Palaeolithic since the work of Smith and Dewey at Swanscombe in the early 1910s. They might have observed that the three great industries – Chellean, Acheulian and Mousterian – no longer succeeded each other in a single line, however minutely subdivided. The hand-axe industries still progressed upwards through time through the various subdivisions of the Chellean and Acheulian, but new ancestors had now been described for the Mousterian. The anomalous industries of the previous decade that seemed to overlap with, or precede the Acheulian had been gathered together in a line, ranging from the pre-Mousterian or Mesvinian to the Levalloisian to the Mousterian. These flake industries were made by a separate race or races, and allotted their own zone of Europe. Attempts at a more detailed reconstruction of this parallel line of flake industries would soon begin in earnest.

The expectations and the questions asked of the record had changed for workers like Obermaier and Warren. Instead of one linear stem, the new ideas depicted two great trees, their roots uncertain and their top branches mingling. Although the old assumption of progression would continue to characterise future work on these flake industries, attempts to identify distinctive retouched core-tool implements within ancient flake-dominated assemblages would become less common. The importance of type-implements in the minds of discoverers had once delayed the acceptance of non-hand-axe Early Palaeolithic assemblages, when researchers like Smith and Dewey, and Rutot tried to find the implements that they expected to characterise a precursor to the hand-axe dominated Chellean. But though typological approaches were still of great importance to interpretations and classifications of industries on the hand-axe line, approaches to the flake-rich industries would now tend to centre upon *technique* rather than typology. The old conceptual distinction between core-tools and flake-tools was destined to become a major structuring principle of research, and soon even the mention of implements in an early flake industry became rather embarrassing, as Warren would discover as he continued to defend the existence of side-chopper implements (which he considered to be neither cores nor true hand-axes) within his Clacton-on-Sea industry in the 1930s.

However, the majority of researchers did not take up the kinds of ideas articulated by Obermaier and Warren until the late 1920s and early 1930s, for reasons that will be explored in Chapter Five. The lack of curiosity in Smith and Dewey's Strépyan or pre-Chellean industry from Swanscombe and Warren's industry from Clacton during the earlier twentieth century provides a massive contrast to later interest on the part of researchers such as Henri Breuil, and it is no coincidence that such concern arose at a time when the concept of parallel industrial cultures was flourishing. However, these later and more famous reconstructions owed much to the confusing period of research described above, when old expectations were starting to be challenged.

## CHAPTER 5

### **Developing an Early Palaeolithic industrial succession for Britain**

*c. 1920 – c. 1950*

‘I still feel that Pleistocene chronology is the clock by which archaeological, or rather Palaeolithic, time is to be measured, and it makes nonsense of time-counting to use as clocks the very objects it is desired to time’ (Zeuner 1959, xiii).

In 1937, Harper Kelley, pupil and friend of the Abbé Henri Breuil (Breuil and Koslowski 1932, 42), observed:

‘During the last few years the importance of the flake cultures of the Lower Palaeolithic has been recognised by prehistorians, and great advances have been made towards the correlation of these industries with various biface cultures, and in the working out of their stratigraphical sequence.

The position of the Clactonian has been firmly established, and the long neglected Levalloisian industry is now assuming its proper place of importance’ (Kelley 1937, 15).

Despite the work of Commont, Obermaier and Warren outlined in Chapter Four, it was not until the early 1930s that the old progressive linear classification of de Mortillet was finally replaced by a vision of greater industrial variety, with Europe as host to an increasing number of contemporaneous Early Palaeolithic industries: flake-rich industries were being recognised and distinguished from the Acheulian and Chellean hand-axe making populations (Burkitt 1933a, 123; Childe 1935, 4; Oakley and Leakey 1937, 240). There has been a general agreement that it was Breuil who managed to give such ideas more general currency in British Early Palaeolithic research (Childe 1935, 4-5; 1951, 234; Garrod 1938a, 2; Dennell 1990, 553; McNabb 1996, 36; Cohen 1999, 306). Indeed, his Early Palaeolithic scheme became far more popular in Britain than it did in France (Sackett 1991, 139, note 2).

What was the reasoning behind Breuil’s scheme? Why was it received so rapturously that the past work of Warren, Obermaier and others was forgotten? How did it come to have this influence on British Palaeolithic research? This chapter will examine the rise and fall of Breuil’s scheme between the 1930s and the 1950s in order to answer these questions. It has been divided into four sections, each using the

Clactonian industry as a touchstone to see how various approaches inspired by Breuil were affecting the perception of Early Palaeolithic industries in Britain.

- The first section (5.1) introduces Henri Breuil and explores the early influences of his general scheme of parallel cultures. The expectations that became associated with the Clactonian industry over the 1930s clarify the reasoning at the basis of Breuil's scheme and indicate the extent of its popularity in Britain.
- The second section (5.2) examines how a more specific aspect of Breuil's research came to influence perspectives of the British Palaeolithic: the detailed scheme of industrial subdivisions that Breuil developed with Koslowski in the early 1930s. The way in which the Clactonian was divided into several different industrial sub-stages by Breuil and by British researchers illustrates the influence such practices had on perceptions of the British Palaeolithic.
- The third section (5.3) explores the broader impact of the Breuil-Koslowski framework on Quaternary correlations over the 1930s, and assesses the use of the Clactonian industrial subdivisions in assisting interpretations of the geological history of the Thames Valley.
- The fourth section (5.4) examines how T.T. Paterson extended Breuil's scheme on both a regional and a global scale over the late 1930s and early 1940s, and analyses the reaction against such excesses by researchers working in Asia, Africa, and Britain. New interpretations of the Clactonian over the late 1940s and 1950s demonstrate how the downfall of Breuil's vision and the new approaches to Palaeolithic research were received in Britain.

1869, 1873, 1881, 1900	De Mortillet develops the French Palaeolithic sequence (Mortillet 1869; 1873; 1881; 1900)
1889	Whitaker (1889) produces the first systematic account of the drift geology of the London region
1900-07	Hinton and Kennard (1900; 1905; 1907) develop a standard relative chronology for the Thames terraces
c. 1902	Obermaier begins his researches
1906-08	Obermaier (1906b; 1908) suggests alternating flake and hand-axe industries in the Early Palaeolithic
1908	Commont (1908) outlines the Early Palaeolithic sequence of the Somme in his <i>Les Industries de L'Ancien Saint-Acheul</i>
1908	Foundation of the Prehistoric Society of East Anglia. Sturge (1908, 9) complains that Britain is not keeping up with Continental research
1901-09	Penck and Brückner (1901-09) publish their scheme of Alpine glacial episodes: <i>Die Alpen im Eiszeitalter</i>
1910	Foundation of the Institute of Human Palaeontology, Paris, by the Prince of Monaco, employing Boule, Breuil and Obermaier
1912	Breuil meets Burkitt in Cambridge, and expresses a poor opinion of British prehistoric research (Burkitt, undated typescript notes c. 1960: ULC: MB, Add 7959, Box 3)
1912-13	Discovery of <i>Eoanthropus</i> at Piltdown, Sussex
1912-14	Collaboration of the Survey and British Museum in the London area results in the excavations at Swanscombe, which establish the British Palaeolithic sequence
1915	Boule (1915, 1-2) denounces the British as backward and insular
1919	Obermaier publishes his interpretation of distinct hand-axe and non-hand-axe groups in Europe (Obermaier 1919)
1920-21	Breuil (1937a, 259; 1948, 66) abandons Boule's short glacial chronology in favour of a longer version, partly stimulated by a visit to the British geological deposits of East Anglia and the Thames Valley, possibly linked to Obermaier's 1919 paper
1922	Warren (1922a) publishes his first major paper on the Clactonian industry
1924	Discovery of <i>Australopithecus</i> in Africa
1926	Breuil (1926) publishes his paper on parallel flake-dominated and hand-axe dominated industries in Middle Palaeolithic cultures
1928	Garrod (1928) gives the Presidential Address to the Prehistoric Society, expounding on parallel cultures
1929	Breuil (1929) publishes <i>La Préhistoire</i> , a general account of the Early Palaeolithic
1930, 1931	Peyrony (1930; 1931) publishes two papers on contemporaneous Mousterian industries, parallel to Acheulian industries
1931, 1932, 1934	Breuil and Koslowski (1931; 1932; 1934) publish their detailed Palaeolithic classification: <i>Études de Stratigraphie Paléolithique dans le Nord de la France, la Belgique et l'Angleterre</i>
1932	Breuil (1932a) publishes his paper on parallel Lower Palaeolithic hand-axe and hand-axe and flake cultures
1936	King and Oakley (1936) publish <i>The Pleistocene Succession in the Lower part of the Thames Valley</i> , heavily influenced by Breuil's industrial subdivisions
Mid 1940s	Oakley's reassessment of the Baker's Hole industry leads to doubts over the reliability of Breuil's industrial subdivisions as zone fossils (Oakley in Zeuner 1944; Oakley and King 1945; Oakley <i>et al.</i> 1948a, 1948b)
1945	Lowe and Breuil (1945, 50-51) note that the Levalloisian of South Africa follows a different trajectory to the European, and is not on a parallel stream to the Acheulian
1948	Movius (1948) attacks the parallel hand-axe (core) vs. flake dichotomy
Late 1940s	Childe (1944), Garrod (1946), Goodwin (1946), Movius (1948), and McBurney (1950) criticise the old obsession with classification and advocate a more anthropological and ecological approach to the past
1950s	Oakley (1952), McBurney (in West and McBurney 1954), and Bordes (1956) attack Breuil's industrial subdivisions

Table 5.1: Time chart: key events in Early Palaeolithic research from the late-nineteenth century to the mid-twentieth century.

## **5.1: The rise of Henri Breuil, his scheme of parallel cultures, and his influence on British Early Palaeolithic research**

As was suggested in Chapter Four, one reason why the conclusions reached by researchers like Obermaier and Warren did not seize the attention of British researchers sooner was that no satisfactory framework yet existed for the discussion of such new ideas. Researchers tended to accommodate these industries within their perceptions based on the old linear framework, and different researchers therefore perceived the same industries very differently – usually as anomalies, or cases of intermixture. This recalls the conclusions of Chapter Three about the differences in perception that characterised arguments over the eoliths.

Nonetheless, as dissatisfaction with the old linear scheme increased, so did discussion of such industries. Several more papers were produced in the late 1920s that further promoted the idea of several contemporary Early Palaeolithic industrial cultures, and these were associated with the rise of interest in industrial sequences beyond western Europe (Childe 1935, 1; Clark 1959, 10-11). Dorothy Garrod saw this as a time of reassessment, an end to the heroic period of classification (Garrod 1928, 260). When glancing back in retrospect, future researchers would cite Breuil's 1926 note on Levallois industries and Garrod's 1928 Presidential Address to the Prehistoric Society of East Anglia as particularly important works that brought about a shift in perspective (Childe 1935, 4; 1944, 18; Clark 1941, 147).

### ***5.1.1: Henri Breuil and his approach to the British Early Palaeolithic in the 1920s***

The Abbé Henri Prosper Edouard Breuil, pictured in Fig. 5.1, was later described by Miles Burkitt as a man of 'an electric, not to say impatient, temperament', who 'smoked without ceasing and would only discuss prehistory' (Burkitt, transcript of radio broadcast, March 5<sup>th</sup> 1962: ULC: MB, Add 7959, Box 3). Breuil had been introduced to the Early Palaeolithic of the Somme Valley river-drifts by a distant relative, Geoffroy d'Ault du Mesnil (1842-1920), in the late nineteenth century (Breuil 1921b, 162; 1948, 65), and many of his autumns were spent in this palaeolithic playground under the tutelage of Commont (Breuil 1937b, 61). However, for the first two decades of the twentieth century Breuil's interest was centred upon

the Upper Palaeolithic industries of the caves (Breuil 1937b, 54). Some joked that being '*jeune et mince*', he was well-suited to cave-work (Kenyon 1937, 255).

By the early 1920s, however, Breuil had taken up Commont's mantle and was studying the Somme Valley deposits in detail (Breuil 1939, 33; Garrod 1961, 206). We have seen the impact that this sequence had on Smith and Dewey's interpretations of the 1910s. The Somme Valley sequence would also feature heavily in Breuil's theories of Early Palaeolithic industrial patterning, and his use of this particular data-source must have contributed to the great influence he had on British Early Palaeolithic research in the 1930s and early 1940s.



Fig. 5.1: The Abbé Henri Breuil (1877-1961), undated photograph (ULC: MB, Add.7959, Box 1). Reproduced courtesy of Cambridge University Library.

Breuil gave his first clear account of contemporary flake-dominated and hand-axe-dominated industries in a short note that appeared in *Man* in 1926: *Palaeolithic Industries from the beginning of the Rissian to the beginning of the Wurmian Glaciation*. Breuil, like many other researchers of the early twentieth century, used a glacial terminology originally developed by Penck and Brückner (1901-09) for a four-

fold scheme of Alpine glaciations (see Chapter Two) that require a brief explanation. The terms Günz, Mindel, Riss and Würm were used for the first, second, third and fourth glaciations respectively, and these glacial labels were linked together to describe the intervening interglacials: thus the third or 'Riss-Würm' interglacial fell between the Riss and the Würm glacial episodes.

Breuil's note of 1926 dealt mainly with the industries from the Somme and Thames Valleys, previously grouped together under the term 'Mousterian', but by then more generally split into an earlier Levalloisian and a later Mousterian, or 'cave-Mousterian' (Warren 1912b, 205; Smith in Higgins 1914, 7; Kennard 1916, 256; Moir 1918c, 508). Breuil based much of his argument on the fact that the Levallois group of industries was not contemporaneous with the Mousterian of the caves but was earlier, belonged to the last interglacial and was associated with the Upper Acheulian (Breuil 1926, 176-177).

This overlap in date between the two industries had been noted in the past, as discussed in Chapter Four. At the start of his paper, Breuil referred to Commont's identification of an anomalously early Mousterian industry in the Somme associated with warm fauna (now Breuil's Levalloisian: Breuil and Kosłowski 1932, 45) that appeared to be contemporary with Upper Acheulian industries (Commont 1909, 38-39; 1912b, 245; 1912c 299-300). Breuil's good friend Obermaier also included the Levalloisian within the Acheulian since this industry first appeared in association with Acheulian hand-axes (Smith 1911, 530). However, Breuil added another level of detail concerning the races of tool-makers who may have contributed to these two industries.

Breuil outlined a picture of contemporary Levalloisian and Micoquian (evolved Acheulian) populations coming and going within one geographical territory and leaving their distinctive industries behind them, pushed around by the changing Quaternary climate between the Riss and the Würm glaciations (Breuil 1926, 178). The same concepts would form the basis of his later, more detailed schemes. Dorothy Garrod took up Breuil's ideas in her 1928 Presidential Address to the Prehistoric Society. Garrod had studied under Breuil at the *Institut de Paléontologie Humaine* in Paris for two years from 1922 (Garrod 1961, 206; Davies 1999, 266) and now, like

Breuil, described three converging lines between the time of the Acheulian and that of the Mousterian (Fig. 5.2): the Chellean-Acheulian line, the Levalloisian, and the Pre-Mousterian (Garrod 1928, 267).

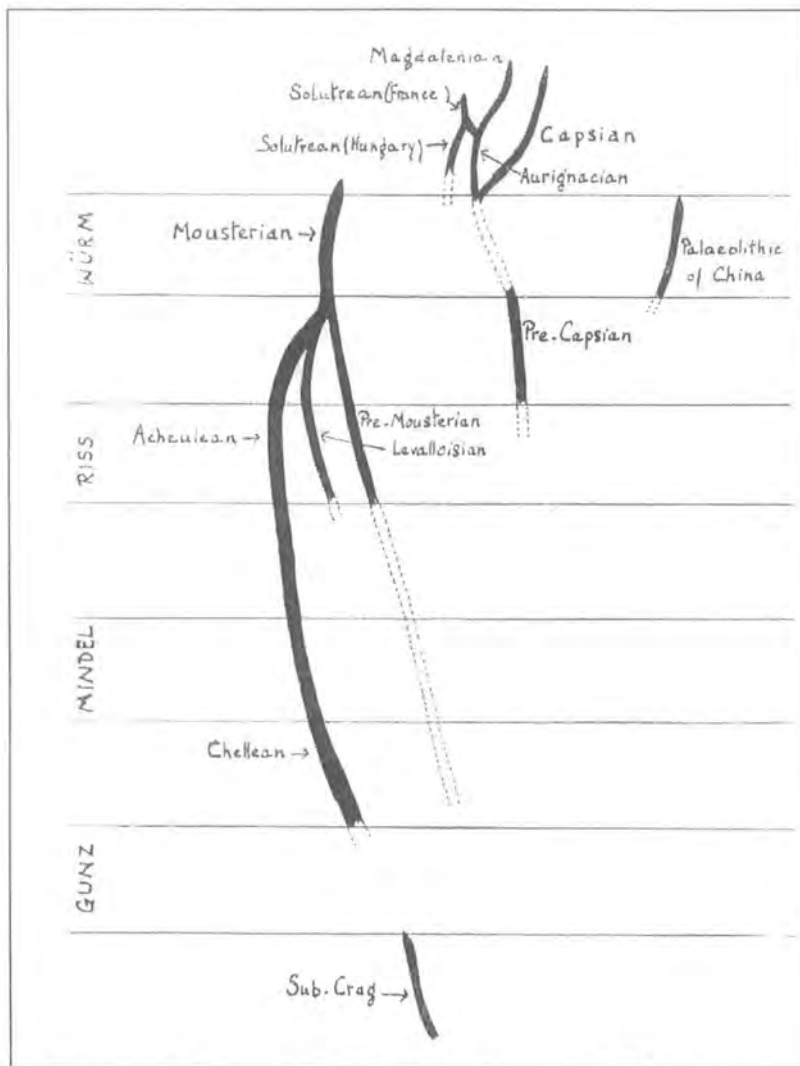


Fig. 5.2: Interrelation of Palaeolithic cultures and their connection with the glacial sequence (Garrod 1928, 262).

### 5.1.2: Breuil's conception of contemporary Early Palaeolithic cultures, and influences on his scheme

Breuil soon produced two articles that applied similar ideas to the Early Palaeolithic (Breuil 1929; 1932b). The second of these, *Le Paleolithique Ancien en Europe Occidentale et sa Chronologie*, gave the most detailed outline of his conception. In this paper, he described two contemporary Early Palaeolithic populations: a hand-axe-making group (Chellean, Acheulian, Micoquian) occupying the South and West of Europe, and a flake-making population (Crag, Clactonian, Levalloisian, Mousterian)

in the North and East of Europe (Breuil 1932b, 571). Table 5.2 provides a summary of Breuil's version of the Early Palaeolithic sequence.

Another brief word on industrial terminology is required before proceeding further. By this time, Warren's 'Mesvinian' industry from Clacton-on-Sea and Smith and Dewey's 'pre-Chellean' or 'Strépyan' from Swanscombe (see Chapter Four) were both covered by a single term, the 'Clactonian', a change in nomenclature that will be explained below. By 1932, Breuil was referring to the Chellean, the earliest true hand-axe industry, as the '*abbeyillienne*', since the site of Chelles contained a relatively late industry (Breuil 1932b, 571).

<i>Glacial succession</i>	<i>Flake-making populations</i>	<i>Hand-axe-making populations</i>
Würm (4 <sup>th</sup> glacial)	Mousterian (pre-Würm to post-Würm maximum)	
Riss-Würm (3 <sup>rd</sup> interglacial)	Levalloisian	Micoquian
Riss (3 <sup>rd</sup> glacial)	Levalloisian (pre-Riss to mid-Würm)	
Mindel-Riss (2 <sup>nd</sup> interglacial)		Acheulian
Mindel (2 <sup>nd</sup> glacial)	Clactonian (end of Günz-Mindel to beginning of Mindel-Riss)	
Gunz-Mindel (1 <sup>st</sup> interglacial)		Chellean (Abbevillian)
Günz (1 <sup>st</sup> glacial)		
	Ipswich (Crag)	

Table 5.2: Correlations made by Breuil (1932b, 573) between glaciations and parallel industrial lines.

The flake industries now clearly occupied a parallel line to the hand-axe group (Breuil 1932a, 131). The Clactonian lay at the root of two branches of flake industries, one progressing to the Levalloisian via the Mesvinian of Belgium; the other progressing to the Mousterian via the Tayacian, as Denis Peyrony's work at La Micoque (which will be discussed below) had demonstrated (Breuil 1932b, 571-572).

The climatic changes over the Quaternary period also formed a central part of Breuil's scheme (see Table 5.2). The glacial-interglacial cycle (and the Penck-Brückner terminology) supplied a useful relative chronology for his Early Palaeolithic industries, which he ranged right back to the Günz glaciation. In linking this cycle with an industrial pattern of alternating hand-axe and flake industries, Breuil was also

supplied with a ready explanation for such an alternation in borderland areas, such as northern France and southern England. He saw this pattern as the reflection of distinct hand-axe-making and flake-making groups, periodically pushed from their territories by the climatic changes that accompanied recurrent glaciations (Breuil 1932b, 570-571), describing how flake industries appeared at the approach of glaciations and lasted into the beginning of the interglacials, whereas the hand-axe industries were truly interglacial (Breuil 1932b, 573). As his scheme rose in popularity, the attempts that were made by British researchers to see this neat pattern in their glacial deposits would cause a certain amount of disruption to earlier ideas. This will be discussed in more detail in section 5.3.

Breuil was adamant that he was the first to conceive of distinct cultures in the lower and middle Quaternary: those with hand-axes and flakes, and the pure flake cultures with no hand-axes. When Leakey used a similar classification, Breuil suggested that he ought to have been cited (Breuil 1936, 208). However, as we have seen in Chapter Four, Hazzledine Warren was making similar suggestions in the early 1920s, based on an obscure industry he had discovered at Clacton-on-Sea (Warren 1922a, 602; 1923d, 614). Perhaps more importantly, Obermaier (1919), once a close colleague of Breuil, had developed comparable ideas which he outlined in more detail and on a broader scale at a time when Breuil was just starting to become interested in the Early Palaeolithic. These will be examined below, together with a brief discussion of the inspiration behind Breuil's glacial chronology, which formed such a central part of his scheme and which would cause such disruption to British Quaternary research in the 1930s.

#### *Breuil's old colleague, Obermaier*

As observed in Chapter Four, Obermaier had published an early outline of the alternating hand-axe and non-hand-axe industries (Obermaier 1906b; 1908), and he later made a more detailed case for the zones of Europe occupied by hand-axe and non-hand-axe making Early Palaeolithic populations (Obermaier 1919; see Fig. 4.18). Breuil and Obermaier worked closely together before the war, and it is therefore likely (despite Breuil's reticence on the matter) that Obermaier's work had a considerable influence on Breuil's later schemes – and, of course, that Breuil may

also have contributed to Obermaier's early formulation of such ideas. Narr (in Collins 1969, 309) was also of the opinion that Breuil adopted Obermaier's views of two different traditions. This connection is worth exploring in a little more detail.



Fig. 5.3: Paul Wernert, Hugo Obermaier and Henri Breuil c. 1913 (ULC: MB, Add.7959, Box 1), reproduced courtesy of Cambridge University Library.

There were many similarities between Obermaier and Breuil. Obermaier was only one month older than Breuil, both were priests, and when they first met in 1904, they spent much time exchanging information on the Palaeolithic of Central Europe and France (Breuil 1950, 105-106). Obermaier and Breuil met frequently over the next few years at conferences and in the field. When Obermaier published his 1908 account of alternating Early Palaeolithic hand-axe and non-hand-axe industries in France, Breuil defended this in print, remarking:

*'j'ai souvent causé avec Obermaier des diverses coupures du y Paléolithique ancien, et autant que j'ai pu suivre son texte, il ne s'éloigne pas notablement de mes propres opinions'* (Breuil 1908, 416).

'I often talked with Obermaier on the various divisions of the Lower Palaeolithic, and as much as I could follow his [1908] text, it does not move away notably from my own opinions.'

At this period, Breuil also agreed with Obermaier that:

*'Il y a certainement des gisements antérieurs à l'Acheuléen supérieur et qui n'ont pas de coup de poing (Micoque inférieur)'* (Breuil 1908, 417).

'There are certainly layers preceding the upper Acheulean and which do not have hand-axes (lower Micoque).'

By 1910, Obermaier was working alongside Breuil as a fellow Professor at the *Institut de Paléontologie Humaine* (Fig. 5.3), and they maintained ties despite the



<i>Glacial succession</i>	<i>Spain</i>	<i>France</i>	<i>Germany</i>
Würm (4 <sup>th</sup> glacial)	Magdalenian Solutrean Aurignacian (Capsian) Mousterian	Magdalenian Solutrean Aurignacian Mousterian Younger Acheulian (hand-axes)	Magdalenian Solutrean Aurignacian Mousterian Younger Acheulian (hand-axes)
Riss-Würm (3 <sup>rd</sup> interglacial)	Acheulian Chellean	Older Acheulian (no hand-axes) Chellean	Older Acheulian (no hand-axes) Chellean (?=Pre-Mousterian)
Riss (3 <sup>rd</sup> glacial)	Chellean		Pre-Mousterian
Mindel-Riss (2 <sup>nd</sup> interglacial)	Pre-Chellean	Pre-Chellean	Pre-Mousterian, Pre-Chellean

Table 5.3: Simplified summary of Obermaier's diagram above (Fig. 5.4). See also Table 4.6.

One aspect of Obermaier's scheme that differed from Breuil's 1930s version was that he was using a glacial chronology that only went back to the Mindel-Riss. Though Obermaier's chronology was longer than the versions more commonly used at the time (see below), Breuil's 1930s chronology was longer still. Since Breuil's glacial correlations would have an immense impact on British Quaternary chronologies in the 1930s, it is important to see where this influence came from. It seems to be linked to Obermaier's 1919 paper and to Breuil's visits to Britain in the early 1920s.

#### *Glacial chronologies in the late 1910s and early 1920s, and Breuil's visit to Britain in 1921*

Those who believed in an interglacial Palaeolithic in the late 1910s (and many British researchers were monoglacialisists at this time) were divided into two main schools of thought about how industries should be correlated to glaciations (Peake and Fleure 1927, 96). James Geikie, Penck, and much of the German school saw the Early Palaeolithic stretching back to the Second (Mindel-Riss) interglacial. However, the majority, led by Boule and the French school, thought that the first Palaeolithic industries arrived in Europe during the Third (Riss-Würm) interglacial (Obermaier 1906a, 374; Osborn 1915, 287), an association which compressed most of the Early Palaeolithic industries within the Riss-Würm interglacial episode (Osborn 1915, 236).

By 1919, it seems that Obermaier, who had originally followed the short chronology, was favouring a slightly longer version (see Table 5.3). However, Breuil, in his 1919 lectures on the French Palaeolithic deposits at the Paris *Institut*, was still using the short chronology of Boule and the French school. The following year, when lecturing on southern England, Breuil abandoned Boule's scheme, and now 'found it necessary to distribute the phenomena observed over several glacial and interglacial cycles' (Breuil 1948, 66). This decision that would gradually lead to the greatly expanded chronology of his influential 1932 paper (Breuil 1932b), and enable British researchers to correlate their own industrial sequences to his scheme via distinctive climatic marker deposits. Many such deposits would be taken from East Anglia and the ubiquitous Penck-Brückner glacial terminology would be used to assist interpretation. It would be surprising if this decision had not been partly inspired by Obermaier's 1919 chronology.

However, Breuil's use of the glacial cycle in developing the Somme succession, which formed the basis for his later schemes, was also connected to a visit that he made to Britain around 1921. This trip must have been linked to his growing interest in the Early Palaeolithic industries of the Somme Valley. The classic French sites which provided the original foundation for the Palaeolithic sequence (see Chapter Four) were themselves far from the glacial districts, which hindered correlation to the relative glacial chronology (Boule 1888, 133). French prehistorians were forced to other regions to clarify the industrial-glacial correlation (Boule 1888, 281; Breuil 1929, 104), and therefore looked upon the British glacial deposits with almost as much enthusiasm as the British exhibited for the archaeological succession of the Somme Valley (see Chapter Four).

The commotion caused to the eolith debates by Henri Breuil's visit to Britain in 1920, when he accepted Reid Moir's Foxhall Hall pre-palaeoliths, has been discussed in Chapter Three. Breuil may have named Warren's industry from Clacton-on-Sea the 'Mesvinian' (see Chapter Four) on this occasion, or perhaps in the following year when leading British geologists had shown him around the East Anglia and Thames areas in 1921. Breuil had also made an important observation for his own researches on this trip, later describing how this led him to realise that the solifluxion

deposits in periglacial areas that alternated with warmer phases offered a hitherto unexplored way of establishing good datum lines for a relative chronology (Breuil 1948, 66). These climatic markers appeared to provide a relative chronology for the Somme Valley industrial sequence that would enable Breuil to compare this famous sequence to other deposits across Europe, many of which were being aligned with the Alpine sequence of Penck and Brückner around this time. This connection provided him with the opportunity of applying his theories of Early Palaeolithic patterning beyond the Somme, and even beyond Europe.

Breuil described his observations of the solifluxion deposits as '*le point de depart de tout un renouveau de ces recherches de ce côté-ci du Channel*' (Breuil 1937a, 259) ('the starting point for a complete renewal of researches from this side of the Channel'). We will see below how Breuil's detailed, Somme-based succession of contemporary hand-axe- and flake-dominated Early Palaeolithic industries, linked to the Alpine glacial episodes, did indeed have an immense influence on British Palaeolithic research in the 1930s. However, Breuil's conclusions were also founded upon a variety of other important influences, some of which have been described above.

#### *Denis Peyrony*

Before concluding this consideration of the influences behind Breuil's scheme, some mention must be made of the case set out by Denis Peyrony (Fig. 5.5) for parallel Early Palaeolithic industrial cultures. His views were based on the numerous Middle Palaeolithic flake cultures from Le Moustier and La Micoque in south-west France (Peyrony 1930; 1931), so may not have had such an influence on Breuil's scheme as Obermaier's larger-scale conception, but Peyrony's work was certainly known to Breuil, and formed part of his wider picture of the Early Palaeolithic of Europe (Breuil 1932b, 572).



Fig. 5.5: Denis Peyrony, 1869-1954 (c. 1938) with the 'baton de Commandement' (enclosed in a letter from Caroline ['Byre Lucus?'] to Burkitt, 13<sup>th</sup> November 1938: ULC: MB, Add.7959, Box 3). Reproduced courtesy of Cambridge University Library.

At Le Moustier, Peyrony traced back two Mousterian industries (Mousterian of Acheulian Tradition, and typical Mousterian) to Acheulian times, arguing that these industries did not succeed each other in turn from the Acheulian era, but were the products of two contemporaneous Mousterian tribes (Peyrony 1930, 172-173). The following year he wrote up his 1929 excavations at La Micoque and reached a similar conclusion (Peyrony 1931). The typical Mousterian (level H) at La Micoque lay beneath the Micoquian (final Acheulian) levels, which made it contemporary with the full Acheulian – even earlier than his 1930 work at Le Moustier had suggested (Peyrony 1931, 441).

*'Il a dû se différencier de bonne heure sous la forme qu'on lui connaît et se développer parallèlement au Clactonian de Breuil, au Levalloisien, à Acheuléen et au Moustérien de tradition acheuléene. Ce sont diverses techniques industrielles qui ont été contemporaines'* (Peyrony 1931, 441).

'It must have differentiated early on into the form in which it is now known, and developed parallel to the Clactonian of Breuil, to the Levalloisian, to the Acheulean and to the Mousterian of Acheulian Tradition. These are various industrial techniques which were contemporaneous.'

### *The varied influences on Breuil's scheme*

When Breuil put forward his ideas, he had the advantage of working at a time when the idea of contemporary industries and greater industrial variety was starting to become more familiar to the wider research community. The similarities between his researches and the earlier conclusions of Obermaier suggest that Breuil may well have drawn on Obermaier's work, although as a close friend and colleague, Breuil would very likely have influenced Obermaier's research as well. Warren was in communication with Breuil in the early 1920s, and Breuil may also have been inspired by Warren's conclusions about the Clactonian (or vice versa). However, like the researches carried out independently by Peyrony, these were based on a smaller regional scale than the conceptions of Obermaier (1919: the whole of Europe) and Breuil (1932b: western Europe).

It is also notable that Breuil based his ideas upon Commont's Somme Valley researches, which were familiar and respected in Britain and had influenced past research (see Chapter Four). We shall see below how Breuil's link between his theories of the Early Palaeolithic and the Penck-Brückner glacial terminology gave it enormous potential for application across the whole of Europe. Finally, Breuil was regarded as an impressive and trustworthy spokesman. The reaction to his change of mind over Moir's pre-palaeoliths has been described in Chapter Three. It is also notable that Miles Burkitt was an early pupil of Breuil, and that Dorothy Garrod chose to study under Breuil at the *Institut* to learn about the Palaeolithic. These are some of the reasons why Breuil would have such an effect on British Palaeolithic research in the 1930s. They help to explain the gap between the events described in Chapter Four – the early recognition of 'anomalous' industries in the 1910s and the suggestions for contemporary hand-axe- and non-hand-axe making populations by Obermaier and Warren a little later – and the wider acceptance of parallel flake tool and hand-axe cultures in the 1930s (Leakey 1931, 233; Kelley 1937, 15; Lacaille 1939; 1940).

#### ***5.1.3: The broader changes in perception engendered by this scheme: the example of the Clactonian***

The Clactonian industry, familiar from previous chapters, has been selected as a touchstone to assess the influence of Breuil's scheme on perceptions of the British

Early Palaeolithic, and to supply another level of detail for a broader discussion of British Palaeolithic research. Since the Clactonian formed the trunk of a bushy branching tree, uncomplicated by contemporary flake industries, and only lying parallel to another tree of hand-axe industries, it provides a relatively clear indication of the changes that took place over the period under review. The following three sections will come back to the Clactonian to examine the influence of some of these developments on the perception of a particular British Palaeolithic industry.

*The Clactonian: background*

The Clactonian industry had become an important fixture of this new scheme, as the source of later flake-industries. A short review is required of the developments that had taken place since Warren's papers on what was then the 'Mesvinian' industry in the early 1920s before going on to see how Breuil's scheme directed interpretations of this industry in the 1930s.

The most obvious change since the early 1920s was in the name of Warren's industry. In 1926, Breuil had removed the old term 'Mesvinian' from the Clacton-on-Sea industry. The Belgian type-site had been revealed as '*un mélange fortuit de deux séries*' (Breuil 1930, 221) ('a fortuitous mixture of two series'). Breuil now applied the term Mesvinian to the more recent, and more Levallois-like, of the two industries (Breuil 1926, 178, footnote). The industry from Clacton-on-Sea was then named 'Clactonian' by Warren in 1926 (Warren 1926, 47, footnote; Chandler 1930, 81, footnote 2). Breuil seemed unaware of this (Chandler 1930, 81; Breuil 1932a, 132) but, coincidentally, he also named the industry '*Clactonienne*' in 1929 (Breuil 1932a, 125, footnote).

Mesvinian (Rutot)	Later stage: Mesvinian = early Levalloisian (Breuil 1926)
	Earlier stage: Clactonian (Warren 1926; Breuil 1929)

Table 5.4: The relationship between the Belgian Mesvinian, the Levalloisian, and the recently named Clactonian (previously the Mesvinian of Clacton-on-Sea).

Other assemblages had also joined Warren's industry from Clacton-on-Sea. In 1929 R.H. Chandler announced that the Mesvinian from Clacton-on-Sea (now starting to be known as the Clactonian) was present in the Thames valley (Chandler 1931a,

175; also noted by Breuil 1929, 105). This was the industry from the Lower Gravel of Swanscombe discussed in Chapter Four that Smith and Dewey had originally termed 'Strépyan' (Dewey 1932, 39). As the idea of contemporary cultures grew more popular, links were also made between the Clactonian and flake industries from the Somme valley, as well as more distant lands (Chandler 1932a, 70).

#### *Dating and correlation to the hand-axe industries*

Warren's difficulties in persuading his colleagues that the Clactonian was indeed an industry in its own right, and one that bore no relation to the Chellean or Acheulian, seemed to be over, now that Breuil's influence began to be felt in Britain. For fine-tuning the date of the Clactonian, the glacial chronology described above was rather coarse. However, once the industry was accepted as a contemporary of the typologically distinctive hand-axe industries, these could give a useful indication of relative date (see Kelley 1937, 15). This switch between the temporal and cultural aspects of industries can make for confusing reading, although it was a widespread practice at the time.

By the 1930s, the Clactonian was generally admitted to be partly contemporary with the Early Acheulian, as Warren had suggested previously (Warren 1922a, 602; 1923d, 614). Chandler (1930, 92, footnote 4) initially referred the Clactonian industry of the Swanscombe Lower Gravel to a pre-Acheulian age, since it lay beneath the industry of the Middle Gravel. The industry of the Middle Gravel was now regarded as Acheulian rather than the 'Chellean' it had been for Smith and Dewey (see Chapter Four; Chandler 1930, 79; Dewey 1932, 43), so Chandler's reasoning is simple enough to follow. However, he soon altered this date to 'Early St.-Acheul' (Chandler 1931b, 250), noting in a letter to E.E.L. Dixon:

'to call the Clactonian "Pre St. Acheul" was too comprehensive. It is more likely to be early St. Acheul perhaps a different race as there is damned little connection between the Clactonian & the St. Acheul' (Chandler to Dixon, 21<sup>st</sup> November 1930: BGS: GSM1/295).

Breuil described the Clactonian as lying '*entre le Chelléen et l'Acheuléen*' 'between the Chellean and the Acheulean' (Breuil 1929, 100). In the Thames Valley, the Chellean (later Breuil's 'Abbevillian') lay at a height above the 100-ft. level (the

Dartford Heath gravels), which led Breuil to suggest that this might comprise a distinct older river-terrace (Breuil 1931, 95; King and Oakley 1936, 59). Within the 100-ft. terrace the Acheulian lay above the Clactonian (Breuil 1929, 105; see also Table 5.2 above). However, Breuil's (1929; 1932b) picture of hand-axe- and flake-making populations, being pushed around Europe in response to climatic shifts, was not just a rigid alternation between the two populations. Breuil allowed for some geographical overlap between the two in borderland areas such as northern France and southern England, where his Clactonian continued into the beginning of the Mindel-Riss (Acheulian) interglacial (Breuil 1932b, 573).

The case presented by researchers like Warren, Chandler and Breuil placed the Clactonian industry in an interesting position – clearly distinct from, but contemporary with the Acheulian. As noted briefly in the conclusions to Chapter Four, this temporal, and occasionally geographical, overlap had certain implications for the interpretation of contemporary industries, and inspired some to emphasise features that not only maintained, but also caricatured, the distinction between the flake and hand-axe lines. The definitions of the Clactonian developed by Warren and Breuil, examined below, and the reception of these different classifications over the 1930s, allow us to explore the extent of Breuil's influence on British Palaeolithic research, and to clarify the important new expectations and questions introduced by his scheme.

#### *The different approaches of Warren and Breuil*

For Warren, the Clactonian was 'characterized above all by the strength and freedom of the flaking', but this was not exclusively a flake industry. Distinct tool types also formed an important part of his definition: scrapers, other retouched forms and, most important and distinctive of all, side-choppers (Warren 1924a, 38). These core-tools were fundamental to Warren's perception of his industry, as noted in Chapter Four.



Fig. 5.6: Samuel Hazzledine Warren (1872-1958) at work in his old clothes, taken just before the Second World War (BM(NH): KPO, DF140/7). Reproduced courtesy of the British Museum (Natural History).

In 1932 Warren published a list of his Clactonian types ranked in order of relative importance. The side-choppers came first, followed by the waste flakes, and then the trimmed flakes (Warren 1932a, 69). Rude pointed implements suggestive of the hand-axe appeared seventh in the list of eleven types, and the primitive tortoise cores came last (Warren 1932a, 69). Warren's definition took account of technique, but was firmly founded on tool types. He later remarked:

'The biface element in the Clacton industry is admirably illustrated in the literature of prehistory under the names of Strepyan & Prechellean [...]. The "Prechellean" implements automatically involve Clacton flakes struck off in making them, but the earlier collectors neglected rough flakes' (Warren, September 1941: PRM: Penniman Papers, Box 17).

For Breuil, however, technique dominated his classification of the Clactonian and other industries on the flake line, and types played a far more subsidiary role. Breuil outlined his definition of the Clactonian in two papers: *Le Clactonien et sa*

*Place dans la Chronologie* in 1930, followed by his 'monumental study of the industry' (Warren 1951, 108), *Les Industries à Éclats du Paléolithique Ancien, I. – Le Clactonien*, published in 1932 (Breuil 1930; 1932a). Breuil presented the Clactonian as a flake industry, distinct from the hand-axe dominated Acheulian (Breuil 1930, 221). Technique was central to Breuil's descriptions of the Clactonian and the way in which he related the industry to others on the same line. He described how the characteristic Clactonian flakes and cores lacked the prepared striking-platform so characteristic of later Levalloisian or Mousterian flake industries (Breuil 1930, 222). Warren's retouched core-tool implements, even if distinct from hand-axes, did not sit happily within Breuil's conception of an industry that belonged on the pure flake culture line, since such types tended to blur the distinction between the two cultural streams. Although Breuil observed the presence of retouched cores in the Clactonian industry, these were still described as '*nucléi*' or cores:

*'Les nucléi sont assez fréquemment réutilisés, mais sans idée systématique d'aboutir à un biface régulier comme ceux du Chelléen et de l'Acheuléen'* (Breuil 1932a, 132).

'the cores are quite frequently re-utilized / re-worked, but without the systematic idea that leads to a regular biface like those of the Chellean and the Acheulian.'

This difference in emphasis observed in the definitions of Warren and Breuil may have been stimulated by a difference in their underlying intentions. Although Warren's approach was naturally tempered by events on the Continent, he was primarily describing a local assemblage that he knew in detail, noting distinctive types as well as techniques. The way in which Warren structured his private museum clarifies his personal interests and thoughts on the palaeolithic. This museum, housed in a large room on the ground floor of his house, included geology, palaeontology and archaeology and was dominated by local finds, mainly from the Essex region (Anon 1907, 48). Besides such local concerns, Warren also attempted to place the Clactonian of Clacton-on-Sea in a regional context through comparison with similar industries from the Thames Valley and elsewhere.

Breuil, on the other hand, was less of a fieldworker and more of a synthesiser (McNabb 1996, 38); he took a more subjective and impressionistic approach to archaeological data (Smith 1962, 205-206). He wanted to position the Clactonian

within a general grouping of flake-cultures that lay parallel to the hand-axe group. Tool-making techniques provided an essential tool in constructing more general links between the Clactonian and other flake cultures, such as the Levalloisian or Mousterian and therefore formed a central focus of his definition.

Although Warren believed that the Clactonian was undeniably distinct from the Acheulian, he apparently found it difficult to reconcile his perception of the Clactonian (assemblage-based, regionally specific) with Breuil's more generalized view (based on technique rather than types, and emphasising the distinction from the parallel hand-axe line). We shall return to this problem of interpretation on different scales later in the chapter. Warren's core implements (bifacial forms and side-choppers) sat uneasily alongside the emerging caricature of the Clactonian as an early representative of a pure flake industry line. The influence of Breuil's scheme is evident from the reception given to these two types by certain colleagues, such as Warren's friend Kenneth Oakley (1911-1981) at the British Museum (Natural History), who followed Breuil's classification and his broader parallel culture scheme.

#### *The impact of Breuil's scheme and the reception of Warren's core implements*

Warren (1932a) described certain bifacial forms that he thought might be the result of Chelleo-Acheulian influence on the Clactonian industry. Although the bifacial implements might seem the obvious focus for argument over the Clactonian, nearly all were agreed that no Acheulian hand-axe forms were present (Breuil 1932a, 132; Chandler, cited in Anon 1930, 145; Oakley in Oakley and Leakey 1937, 239). The presence of bifacial forms was therefore of little concern (also noted by White 2000, 20), so long as they remained scarce enough not to threaten the concept of the Clactonian as a flake-culture. As Oakley stated:

'It is untrue to say that bifacial forms are entirely absent [...] They form, however, a very subsidiary element in the early Clactonian industries, and are essentially different in facies from the *bifaces*, or hand-axes proper, associated with the Abbevillio-Acheulian family of cultures' (Oakley in Oakley and Leakey 1937, 235; emphasis in original).

Warren's side-choppers, however, were harder for Oakley and other researchers to accept. Warren described these types as the 'most important and

characteristic implement of the Clactonian industry' (Warren 1932b, 20). Chandler was open-minded about their use (Chandler 1930, 87; Chandler 1932b, 377). But Breuil did not recognise the choppers as anything but re-worked cores (Chandler 1930, 87; Breuil 1932a, 132), an argument that, initially, did not bother Warren unduly. He remarked: 'Even if their use as choppers be open to some doubt, the form is the most important feature of the Clactonian industry, and it is convenient to give it a name' (Warren 1932b, 21).

However, Warren wrote these words just before Breuil's scheme took off in Britain, and as we have seen above, core implements had no place within Breuil's vision of flake industries – although they had had in the past (see Dewey 1919). Oakley followed Breuil in seeing Warren's core *implements*, including the side-choppers, as mere *cores*: 'we maintain that the majority were in the first place cores, even if they were eventually utilized' (Oakley and Leakey 1937, 227). Oakley was happy that 'the industry belongs to what has been termed the flake-culture group'. For him, cores were by-products and not transformed into tools, hand-axes were absent, and bifacial forms formed only 'a very subsidiary element in the early Clactonian industries' (Oakley and Leakey 1937, 235).

The following year, when such opinions entered the Swanscombe Report, Warren considered the matter important enough for him to request Oakley and Hawkes, his co-authors, to make certain changes:

'In line 2 of "B", after "cores" I would suggest in brackets "(or perhaps core-implements)". In my own mind, I am satisfied that the Clacton industry includes many primitive core-implements, that I call "barbarous imitations of the Acheulian hand-axe" – I agree that there should be a "perhaps" or a "?" in the case of a joint report, although if I were writing only for myself I should call them bi-face core-implements without qualification' (Warren to Hawkes 2<sup>nd</sup> January 1938: BM(F): Swanscombe II).

Warren insisted that although no *Acheulean* hand-axe occurred in the Clactonian of Swanscombe or Clacton, the report must include the fact that 'crude core implements of pointed and other forms are of frequent occurrence' (Warren, draft report, c. 1938: BM(F): Swanscombe I). In the published version, this was accepted, with the proviso 'though a subsidiary one in the sense that they do not upset

the basic conception of it as a “flake-culture” (Hawkes, Oakley and Warren 1938, 31-32).

#### **5.1.4: In summary**

Breuil's success in presenting his version of contemporary Early Palaeolithic industries to British researchers led to a considerable change in the perception of the British Palaeolithic and, as we shall see, in the interpretations offered by British researchers to explain global Palaeolithic patterning. A great conceptual distinction had grown up between flake industries and other industries, usually characterized as hand-axe, biface, or core cultures. (Breuil (1932b, 571) spoke of *biface* and flake cultures, but noted that the term ‘bifaces’ was often applied to hand-axes: ‘*souvent appelés haches ou coups-de-poing*’; and Burkitt (1933b, 56) noted that ‘essentially coups-de-poing are core-tools’). This division between flake and core (or hand-axe) industries was maintained first by the methodological distinction of using technique to define flake industries, and both technique and typology to classify core industries (Paterson 1945, 5); and second, by associating each stream of industries with a different biological population, an old device familiar from Chapters Two and Three (Allen Brown 1893, 95; Lankester 1913; Sollas 1911).

The division between the flake and the hand-axe was growing so strong that some researchers even felt it necessary to remind their colleagues that although many flake cultures (particularly the earlier ones) lacked hand-axes, hand-axe cultures *did have flakes* (Garrod 1928, 266; Leakey 1931, 35; Kelley 1937, 15; Paterson 1945, 1). This revival of an argument that had apparently been effectively crushed decades ago (see Sollas 1911, 116) following the attacks on de Mortillet's (1883, 133) definition of the Chellean, which he had thought to be characterised *solely* by the hand-axe, illustrates the power of the new dual categorisation. In fact, Breuil had clearly distinguished between pure flake cultures with no hand-axes, and hand-axe-*and-flake* cultures (Breuil 1932b, 571; Breuil 1936, 208).

One of the most important points raised by this discussion is that besides working at an advantageous time, Breuil was also working on a larger scale than many other workers, in terms of his vision, his prestige, and the applicability of his

scheme. Herein lay the seeds of its downfall, as it would become too useful to too many different researchers, working on different data-sets at different scales. At this point in the narrative, in the 1930s, Breuil's scheme was still progressing upward. However, we shall return to this question of scale later in the chapter when assessing the reasons for its decline.

## 5.2: Parallel cultures, Quaternary chronologies, and the Breuil-Koslowski framework

The popularity of Breuil's (1929; 1932b) parallel culture scheme was due to more than its promised scope, Breuil's personal prestige, and the way in which his scheme seemed to explain confusing patterns in the Palaeolithic sequence. In a series of papers co-authored with Koslowski, which also appeared in the early 1930s, Breuil incorporated his general views of parallel cultures (discussed above) within a more detailed framework that placed extensively subdivided industries alongside his old glacial chronology (Breuil and Koslowski 1931; 1932; 1934). This level of chronological detail proved very attractive for British Quaternary researchers, for various reasons (explored below), and Breuil's ideas thus infiltrated perceptions of the British Palaeolithic from a number of different angles of research.

Breuil's parallel culture scheme and these co-authored papers were intricately related, but they will generally be distinguished from this point onwards as 'Breuil's scheme' and the 'Breuil-Koslowski framework'. The discussion above of Breuil's scheme focused mainly on the general ideas behind the concept of a flake line and a hand-axe line. The following analysis of the Breuil-Koslowski framework will examine attempts to order the Quaternary sequence. This approach not only makes our discussion clearer; it also emphasises the fact that Breuil's conception was alternately flexible and rigid by degrees, and could be read on several different levels. Here was another reason for its success.

### *5.2.1: The shift from monoglacial to interglacial perspectives on the Early Palaeolithic in Britain, and the use of Palaeolithic industries as zone-fossils*

We have noted above, and in Chapter Two, that most English geologists took a monoglacial stance during the first two decades of the twentieth century. Despite the work of James Geikie and others in the previous century, the majority believed that only one glacial episode had afflicted southern England (Lamplugh 1906, 533, 557; Sturge 1911, 101), and most assumed that the implements of the river-drifts were deposited after the Chalky Boulder Clay, the material reflection of the Ice Age

familiar from Chapter Two (Chandler 1914, 62; Kennard 1916, 259; Halls and Sainty 1926, 89; Boswell 1936, 149).

However, during the 1910s and 1920s James Reid Moir (already familiar from Chapter Three for his Pre-Palaeolithic discoveries) began describing palaeolithic industries interspersed through a *number* of boulder clays lying *beneath* the Chalky Boulder Clay in East Anglia. Moir's Palaeolithic finds undoubtedly assisted the shift from theories of a postglacial to an interglacial Palaeolithic, and the original Chalky Boulder Clay became one of several glacial marker deposits (Moir 1913a, 308-311; 1913b, 369-370; Marr 1920, 181; Moir 1922a, 561-562; 1927, 141; Boswell 1922, 303; Boswell 1936, 149; 1945, 67-68). Moir's identification of several industries at the earlier end of the Palaeolithic sequence must also have contributed to the development of a long glacial chronology in Britain, particularly his pre-palaeoliths (see Chapter Three), and an Early Chellean industry from the Cromer Forest-bed which he linked to the Günz-Mindel interglacial (Moir 1921b, 418; 1923, 136-137; 1929a, 103-104; 1929b, 243; Sollas 1923b, 334; Breuil 1926, 179; Peake and Fleure 1927, 99; Solomon 1932, 243; Boswell 1930, 380; 1932, 66). Even as early as 1920, Marr stated, 'whereas a few years ago it was universally maintained that man only appeared in post-glacial times, there are now few, if any, who would subscribe to this belief' (Marr 1920, 190).

Once the British Palaeolithic industries had been stretched out through geological time they could be used by British researchers who were trying to understand and order their Quaternary sequences as zone-fossils. Quaternary researchers often referred to the sequence of Palaeolithic industries recorded in the boulder clays of East Anglia or the river-deposits of the Thames Valley to confirm relative chronologies built upon other data-sets. In turn, Palaeolithic researchers used these Quaternary chronologies to help arrange their industrial sequences. We have seen above how important the solifluxion deposits were for Breuil's researches in the periglacial region of the Somme valley (Breuil 1948, 66). The acceptance of an interglacial Palaeolithic in Britain during the 1920s stimulated similar activities in East Anglia, where British researchers had the advantage of boulder clays for correlation. Of course, this interglacial Palaeolithic, combined with the suggestions of a long glacial chronology inherent in Moir's discoveries of industries scattered

through several glacial marker deposits, also set up the possibility of applying Breuil's scheme to the deposits of southern England.

*Boswell, Moir, and the development of the East Anglian boulder clay sequence*

Percy Boswell (Fig. 5.7), Professor of Geology at Liverpool and later at Imperial College, described the area around Cromer and Norwich as having 'the most complete series of Upper Pliocene and Lower Pleistocene deposits in Western Europe', 'in all probability the most complete sequence of glacial deposits in Britain' (Boswell 1923, 208). In the 1920s, Boswell had started to use Moir's recent discoveries of palaeolithic industries within the complicated East Anglian boulder clays to clarify the geological succession. The Palaeolithic content of these complex boulder clay deposits, which were rarely stratified clearly in section, provided an important source of information about their relative age.

Boswell was soon converted from his monoglacial stance and accepted the idea of an interglacial Palaeolithic, so the number of boulder clays, and of the industries interspersed amongst them, was set to increase. Boswell, his pupil John Solomon, and Reid Moir led interpretations of the East Anglian glacial and archaeological sequence over the 1920s and 1930s (Wright 1937, 82). Their work would become a standard reference point for researchers working on other aspects of the British Quaternary record.

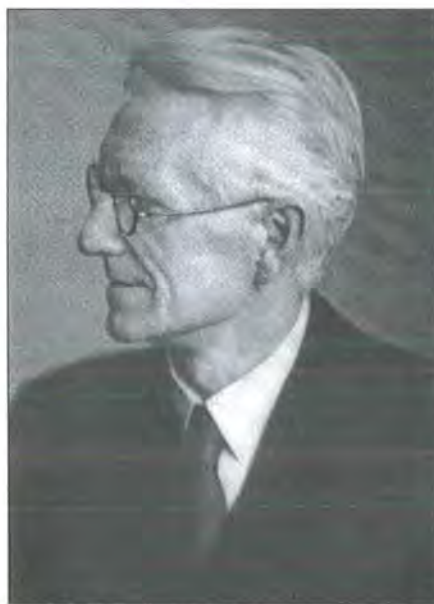


Fig. 5.7: Percy G.H. Boswell (1886-1960) (Mitchell 1961, 16).

*The effect of the parallel culture concept on industrial zone-fossils*

Boswell presented his conclusions on *Early Man and the Correlation of Glacial Deposits* to the 1930 meeting of the British Association for the Advancement of Science, held at Bristol. He sent a copy of his paper to Burkitt with the remark 'From this you will see that there are snags in the East Anglian succession', referring to the problems of correlating the boulder clays and their industries to the accepted Continental sequence of Alpine glaciations and industries (Boswell to Burkitt, October 6<sup>th</sup> 1930: ULC: MB, Add 7959, Box 2). In this paper, Boswell expounded on the geological value of archaeological industries, claiming:

'it would seem that we may expect most from the human industries themselves. If we use these industries for detailed correlation, we must regard them as contemporaneous, notwithstanding the time occupied in the migration of the peoples responsible for them, or in the diffusion of technique' (Boswell 1930, 379).

Palaeolithic industries provided an attractive aid to correlating the enormously complicated Quaternary deposits. There was some caution about such use of industries, but they seemed to offer a more specific chronological indication than many other Quaternary sources. Faunal change was coarse-grained; river or sea terraces might have seen differential uplift or had patchy records, making them unreliable as correlative tools; it was difficult to link river-terrace to glacial deposits; the maximum extensions of glaciers seemed too local in character, and were not synchronous over all Europe, or even over different parts of Britain (Boswell 1930, 379). In addition, various researchers distrusted the Alpine glacial sequence (Gregory 1930; Sandford 1930, 379; Boswell 1930, 379; Solomon 1930, 381; Peake 1930, 383; Sandford 1932, 2).

However, the recent rise in popularity of the parallel culture concept presented Quaternary research with a number of problems, and Boswell's paper provoked the following response from the archaeologist Harold Peake (1867-1946):

'As a geologist he [Boswell] is sceptical of the possibility of solving the problem [the East Anglian glacial succession] by geological means, and turns to archaeological evidence as supplying more reliable data for the purpose. As an archaeologist I have similar doubts as to the efficacy of my

own subject, though I am inclined to believe that the possibilities of the geological approach have been under-rated.

I would submit that the true succession of types of the Lower and Middle Palaeolithic phases, with which alone we are concerned, appears today to be by no means as certain as it did ten years ago. Broadly speaking we have evidence of successive stages of two industries, a core industry and a flake industry' (Peake 1930, 382).

Peake's problem was that this flake industry, which included 'the types known as Levallois and Le Moustier and perhaps others', was around before the core industry went out of use, which meant that 'the simple succession, Early Chelles, Chelles, Evolved Chelles, St. Acheul and Le Moustier no longer holds good' (Peake 1930, 383). With the rise in popularity of the parallel culture concept in the 1930s, following Breuil's papers on the subject, there was some concern that the value of industries as time-markers had depreciated. Two different zone fossils (such as a late Acheulian hand-axe and a Levallois flake) could now be of the same age. Dewey suggested that since the old industrial terminology had now become completely meaningless, it should be abandoned altogether (Dewey 1931, 147-148), and Boswell complained in the following year: 'If, as Mr. H. Peake has recently said, "... the simple succession Early Chelles, Chelles, Evolved Chelles, St. Acheul and Le Moustier no longer holds good," I personally almost despair of a solution' (Boswell 1931, 107).

In the same year, Breuil published the first in a series of articles co-authored with Koslowski that would not only complement his conception of parallel Palaeolithic cultures, described above, but would also provide a solution to this problem faced by Boswell and other Quaternary researchers suspicious of the value of this new variety of industries as zone fossils.

#### *The Breuil-Koslowski solution*

Breuil and Koslowski set out their framework in a series of four papers in the prestigious French journal *L'Anthropologie: Études de Stratigraphie Paléolithique dans le Nord de la France, la Belgique et l'Angleterre* (Breuil and Koslowski 1931; 1932; 1934). The reference to 'Angleterre' may have referred to a planned fifth paper, but this never appeared, perhaps because King and Oakley (1936) took up the ideas of the first four papers in a detailed article of their own on the Thames Valley

succession, examined below, that made such an addition redundant (a suggestion made by Roger Jacobi, pers. comm. 2003).

<i>Commont</i>	<i>Breuil and Koslowski 1931-1932</i>		
	<i>Industries</i>	<i>Glacial phases</i>	<i>Selected Fauna</i>
	Aurignacian, Solutrean, Magdalenian	2 <sup>nd</sup> phase of the Würm	
	Levalloisian V, Upper Levalloisian. True Mousterian of the caves is not present in the Somme, but influenced the Middle and Upper Levalloisian	1 <sup>st</sup> phase of the Würm	<i>Elephas primigenius</i> , <i>Rhinoceros tichorhinus</i> , <i>Rangifer tarandus</i>
Acheulian; Warm Mousterian	Final Acheulian VI, and VII (Micoquian); Levalloisian III-IV	Riss-Würm	Levalloisian III-IV is associated with <i>E. antiquus</i> , <i>Rh. mercki</i> , <i>Hippopotamus</i>
Acheulian	Upper Acheulian V, Levalloisian I-II	Riss	<i>E. primigenius</i> , <i>Rh. tichorhinus</i>
Evolved Chellean	Middle Acheulian IV	Mindel-Riss, 3 <sup>rd</sup> phase	
	Middle Acheulian II-III	Mindel-Riss, 2 <sup>nd</sup> phase	<i>Rh. mercki</i> , <i>E. antiquus</i> , <i>Hippopotamus</i>
Chellean	Early Acheulian Evolved Clactonian	Mindel-Riss, 1 <sup>st</sup> phase	<i>E. antiquus</i> , <i>E. primigenius</i>
Pre-Chellean	Chellean (and Early Clactonian of England)	Günz-Mindel	Pliocene affinities, inc. <i>E. antiquus</i> , <i>E. meridionalis</i>

Table 5.5: Comparison between Commont's industrial succession and the correlations between industries, glaciations and fauna produced by Breuil and Koslowski (1931; 1932).

The Breuil-Koslowski framework incorporated a hydra of minutely-subdivided industries on a number of parallel lines. Each industry was rigidly locked into position within a complex industrial framework through associations with other industries, with a broad-scale relative chronology supplied by the glacial succession. The debt to Victor Commont's earlier work in the Somme Valley is particularly evident in the content, references, and even in the illustrations of this series of papers (see Table 5.5 above). Breuil and Koslowski retained traditional ideas of industrial progress, but incorporated an enlarged list of flake industries on contemporaneous lines to the hand-axe cultures, and this brought another facet – a more detailed, but more prescriptive, rigidity – to Breuil's overall conception of two parallel industrial lines (see Fig. 5.8 below).

In the past, industrial terms had been ranged along a single line: here the names of industries (such as 'Acheulian') suggested *cultural* affinities, the existing practice of subdividing such industries (such as 'Acheulian I') positioned them in relation to each other in *time*, and the gradual shift from (say) Acheulian I to II to III (and so on) invoked the idea of *progress* (see Dennell 1990, 553). But once Breuil and Koslowski had further subdivided these industries and ranged them along several contemporary cultural lines (Chellean-Acheulian, Clactonian-Mesvinian-Levallois, and Clactonian-Tayacian-Mousterian: Breuil 1932b, 571-572), each subdivision was locked in time to sub-divisions of industries on other lines, as well as being locked into relative chronologies based on faunal and glacial / interglacial episodes.

By expanding the number of industrial sub-stages and then freezing each sub-stage into a temporal and cultural (i.e. technological and typological) position within a network of other sub-stages, Breuil and Koslowski managed to acknowledge complexity, suggest progressive evolutionary links, and also maintain the practice of linking cultures (or, in this case, industrial sub-stages) to specific points in time. Their framework promised greater certainty when pronouncing upon the age and cultural affinities of an industry, and it provided a detailed outline of how these industrial stages were related to the relative chronologies of terrace levels, palaeontological markers and glacial phases. The latter reason in particular suggests why the scheme proved so popular for researchers like Boswell who were trying to order the British Quaternary deposits. The industrial sub-stages offered an extremely fine-grained relative chronology for correlative purposes within the broader structure provided by the glacial chronology.

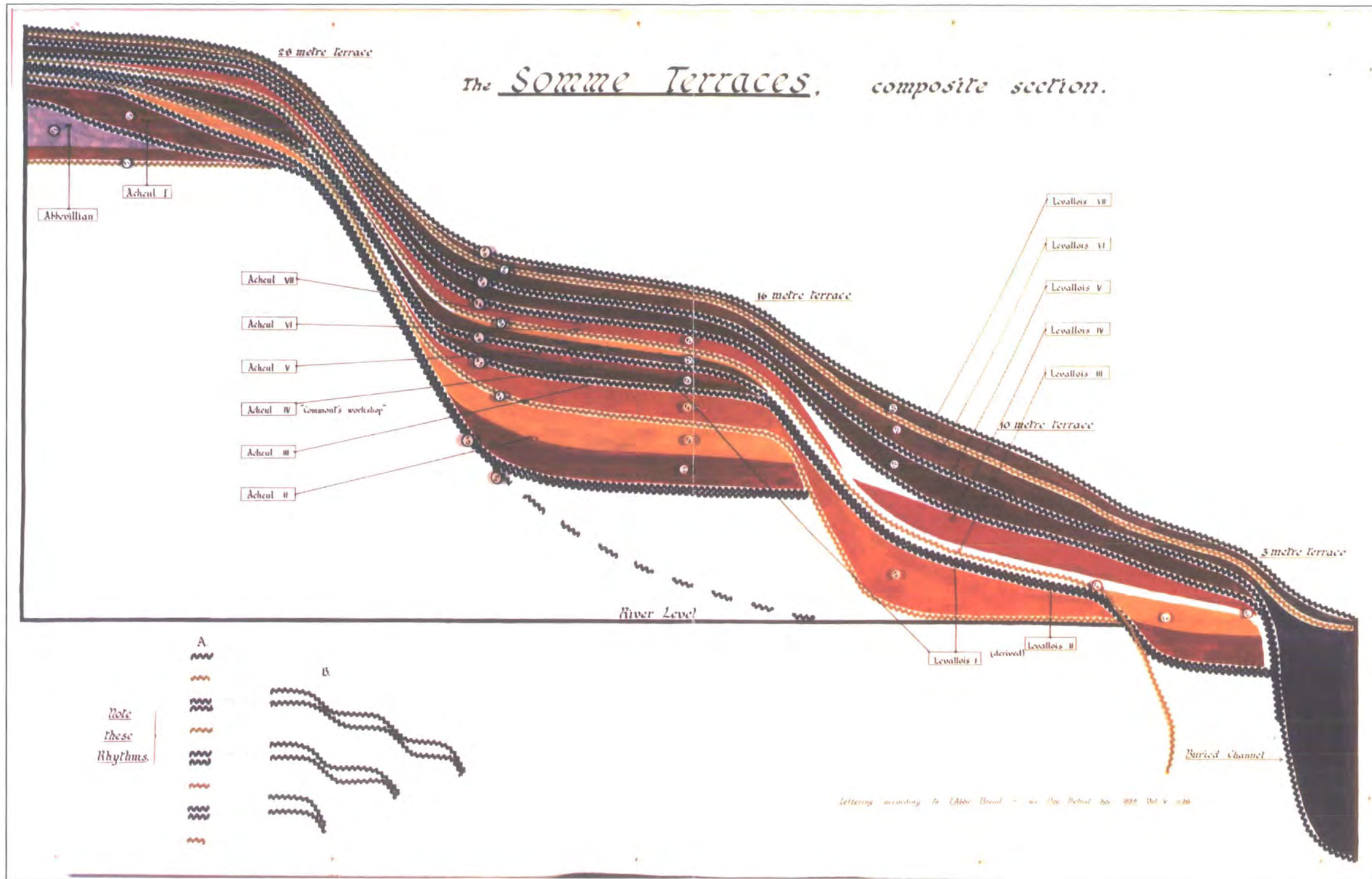


Fig. 5.8: Diagram of the Somme terrace deposits, with lettering according to Breuil (1939, 38) (Anon, AAM: LL1/2/10).  
 Reproduced courtesy of the Archaeology and Anthropology Museum, Cambridge.

*The British response to the Breuil-Koslowski framework*

The various flake industries of Breuil's scheme and the more detailed industrial sub-stages presented by the Breuil-Koslowski framework were seen by many Quaternary researchers as a welcome improvement upon the vague earlier terms that had characterised British flake industries in the days of the unilinear sequence. Indeed, all British Early Palaeolithic flake industries had previously been subsumed under the umbrella term 'Mousterian' (Oakley *et al.* 1948a, 24), sometimes with a prefix or suffix, for added detail. Thus Major James P.T. Burchell, who carried out much palaeolithic research in the Thames Valley, spoke of the 'Early Mousterian (Levalloisian)' and the 'Early Mousterian (Clactonian)' (Burchell 1932, 258, 262).

However, such generalised terminology was of little use for fine-grained Quaternary correlation, and, by 1935, Moir and Burchell observed: 'The term "Early Le Moustier" is now somewhat superseded by others, such as "Clacton" and "Levallois"' (Moir and Burchell 1935, 129). Boswell now argued that many industries previously identified by British researchers under the general term 'Mousterian' would now have to be re-examined in the light of recent work on flake cultures before they could be used for correlation:

'It is important that we should know to what industry exactly these should be referred; an early Levalloisian is suggested. Indeed, all the older records of "Mousterian" implements should now be re-examined and re-defined before they can be used for correlation or for dating' (Boswell 1936, 157).

Frederic Everard Zeuner (1905-1963) took a similar line. Zeuner was an 'eminent German geologist' who had arrived from Germany in June 1934 (in response to Hitler's anti-Jewish policies), and had recently been appointed Honorary Lecturer to the newly-opened London University Institute of Archaeology in 1937 (Clark 1937, 166). He warned:

'The middle Pleistocene of East Anglia has up to the present furnished very few implements, and these, moreover, do not help us in correlating. Finds of Clactonian, Levalloisian or Acheulian implements in this period are of little value unless the cultural phase can be reliably determined. All three techniques continue on the Continent until after the Saale (Riss 2) glaciation' (Zeuner 1937, 152).

However, if the cultural phase *could* be reliably determined – and Breuil himself was often happy enough to oblige – then industries could still be used as zone fossils. The chronological specificity of the Breuil-Koslowski classification further enhanced its popularity in Quaternary correlation. Breuil developed a strong presence in Britain over the 1930s (he was President of the Prehistoric Society in 1934) and soon became the acknowledged expert in determining such levels of detail (see Sainty 1927, 187; Dines 1929, 24; Bury 1935, 65; King and Oakley 1936, 52-53; Kelley 1937, 15; Zeuner 1937, 151). This position strengthened his hold over British Palaeolithic research and may also have delayed recognition of anomalies that did not fit his scheme and the Breuil-Koslowski framework.

In section 5.3 we shall see how the Breuil-Koslowski framework, and the inherent implication that detailed sub-stages of palaeolithic industries could be used as a fine-grained relative chronology, would stimulate some tumultuous changes to the British Quaternary chronologies. However, before looking at the reaction to this new framework, we will return once again to the Clactonian and see how this industry was being dissected after the style of Breuil and Koslowski.

### ***5.2.2: Classification and subdivision of a Clactonian I, II and III***

We have seen above, in section 5.1.3, how the Clactonian was positioned in time as partly contemporary with the early Acheulian, and how Breuil ran the Clactonian back into the preceding Mindel glaciation (Breuil 1932b, 573). The Breuil-Koslowski framework only mentioned an early Clactonian of England and an evolved Clactonian (Breuil and Koslowski 1934, 256-257, 313). However, the Clactonian was soon subdivided into a Clactonian I, IIA, IIB, and III. This development was associated with an assumption of progressive improvement in tool-making techniques that had been introduced in the earliest years of Palaeolithic archaeology (see Chapter Two), and which had more recently been employed by Breuil to define and order his pure flake culture line.

The subdivision of the Clactonian began in the Thames Valley, where Chandler identified two industries of different ages in the assemblage from the Swanscombe Lower Gravel in the late 1920s (Chandler 1930, 84): an older derived

series: Clactonian I; and a more recent contemporary series: Clactonian II, which had strong affinities to the industry from the type-site at Clacton-on-Sea (Chandler 1931a, 175). It is possible that Breuil suggested these Clactonian sub-stages when he visited Chandler at Swanscombe in 1928 (Chandler 1930, 81). In any case, by 1932 Breuil was also distinguishing between '*le Clacton I*' and '*Le Clacton évolué, que je note II provisoirement*' (Breuil 1932a, 129-130) ('The evolved Clactonian, which I have provisionally termed II'). Although Warren (1932a) was dubious about the Clactonian I of Chandler and Breuil, it was not long before such terms became more widely used.

The Clactonian line soon received a further addition: Breuil saw the High Lodge industry, which he had dragged back in time from the Würm to the preceding Riss glaciation in 1931 (Breuil to Sollas, 24<sup>th</sup> August 1931: BGS: GSM1/445; Sandford 1932, 18), as the latest Clactonian in England. This industry of finely-worked flake tools had previously been one of those described by British researchers as Mousterian (Sturge 1911, 69; Clarke 1917, 348; Moir 1921d, 367), and a little later as Early Mousterian (Moir 1927, 143). However, Breuil traced a relationship between the High Lodge industry and the Swanscombe Clactonian II on the basis of its reduction technique (Breuil 1932a, 160-162), and the High Lodge industry now became Clactonian III, on the end of the Clactonian line (Leakey 1934, 119-122; King and Oakley 1936, 60). This succession was apparently reinforced by Kenneth Oakley's observation of High Lodge (Clactonian III) flake-tools in the Middle Gravel of Barnfield pit, clearly stratified above the Clactonian II from the Lower Gravel (Oakley in Oakley and Leakey 241, 242). As Warren said: 'At least it would appear to be clear that the Clactonian-High Lodge sequence is an independent line of evolution in part contemporary with the sequence that we formerly called Strepayan-Chellian-Acheulian' (Warren 1932b, 26).

Oakley made a final refinement to this sequence following his excavations with Mary Leakey (then Nicol) at Jaywick Sands, Clacton, in 1934, when he divided the Clactonian II into an earlier and a later stage. Breuil's 'Clactonian II' covered both the industry from Clacton and that from the Lower Gravel of Barnfield Pit, Swanscombe. However, Warren and Oakley both believed that the Clactonian industry from the Lower Gravel of Barnfield pit, Swanscombe, in the Thames Valley was earlier than the industry from the Clacton Channel, Clacton-on-Sea (Warren

1932a; 1932b, 25; Oakley in Oakley and Leakey 1937, 240). Oakley wrote to Christopher Hawkes, Assistant Keeper in the Department of British and Medieval Antiquities at the British Museum: ‘It is proposed to refer to the “Lower Gravel” industry (Swansc.) as Clacton IIA, and to the Type industry of Jaywick and Clacton as Clacton IIB’ (Oakley to Hawkes, 1935: BM(F): misc. correspondence; emphasis in original). We will see below how Oakley’s opinion that a still later Clactonian III was present in the Middle Gravel of Barnfield pit would lead to problems (Oakley in Oakley and Leakey 1937, 242).

Clacton III	High Lodge Stoke Newington Swanscombe Middle Gravel
Clacton IIB	Clacton Channel, Clacton-on-Sea
Clacton IIA	Swanscombe Lower Gravel
Clacton I (associated with Abbevillian and (Early) Acheulian I-II)	Found in derived condition in the Swanscombe Lower Gravel

Table 5.6: Oakley’s Clactonian sub-stages (from King and Oakley 1936 and Oakley and Leakey 1937).

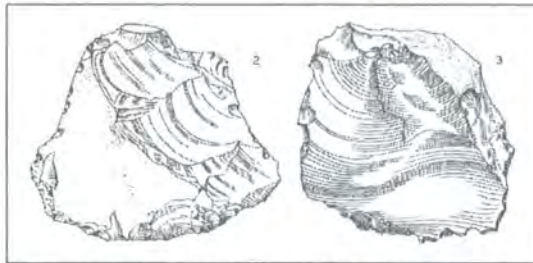


Fig. 5.9: Clactonian IIA flakes from Little Thurrock, Essex (Oakley and Leakey 1937, 255, Fig. 11).

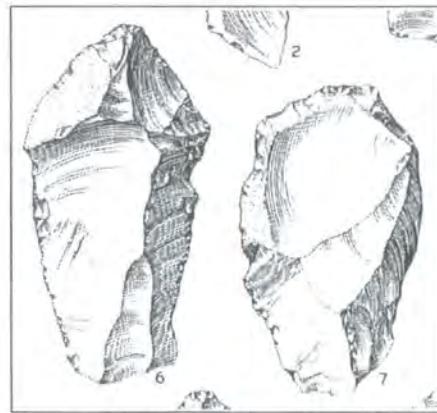


Fig. 5.10: Clactonian IIB. ‘Clactonian IIB flake-tools from Jaywick Sands’ (Oakley and Leakey 1937, 229, Fig. 3).



Fig. 5.11: 'Flake-tool of High Lodge (Clactonian III) type from the Middle Gravels, Barnfield Pit, Swanscombe' (Oakley and Leakey 1937, 241, Fig. 7).

By 1935 the Clactonian had been fully subdivided and the new terms began to be more widely used, a trend that was encouraged by the work of Leakey (1934), King and Oakley (1936), and Oakley and Leakey (1937). However, Warren was uneasy about these subdivisions, having seen the confusion caused in the past by the shifts in the meaning of de Mortillet's Chellean and Acheulian (Warren 1924c, 279; 1926, 41-43; 1932b, 5-7). The joint Report of the Swanscombe Committee, in which Warren collaborated with Oakley and Hawkes, described how:

'one of us (S.H.W.), viewing such serial sub-division with some misgiving, would prefer to speak of "the Clacton industry" for that of the type-site, and to call the main Lower Gravel industry "the Swanscombe Clactonian"' (Hawkes, Oakley and Warren in Swanscombe Committee Report 1938, 31).

The original draft in Warren's handwriting gives a little more detail of his reasoning. Here Warren, always cautious, viewed the subdivision by numbers:

'with some misgiving, *as these in course of time are too readily transferred from one industry to another (which confuses the literature)*, and would prefer to speak of "the Clacton industry" for that of the type-site, and to call the main Lower Gravel industry "the Swanscombe Clactonian". *This does not connote any undue implication of relative date*' (Warren c. 1938, draft report on the Lower Gravels of Barnfield Pit, for the Swanscombe Committee: BM(F): Swanscombe I Kent; parts not included in the published report italicised here).

But for other researchers the implication of relative date was a very attractive aspect of industrial subdivision. The Clactonian was just one of many industries to be carved up in this manner over the 1930s, and the change in approach to the Clactonian reflects broader changes in the perception of Early Palaeolithic industries in Britain, which were partly stimulated by Breuil's scheme and the Breuil-Koslowski framework. Some mention has been made of the use to which these subdivisions were put by Quaternary researchers seeking a more detailed understanding of their deposits. We will see in the next section that by the time Warren had made the above statement, Oakley had already used the Clactonian sub-stages as a fine-grained indicator to help him order the sequence of deposits left by the ancient River Thames. Warren would also come to appreciate the advantages of a detailed industrial chronology in determining the relative age of different deposits.

### ***5.2.3: In summary***

Despite the doubts of Peake and the concerns of Boswell, discussed above, about the chronological value of palaeolithic industries when the idea of contemporaneous industrial cultures was first taking hold (Peake 1930, 382), it appeared that with the right level of detail, these industries could still be used to assist Quaternary correlations. The confidence with which Breuil and Koslowski presented their framework seems to have encouraged the adoption of their particular version of parallel cultures. The Clactonian, only recently recognised as a distinct industry in its own right, was only one of many industries to undergo further subdivision through the 1930s, each industrial fragment promising a finer level of chronological resolution.

We shall see in section 5.3 how Palaeolithic researchers were not just presented with a straightforward choice of whether or not to adopt Breuil's dual scheme of parallel cultures and the Breuil-Koslowski framework of industrial subdivisions. Breuil's conception had set out a chronology on the coarse scale of the glacial sequence as well as on the finer scale of the industrial subdivisions, and therefore provided valuable assistance to various Quaternary researchers who were trying to put their geological sequences into order. On one hand, Breuil's ideas were presented as a straightforward means of ordering and interpreting palaeolithic industries; on the other hand, his conception was being used to arrange the various

Quaternary frameworks that Palaeolithic researchers also relied on to order their industrial sequence. Interpretations of the British Palaeolithic were assailed on all sides by Breuil's conception of the past, which led to some confusing and circular arguments.

### 5.3: Reshuffling the British Quaternary chronologies to fit the Breuil-Koslowski framework

Breuil drew a great distinction between British Early Palaeolithic research in the early 1920s – an age, in his opinion, of scattered local geological studies (Breuil 1937a, 259)<sup>28</sup> – and later research. He described:

*‘un renouveau de ces recherches de ce côté-ci du Channel, auquel les noms de King, Oakley, Palmer, Paterson, Burchell se sont brillamment attachés’* (Breuil 1937a, 259).

‘a renewal of researches from this side of the Channel, with which the names of King, Oakley, Palmer, Paterson, Burchell were glowingly linked.’

It is not surprising that Breuil characterised this period as a ‘renewal of researches’. His own research had stimulated some influential reassessments of British Quaternary succession over the 1930s, the most famous being King and Oakley’s (1936) interpretation of the Thames terraces, and the correlations by Boswell and Zeuner based on the East Anglian boulder clays (Boswell 1930; 1931; 1932; 1936; Zeuner 1937). This section of the chapter will examine two different scales at which researchers were working during the 1930s in their efforts made to correlate the British Quaternary deposits: the broad-scale attempts to link the British deposits to the Continent using the glacial chronology; and the more localised correlations within Britain between the deposits of the Thames Valley and the sunk channel of Clacton-on-Sea (the Clacton Channel), which were assisted by the new fine-grained industrial sequence. Both drew on Breuil’s research: his interpretations of the links between industries and glaciations, and the practice of detailed industrial subdivision that he had popularised, particularly in his papers with Koslowski, discussed above. .

Although the specifics of the arguments during the 1930s over the boulder clay sequence of East Anglia, the order of deposition in the Thames Valley, and the industrial sub-stages might seem of little relevance nowadays, this subject inspired extensive discussion at the time. The journals of the era are full of such arguments,

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<sup>28</sup> Breuil referred to *‘les recherches un peu dispersés de vos excellents chercheurs locaux’* (Breuil 1937a, 259) (‘the slightly-scattered researches of your excellent local researchers’).

and in the archives of those researchers, scraps of paper survive with scribbled suggestions of how the Alpine, East Anglian, Thames terrace and industrial sequences might be brought into alignment. An attempt has been made to keep the discussion below brief, but also to give an idea of the complexity of such correlations. For the current discussion, the *tone* of the argument, and the way that it reflects Breuil's influence, is far more important than the *content* of the case studies below.

### 5.3.1: *Breuil and Koslowski's glacial-industrial sequence, and broad scale Quaternary correlations within Britain*

'In a valley in die Alpen in neunteen-hundred neun  
Oh, there werkte Penck and Brückner, carving up the Pleistoceun.  
They had Günzes, they had Würmses in the Stages on their list,  
But the biggest and the Grösstest – Interglacial Mindel-Riss

CHORUS (repeat after each verse)  
*Correlation, constipation. Oh, now what are we to do?  
All these bedses without dateses. Oh we're really in the stew!*

(Song written for the last day of the 1973 conference on the Middle Pleistocene held at Burg Wartenstein, to be sung to the tune of *Clementine*. In Leakey 1984, 195).

We have seen above how the acceptance of an interglacial Palaeolithic had, by the 1920s, encouraged the use of Palaeolithic industries as zone-fossils in various attempts to order the boulder clays of East Anglia. The boulder clays themselves were also used as relative time-markers on a local scale, and various suggestions were made about how these were linked to the periglacial deposits of the Thames Valley. In turn, they were matched to the broader glacial chronology of the Continent and the Penck-Brückner glacial terminology. Some were doubtful as to the reliability or value of wider correlation between British and Alpine glaciations. Chandler explained to Dixon:

'I once (before the War) was impertinent enough to try & apply Penck's classification to Raised Beaches & Terraces inland but I read & read & took notes till I could not see the forest for trees & gave it up.

Now, I don't see why we should not work out our own glaciations without reference to what happened on the Continent. [...]

Stratification is the key, I believe & palaeontology may be helpful or, it may be a bloody nuisance, as we have seen.

E. Anglia is the place, but very difficult, & I confess I have never been able to read S.V. Wood & understand him' (Chandler to Dixon, 21<sup>st</sup> November 1930: BGS: GSM1/295).

Notwithstanding Chandler's caution and confusion, some attempts had been made to correlate the glaciations reflected in the boulder clays of East Anglia to the Alpine sequence.

In the 1920s, despite Moir's discoveries and suggestions in the early 1920s for a long glacial chronology (Smith 1932, 124), some were still following Boule's short chronology (described earlier in the chapter), which compressed the majority of Early Palaeolithic industries within the Riss-Würm interglacial (see Table 5.7 below). However, a few were starting to follow a longer chronology (Sollas 1923a, 5-6), and Moir's discoveries certainly contributed to this trend (see Peake and Fleure 1927, 98-102). When Breuil presented a similarly long chronology in the late 1920s and early 1930s, also using the Penck-Brückner glacial chronology, greater efforts were made by British researchers to match this scheme in the industrial and glacial sequences of East Anglia and the Thames Valley (see Table 5.8 below). In a complex process that will be explored below, it was soon generally accepted that the British Early Palaeolithic reached back far beyond the Riss glaciation, and the old 'Acheulian' was pushed back from the Riss-Würm to the Mindel-Riss, where it was joined by a number of flake industries hitherto grouped with the Mousterian (Boswell 1931, 109).

	<i>Penck</i>	<i>J. Geikie</i>	<i>Brooks</i>	<i>Obermaier</i>	<i>Dewey</i>	<i>Boule</i>	<i>Marr</i>	<i>Hinton &amp; Kennard</i>	<i>Warren</i>
<i>Azilian</i>					postglacial		postglacial	postglacial	postglacial
<i>Magdalenian</i>	Würm	Würm	Würm	Bühl	cold	cold	Bühl	glacial	cold
<i>Aurignacian</i>						postglacial		pre-glacial	
<i>Upper Mousterian</i>			Würm			Würm			
<i>Lower Mousterian</i>	Riss	Riss	Riss		temperate		Würm	pre-glacial	temperate
<i>Acheulian</i>									
<i>Chellean</i>				Riss					
<i>Pre-Chellean</i>									
<i>Upper Boulder Clays</i>	Mindel	Mindel	Mindel	Mindel	Riss	Riss			glacial
<i>Middle Glacial</i>									
<i>Lower Boulder Clays</i>	Günz				Mindel & Günz		Riss		glacial
<i>Cromer Forest Bed</i>									
<i>Weybourne and Chillesford</i>		Günz	Günz	Günz		Mindel & Günz	Günz		
<i>Crag</i>									

Table 5.7: Comparison of different glacial-industrial correlations (after Warren 1924c, 266: *Table of Comparative Pleistocene Classifications*). Note whether the Pre-Chellean is placed after the Mindel (long chronology) or after the Riss (short chronology). In 1924, when Warren developed the table on which this is based, many British geologists were tending to follow the short chronology of the French school (Boule) rather than the long chronology of the German school (later popularised by Breuil). Warren held the minority view that the last glaciation was associated with the Magdalenian rather than the Mousterian, so his selection of researchers probably reflects this (Warren 1924c, 270).

<i>Glacial / interglacial stages</i>	<i>Glacial sequence commonly used in early 1920s Britain (Burkitt 1920, 414; 1925, 47; Peake and Fleure 1927, 99)</i>	<i>Simplified summary of Breuil 1932b, 573</i>	
		<i>Flake</i>	<i>Hand-axe</i>
4 <sup>th</sup> glacial (Würm)	Mousterian	Mousterian	
3 <sup>rd</sup> interglacial (Riss-Würm)	Chellean, Acheulian, Early Mousterian		Micoquian
3 <sup>rd</sup> glaciation (Riss)		Levalloisian	
2 <sup>nd</sup> interglacial (Mindel-Riss)			Acheulian
2 <sup>nd</sup> glaciation (Mindel)		Clactonian	
1 <sup>st</sup> interglacial (Günz-Mindel)	Moir's Early Chellean / pre-Chellean		Chellean
1 <sup>st</sup> glaciation (Günz)			
	Moir's pre-palaeoliths (Foxhall Hall)	Ipswich (Crag)	

Table 5.8: Comparison between a glacial sequence in common use in Britain during the 1920s, and Breuil's (1932b) scheme.

Note that the Early Palaeolithic (with the exception of Moir's Early Chellean) is compressed into the Riss-Würm interglacial.

Two major problems faced those who wanted to correlate the East Anglian glacial deposits and the Thames Valley terrace sequence to the glacial-industrial sequence set out by Breuil (1932b) and Breuil and Koslowski (1931; 1932). The first problem was that there did not seem to be enough boulder clays in East Anglia to accommodate the now expanded Mousterian, and include Breuil's Levalloisian industry as well. The second was how to match up the cold deposits of the Thames Valley to both the East Anglian and the Alpine glacial sequences. The following discussion will illustrate the complex interconnections between different aspects of the Quaternary record and the influence which Breuil had over the interpretations that were put forward over the 1930s. Table 5.9, below, clarifies much of the terminology and correlations covered in the following discussion.

Glacial / interglacial stages	Glacial sequence commonly used in early 1920s Britain (Peake 1922, 126; Burkitt 1925, 47)	Breuil (1932b, 573), and Breuil and Koslowski (1931; 1932)		East Anglian boulder clays (Solomon 1932)	Thames Valley cold stages (King and Oakley 1936: linked to Solomon 1932)	East Anglian boulder clays (Boswell 1936, Zeuner 1937)
		Flake	Hand-axe			
Würm II					Slades Green Trail	Hunstanton (Brown) Boulder Clay (Upper Pal)
4 <sup>th</sup> glacial (Würm)	Mousterian	Mousterian, Upper Levalloisian		Upper Chalky Boulder Clay / Little Eastern (Mousterian)	Baker's Hole Coombe Rock (Levalloisian)	Upper Chalky Drift (Mousterian)
3 <sup>rd</sup> interglacial (Riss-Würm)	Chellean, Acheulian, Early Mousterian	Mid Levalloisian	Final Acheulian (Micoquian)		Acheulian, Clactonian	
3 <sup>rd</sup> glaciation (Riss)		Early Levalloisian	Upper Acheulian	Lower Chalky Boulder Clay / Great Eastern (?)	Chalky Jurassic Boulder Clay of Essex	Lower (Great) Chalky Boulder Clay (Acheulian, Clactonian)
2 <sup>nd</sup> interglacial (Mindel-Riss)		Evolved Clactonian	Early and Middle Acheulian		Clactonian I, Early Acheulian, Abbevillian	
2 <sup>nd</sup> glaciation (Mindel)		Clactonian		North Sea Drift / Norwich Brickearth	Plateau Drift	North Sea Drift and Cromer Till (? Chellean)
1 <sup>st</sup> interglacial (Günz-Mindel)		Early Clactonian	Abbevillian	Forest-bed (Abbevillian)		Cromer Forest-bed (Abbevillian)
1 <sup>st</sup> glaciation (Günz)						Later Crag deposits (pre-Chellean)
		Ipswich (Crag)		Crag (pre-Chellean)		

Table 5.9: Comparison of correlations suggested in the 1920s and 1930s, between the palaeolithic industries, the Alpine and East Anglian glacial sequences, and cold stages in the Thames Valley.

Not all the British schemes related to the Alpine sequence. King and Oakley (1936) referred only to Solomon (1932). Moir's pre-palaeolithic industries from the Crag (Chapter Three) are by now generally referred to as pre-Chellean.

*Fitting the Mousterian and Levalloisian into the glacial deposits of England, and the glacial-industrial chronology of Breuil*

In the 1920s, Boswell and Moir had reopened some of the famous old sections of Foxhall Road, and Hoxne. They proved to their satisfaction that the Acheulian and Early Mousterian<sup>29</sup> industries were interglacial, falling in time between the Lower and the Upper Chalky Boulder Clays, and the same pattern appeared to be present at High Lodge (Marr 1921, 362; Moir 1922a, 562; 1927, 142-143; Boswell in Boswell and Moir 1923, 233). Moir had also identified earlier industries in East Anglia, including an Early Chellean from the base of the Cromer Till. These industries and boulder clays seemed to suggest a long chronology for the British Palaeolithic, but would prove difficult to link to Breuil's glacial-industrial sequence (Moir 1921b, 418; Moir 1924a, 236-237; Boswell 1930, 380; Boswell in Solomon 1932, 271). John Solomon (1932, 271) disapproved of efforts to manipulate the geology to suit current archaeological views, remarking: 'The finding of flint implements *in situ* in various deposits in the Cromer district stimulated both geologists and, more especially, archaeologists, to a perfect orgy of correlation, much of it ill-informed' (Solomon 1932, 243). However, such correlations comprised an important aspect of discussion in the 1930s.

One of the most obvious obstacles to a neat correlation was that the Mousterian and the Levalloisian appeared to be compressed within the same glacial episode and this did not match Breuil's scheme, which assigned each industry its own glacial slot (see Table 5.9). The reasoning of British researchers went something like this: if the conclusions drawn by Moir and Boswell from the sites of Foxhall Road, Hoxne, and High Lodge were correct (Boswell in Boswell and Moir 1923, 233; Moir 1927, 142-143), then the Upper Chalky Boulder Clay was associated with the Mousterian and the Würm glaciation of the Alps (Solomon 1930, 382; Burkitt 1933b, 138; Boswell 1936, 160; Zeuner 1937, 139, 153). Some researchers admitted the possibility of wishful thinking on this question, but were clearly eager to see this

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<sup>29</sup> Moir's designation of the Hoxne implements as 'Early Mousterian' would later cause confusion, and delay the correlation of Baden-Powell's Hoxnian Interglacial with the mid-Acheulian age Swanscombe interglacial (Burkitt in West and Donner 1956, 90). West only recognised one cultural horizon (West 1956, 302).

connection in the record. F.W. Shotton, who was working on the boulder clay correlations, remarked in a letter to Burkitt:

'I really want finds in this pit before I can definitely say my "Upper Boulder Clay" is the Wurm I; fortunately, I worked out my sequences and correlations before getting any artefacts, and the finds of these are exactly of the age I wanted. So I feel reasonably happy about the situation up to date' (Shotton to Burkitt, February 22<sup>nd</sup> 1934: ULC: MB, Add 7959, Box 2).

However, this link meant that the Lower Boulder Clay which underlay the Acheulian deposits at Foxhall Road and Hoxne should be correlated with the preceding Riss glaciation. Since the Acheulian of East Anglia was sandwiched between the two boulder clays, this meant that the Levalloisian, which in Breuil's (1932b, 573) view occupied a glaciation between the two, was squeezed out. In Table 5.9, the (Mousterian) Upper Boulder Clay of East Anglia, and the (Levalloisian) Coombe Rock at Baker's Hole in the Thames Valley were left sharing the same cold slot (Boswell 1936, 160). As Oakley and Zeuner pointed out, it was no good even associating the Upper Chalky Boulder Clay of East Anglia with the early Levalloisian, instead of the Mousterian since Moir had found Upper Palaeolithic *Aurignacian* artefacts in the next boulder clay up, the Hunstanton, fifth and last boulder clay of East Anglia (Boswell 1931, 98). If this change was made, there would then be no room for the *Mousterian*, since Moir's Aurignacian-Hunstanton link put a cap on the glacial sequence (Boswell 1936, 158-159). Once again, the reader is referred to Table 5.9 for further elaboration of such problems, and for an ready guide to the terminology of the boulder clays.

This Mousterian-Levalloisian problem gives an idea of the kinds of arguments that characterised much research in the 1930s, following the rise in interest in flake cultures and the popularity of Breuil's long glacial chronology. Great perplexity was produced by such wishful correlation between the East Anglian glacial deposits and the glacial-industrial succession developed by Breuil from his Somme Valley researches, and additional confusion was created by Moir's industrial identifications through the East Anglian boulder clays.

*The problems of linking the industries of the Thames river-terraces to the glacial sequence*

The arguments surrounding the sequence of industries and cold stages in the Thames Valley became still more complex. In the 1930s, these were not only matched to Breuil's industrial-glacial pattern, but also to the local East Anglian glacial sequence – and as we have seen above, there were already problems in reconciling the East Anglian sequence with Breuil's industrial-glacial correlations.

Three important cold stages of Palaeolithic age had been observed in the Thames Valley: the earliest was the Chalky Jurassic Boulder Clay; then came the Coombe Rock; and finally the Arctic beds of the Admiralty Section and Ponder's End, which had probably been left by a late glaciation. Only the first two will concern us here. These cold stages were related to the sequence of river-terraces that had been developed over the previous few decades. The higher river-terraces were generally taken to be older in date, and the lower ones, closer to where the Thames now flowed, younger. In very general terms, the terrace lying at 100-ft. above sea-level had been associated with the Acheulian, whilst the Mousterian industries (which now also included the Levalloisian) were linked to the 50ft. terrace below (Hinton and Kennard 1905, 99; Dewey 1915, 112; Kennard 1916, 257).

The Chalky Jurassic Boulder Clay seemed to lie beneath the 100-ft. terrace, according to a section observed by Holmes (1892) at Hornchurch. (This has been mentioned in Chapter Two, and was one of the observations that led many researchers in the late nineteenth and early twentieth centuries to support a postglacial Palaeolithic.) However, the boulder clays did not generally reach the Thames region, and most of the glacial correlations were based on solifluxion deposits that suggested episodes of intense cold. This brings us to the second important cold marker deposit: the Coombe Rock solifluxion deposits that covered the Levalloisian deposits of the 50-ft terrace.

In accordance with the East Anglian sequence and the conclusions from High Lodge, Hoxne, and Foxhall Road, mentioned above, the Riss was associated with the boulder clay underlying the 100-ft terrace, which contained Acheulian and Clactonian industries (Dewey 1932, 52). The Coombe rock of the 50-ft. terrace, which was

associated with the Levalloisian, therefore tended to be linked to Würm, with the attendant problems of a missing Mousterian cold stage noted above. However, this placed the 100-ft. gravels of the Thames and their Acheulian and Clactonian II industries in the Riss-Würm interglacial, which was one stage too late for the Breuil-Koslowksi scheme, which positioned these industries in the preceding Mindel-Riss interglacial (see Table 5.10).

Leaving aside the problem of the missing Mousterian, the dating of the 100-ft. terrace deposits soon shifted to Mindel-Riss, in line with Breuil's scheme. This was made possible largely through Breuil's own change of mind regarding the glacial correlation of the Coombe Rock, which he had originally correlated to the Würm glaciation (Breuil 1926, 177). In 1931, Breuil decided that the Levalloisian from Northfleet belonged to a much earlier subdivision of the Levalloisian industry, equivalent to his Levalloisian I-II from the Somme, which was associated with the previous Riss glaciation (Breuil in Sandford 1932, 18). Working back from a Coombe Rock of Riss age, the deposits from the 100-ft terrace above fell neatly into the Mindel-Riss slot predicted by the Breuil-Koslowksi scheme (see Tables 5.5 and 5.10).

<i>Thames deposits</i>	<i>Late 1920s, Breuil 1926</i>	<i>Breuil 1931</i>
Ponder's End	Magdalenian	Würm (Late Levallois / Mousterian?)
Coombe Rock, 50-ft. terrace	Würm (Mousterian)	Riss (Early Levalloisian)
100-ft. terrace	Riss-Würm (Acheulian, Clactonian II)	Mindel-Riss (Acheulian, Clactonian II)
Chalky Boulder Clay (Hornchurch)	Riss	Mindel

Table 5.10: Correlation of the cold stages of the Thames terraces with the Alpine glaciations in the late 1920s, and according to Breuil (1931, 35).

The famous 1936 paper on the Thames Valley river-deposits co-authored by Kenneth Oakley and W.B.R. King (1889-1963), Professor of Geology at University College, London, rarely mentioned the Alpine glacial scheme. Although King and Oakley described Breuil's re-dating of the Baker's Hole Coombe Rock deposits to an earlier glacial stage, they avoided naming this stage as the Riss (King and Oakley 1936, 67). Such avoidance is hardly surprising in light of the confusion with the East Anglian sequence mentioned above. In addition, King and Oakley had associated the boulder clay beneath the 100-ft. terrace with the Lower (Great) Chalky Boulder Clay

of East Anglia (King and Oakley 1936, 53), which was *also* generally correlated to the Riss glaciation (Solomon 1930, 382; see Table 5.9). The difficulties were immense.

However, Oakley later noted that by 1937, these 100-ft gravels were generally thought to be Mindel-Riss (Oakley in West and Donner 1956, 89), and cautiously followed this correlation himself in the *Report on the Swanscombe Skull*, in which the influence of the French sequence is evident (McNabb 1996, 41). Indeed, it was often made quite explicit that the reason for this shift in interpretation was that the French associated Clactonian II and Early Acheulian with the Mindel-Riss – the French scheme clearly had a powerful influence on British interpretations at this time (Zeuner 1937, 154).<sup>30</sup>

This association of the 100-ft. terrace gravels as Mindel-Riss had a knock-on effect upon the old connections to the East Anglian boulder clays. Zeuner (1937, 154) now correlated the pre-100ft glaciation with the North Sea Drift (generally seen as Mindel) rather than with the succeeding Lower Chalky Boulder Clay of East Anglia, and also observed that the late Acheulian and Early Mousterian of Hoxne ‘has been modified so as to make the implements older, in order to fit Hoxne into the Mindel-Riss interglacial’ (Zeuner 1937, 141). Oakley also dropped the correlation of the pre-100-ft boulder clay with the Lower Chalky (Great Eastern) Boulder Clay of East Anglia (King and Oakley 1936, 53), and tentatively followed this alteration (Oakley 1939, 357). Everything was falling into place – even Moir’s pre-palaeoliths (now pre-Chellean) of the Craggs now met the expectations of an industry from the time of the Günz glaciation (Zeuner 1937, 151).

British Quaternary researchers were bewildered by the intricacies of correlations such as these over the 1930s; many turned to Breuil’s scheme and relied upon his personal opinion as they tried to sort out the complex interconnections between different aspects of the Quaternary record. Although not all researchers were

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<sup>30</sup> Breuil’s demonstration that the Chellean was Günz-Mindel age had also led Zeuner to shift this industry back a stage from the Mindel-Riss, with which he had correlated it two years previously (Zeuner 1937, 151).

obsessed with glacial correlation, the apparently reliable glacial-industrial scheme of Breuil and Koslowski seemed to offer a wonderful opportunity for sorting out the British Quaternary sequence. However, a change in one area required a change to another, until the glacial, fluvial and industrial sequences of Britain all danced to the same tune set by the glacial-industrial sequence of the Breuil-Koslowski framework.

### *5.3.2: The Breuil-Koslowski industrial subdivisions, and regional correlations between the Thames Valley and Clacton-on-Sea*

Turning now from the broad scale of glacial-industrial correlations to the finer scale of Breuil and Koslowski's industrial subdivisions, the latter proved very useful for fine-grained correlation of local sequences. We have seen the confusions faced by King and Oakley in their attempts to order the sequence of deposits left by the ancient River Thames using glacial markers. However, now that the Breuil-Koslowski scheme had reinforced the value of industries as zone-fossils, the Clactonian I-IIA-IIB-III subdivisions described in section 5.2.2 offered another way to make sense of the Thames geology. We will see how Oakley's belief in the secure basis of these industrial subdivisions (founded mainly on the assumption of a progressive improvement in technique) led him to construct some of his geological interpretations of Thames Valley Quaternary stages upon the archaeological chronology.



Fig. 5.12: 'Lion Point 2 mit Mr & Mrs. Warren' (Zeuner, 22<sup>nd</sup> April 1937: IAL: Zeuner Diary 2, p. 12), reproduced courtesy of the Institute of Archaeology, London.

Warren's implements from Clacton-on-Sea came from an ancient river-channel that he initially considered a tributary of the Thames (Warren 1922a, 597).

However, Warren later realised that the Clacton Channel was no tributary; the quantity of Thames-Medway material indicated that it was 'the actual channel of the Thames' (Warren 1932b, 16). Oakley and Leakey insisted:

'It is therefore of some importance, for geological as well as for archaeological reasons, that an exact correlation should be made between the deposits of the Clacton channel, and the terrace succession of the Lower Thames valley. Notwithstanding there have been differences of opinion on this point' (Oakley and Leakey 1937, 217).

*River-terrace sequences and the correlation of the Clacton Channel to the Thames Valley: some background*

Although the succession of terraces left by the Quaternary rivers as they eroded down through their beds seemed to provide useful physiographic markers for ordering the British Palaeolithic sequence, relative height above sea-level was not necessarily a reliable chronological guide to age, and the highest deposits were not necessarily the oldest. It had long been recognised that the ancient behaviour of rivers and seas was neither simple nor directional, and that different regions, such as the Somme and the Thames Valleys, might have experienced different histories of elevation and depression, tectonic movement and denudation (Harmer 1902, 422; Abbott 1911, 459-460; Hinton 1909-10, 501; Underwood 1912, 137; Smith 1915b, 3; Bury 1916, 189; 1923, 38-39; Warren 1924c, 269).

We have seen above that the Acheulian industries were generally associated with the 100-ft (or Boyn Hill) terrace, and the Mousterian industries with the 50-ft. (or Taplow) terrace (Hinton and Kennard 1905, 99; Dewey 1915, 112; Kennard 1916, 257). However, the complexity of the Thames Valley river-deposits had led to some suggestions for further subdivision of the Thames terraces, with intermediate terraces between these two (Treacher 1909-10, 198-199; Warren 1926 43; Burchell 1934, 37). The recognition of complex river behaviour, and the ambiguity and argument about the number of river-terraces in the Thames Valley in the early twentieth century, meant that different researchers often assigned the same deposits to different terraces.

Hinton and Kennard, leading palaeontologists of the time and committed monoglacials, blurred the boundary between the 100-ft. and the 50-ft. terraces still

further. They placed the major Pleistocene faunal break *within* the 50-ft. terrace (see Table 5.11), and Hinton grouped the early 50-ft terrace fauna with the 100-ft. terrace fauna (Hinton 1909-10, 504-505; 1926, 337-338; Kennard 1916, 259). In the light of all this, it is not surprising that the Clacton Channel was placed by some authors in the 100-ft. terrace (Breuil 1932a, 134), and by others in the 50-ft. terrace (Warren 1922a, 597; 1923d, 618).

<i>Terrace stages</i>	<i>Type-sites</i>	<i>Microtine fauna</i>
upper 50-ft. terrace		Cold fauna
early 50-ft. terrace	Grays (Little) Thurrock	Survivals of early warm fauna (Cromerian), and <i>Arvicola</i>
100-ft. terrace	Ingress Vale (Swanscombe)	Early warm fauna (Cromerian)

Table 5.11: Summary of Hinton's (1926) views on microtine fauna / terrace correlations.

*King and Oakley, the Thames Valley stages, and the new Clactonian subdivisions*

The development of detailed industrial subdivisions, after the manner of the Breuil-Koslowski framework, seemed to provide a way through this complexity. The Clactonian I-IIA-IIB-III sequence fell right in the middle of this area of ambiguity between the 50-ft. and 100-ft. terraces. These industries seemed to offer a finer-grained resolution than the old terrace system, Hinton and Kennard's fauna (their early warm fauna characterised the entire period and their finer-grained divisions were complicated: see Table 5.11 above), or the cold marker deposits (the Chalky Jurassic Boulder Clay and the Coombe Rock lay at either end of the problematic area: see Table 5.12 below).

When King and Oakley abandoned the old terrace divisions of the Thames, which were of dubious chronological significance (King and Oakley 1936, 52), and divided the Quaternary deposits of the Lower Thames Valley into numerous stages instead, they relied heavily on this Clactonian industrial sequence in reaching their geological conclusions about this confused area. Those relating to the period under discussion have been given in Table 5.12 below.

<i>Lower Thames stages</i>	<i>Selected sites</i>	<i>Industries</i>
Taplow	Taplow Station pit	Derived Early Levalloisian
Baker's Hole (Main Coombe Rock) (glacial period)	Baker's Hole, Northfleet	Early Levalloisian
Pre-Coombe Rock erosion	Wansunt Channel; Globe pit, Greenhithe; Swanscombe Upper Loam	Late Acheulian Early Levalloisian (Swanscombe Upper Loam)
Middle Barnfield (late Boyn Hill)	Final stage of aggradation. Middle Gravel of Swanscombe; Stoke Newington	Middle Acheulian (Swanscombe) Clactonian III and Middle Acheulian (Stoke Newington)
Ilford	Uphall pit; Cauliflower pit, Ilford	
Clacton-on-Sea	Aggradation. Deposits in the channels at Clacton-on-Sea and Little Thurrock	Clactonian IIB (Little Thurrock, Clacton-on-Sea)
Inter-Boyn Hill erosion	Sunk channels of Little Thurrock and Clacton (cut during this period of uplift)	
Lower Barnfield (early Boyn Hill)	Lower Gravel and Lower Loam of Barnfield pit, Swanscombe	Clactonian IIA Derived Clactonian I Derived Abbevillian
Pre-Boyn Hill erosion		
Great Eastern Glacier (glacial period)	Chalky Jurassic Boulder Clay at Hornchurch	

Table 5.12: The Lower Thames Valley stages of King and Oakley (1936) for the period covered in the text.

One of the major tasks was to decide how the industries and deposits from Swanscombe in the Thames Valley related to those from the channels at Clacton and Little Thurrock. Warren and Oakley both considered the Clactonian IIB industry from the Clacton Channel to be later than the Clactonian IIA from the Lower Gravel of Barnfield pit, Swanscombe (Warren 1932a; 1932b, 25; King and Oakley 1936, 57). However, there was one point on which they differed. Warren (1932a) initially concluded from the faunal evidence that his Clacton Channel industry had been deposited *after* the Middle Gravel of Barnfield pit (which lay at the 100-ft level), whereas Oakley argued that the Clacton Channel industry, though at a lower level, had been deposited *before* the Middle Gravel of Barnfield pit (Oakley and Leakey 1937, 253; Oakley 1939, 358).

Whereas Warren's (1932a) decision to date the Clacton Channel after the Middle Gravel of Barnfield pit seems to have been partly coloured by the palaeontological opinion of Hinton and Kennard, with whom he had often collaborated in the past (Warren 1922a, 597; 1923d, 618; Hinton 1926, 337-338). Oakley's interpretation was partly influenced by his belief that the various Clactonian sub-stages (I, IIA, IIB, and III) comprised a progressive series of successive industries. Oakley's archaeological assumptions would lead him into a rather unconventional interpretation of the Thames Valley terrace succession, as we shall see below (Oakley and Leakey 1937, 256; Oakley in Bull 1942, 31). The way in which he presented his case and the reception of these arguments illustrate the confidence invested by many researchers in the new industrial sub-stages during the 1930s. Oakley's arguments also demonstrate the close connections between interpretations of Palaeolithic archaeology and of Quaternary geology.

#### *Oakley's case*

At Swanscombe, in deposits lying at around the 100-ft. range, Oakley had Clactonian IIA in the Lower Gravel (Early Boyn Hill stage), and further up in the Swanscombe sequence, he had Middle Acheulian (III-IV) hand-axes from the Middle Gravel (Late Boyn Hill stage) (King and Oakley 1936, 56, 60). Oakley considered the Middle Acheulian industries to be contemporary with the Clacton III; this association was clear at Stoke Newington (King and Oakley 1936, 60), and the following year Oakley also recorded Clactonian III in the Swanscombe Middle Gravel (Oakley in Oakley and Leakey 1937, 242).

However, Oakley believed that the Clactonian IIB industry had been manufactured at a period *between* the deposition of the Lower and Middle Gravels of Swanscombe. His problem was that this Clactonian IIB industry of supposedly intermediate age had not been found at Swanscombe, but had been recovered from deposits lying at a far *lower* level at Clacton-on-Sea and Little Thurrock. This difference in level between Clacton and Swanscombe had puzzled earlier researchers (Chandler 1930, 90 footnote 4), and Oakley also admitted:

'the low level of the Clacton channel deposits requires some special explanation, if, as the evidence indubitably suggests, they post-date the Lower Gravel, but pre-date the Middle Gravels of the 100-ft. terrace' (Oakley in Oakley and Leakey 1937, 253).

The solution presented in King and Oakley (1936) drew upon the acknowledged complexity of Quaternary river behaviour described above. The Lower and the Middle Gravels were assigned to different stages in the Thames Valley chronology and it was suggested, on rather sparse geological grounds, that the ancient River Thames had rapidly cut down through its old bed between these two stages (the Inter-Boyn Hill erosion stage in Table 5.12 above), and then returned to its original 100-ft. level. This down-cutting event meant that the Clactonian IIB industries (Clacton-on-Sea and Little Thurrock) could now have been deposited *before* the Middle Gravel of the Late Boyn Hill stage, and still be at a relatively low level. Oakley's down-cutting and subsequent aggradation back to the 100-ft. level also explained why the Stoke Newington deposits lay at a lower level than the Middle Gravel of Swanscombe when both had the same industries and had been assigned to the same (Late Boyn Hill) stage. The Stoke Newington deposits (with their Clactonian III industry) could now belong to a slightly later stage in this same aggradation before the river returned to the 100-ft. level and deposited the Middle Gravel at Swanscombe (King and Oakley 1936, 57-60).

In conclusion, this period of channel cutting, which had been inspired by his reading of the archaeology, allowed Oakley to argue that

'it is not unreasonable to expect to find deposits of approximately the age of the Clacton gravels at a lower level than the 100-ft Terrace in the region of Swanscombe. Such deposits do in fact occur at Grays (Little) Thurrock, in Essex, on the north side of the river opposite Swanscombe' (Oakley and Leakey 1937, 254).

This argument was presented as a necessary development: a progression from old assumptions that higher-level gravels were older than lower-level gravels, and that all flake industries (originally all grouped as Mousterian) were later than hand-axe industries (Oakley and Leakey 1937, 256).

### *The reception of Oakley's argument*

How were such twists and turns through different Quaternary frameworks received by the wider intellectual community? By 1942, Warren had come round to Oakley's 'unorthodox view' (Oakley in Bull 1942, 31) that the Clacton Channel was formed at a period of hiatus *between* the deposition of the Lower and Middle Gravels of Swanscombe, and was correlated with the Little Thurrock Channel (Warren 1942, 173-174; 1951, 130). Warren justified this on the grounds that archaeology was a more fine-grained indicator than palaeontology, and now brought Oakley's Clactonian III into his own argument:

'As aggradation continued it would seem that the Furze Platt gravels [...] were built up to nearly 100ft O.D. The fossils prove a very close association between Grays, Clacton, and Swanscombe, but I think for more precise relative dating one must look to the human industries. In this connection it is noteworthy that the "middle gravel" of Swanscombe yields a Clactonian that is intermediate between the Clacton II of the name site and Clacton III (= High Lodge)' (Warren 1942, 174).

However, others attacked Oakley's view as special pleading. Chandler observed:

'The relations of the Swanscombe Clactonian-bearing gravels to the similar gravels on the foreshore at Clacton itself may need explanation, but such explanation cannot be found by dividing the lower from the upper deposits at Swanscombe by an enormous period of time and an interruption of deposition of which no sign can be seen in the greater part of the section' (Chandler in Bull 1942, 29).

Later still, at a time when Breuil's scheme was being challenged, S.W. Wooldridge stated that he too saw no evidence for 'positive movement large enough to invert the physiographic sequence of deposits' which was required to support King and Oakley's (1936) suggestion that the Clacton and Ilford stages intervened between the Lower and Middle Gravels of Barnfield Pit in the 100-ft. terrace (Wooldridge in Hare 1947, 337-338).

By the 1950s, as we shall see below, when the industrial subdivisions themselves came under threat, Oakley and Warren became uneasy about the relationship between Clacton, Little Thurrock, and Swanscombe. However, despite the arguments of geologists such as Chandler and Wooldridge, King and Oakley's

scheme became widely used. The circumstances of its creation illustrate the attractive nature of a fine-grained industrial chronology for the task of ordering and interpreting the complex Quaternary river-terrace deposits, and reflect some of the optimism introduced by the Breuil-Koslowski framework.

### *5.3.3: In summary*

The Breuil-Koslowski scheme quelled concerns that the concept of parallel industrial cultures, and contemporary industries would bring an end to the use of archaeological artefacts as zone-fossils. Detailed industrial sub-stages, such as the Clactonian I-IIA-IIB-III sequence discussed above, had become a very useful means of fine-tuning correlations within the relatively short period of geological time which comprised the Quaternary period. Many researchers regarded them to be superior to the slower changes offered by palaeontologists (Bull 1942, 2), or by geologists working on the sequence of Quaternary river-deposits (Zeuner in Bull 1942, 37). Breuil's old pupil, Miles Burkitt, gave unsurprisingly fulsome praise for 'the recent brilliant researches of Professor Breuil on the industries of the Somme valley', which were 'enabling prehistorians to subdivide minutely the lower palaeolithic industries in the area' (Burkitt 1933b, vi). Boswell stated:

'I believe that with caution we can use the industries of early man for correlative purposes. Although we recognize that in a few instances different cultures may be contemporaneous, the succession of industries has nowhere yet been found to occur in reverse order' (Boswell 1936, 155).

Sections 5.2 and 5.3 above illustrate the influence of the Breuil-Koslowski framework on two different scales. On a coarse-grained scale, British researchers brought their complex Quaternary patterns into line with the glacial-industrial sequence of this framework. On a more fine-grained scale, the new industrial subdivisions popularised by Breuil and Koslowski enabled researchers like Oakley to attempt a more detailed correlation between different regions, in this case connecting the Quaternary deposits of the Thames Valley deposits to those at Clacton-on-Sea. These arguments over fluvial and glacial deposits and Palaeolithic industries not only reveal the influence of Henri Breuil over British Palaeolithic research; they also demonstrate the symbiotic relationship between a number of different areas of

Quaternary research, and the difference in value accorded to each area by different researchers.

Breuil's scheme of parallel cultures, and its more detailed articulation within the Breuil-Koslowski framework, became popular in Britain for a number of reasons. There was already some appreciation of industrial contemporaneity in Britain, so Breuil's conception of parallel cultures fell on fertile ground. His research would also have struck a chord with British expectations, since it was based largely on Commont's research in the Somme Valley, which had moulded interpretation of the British Palaeolithic succession in the past (see Chapter Four). Breuil's personal standing gave his scheme added kudos. But perhaps most importantly of all, the level of rigid chronological detail offered by Breuil and Koslowski's industrial subdivisions offered a way of correlating and interpreting the complex British Quaternary deposits, and Breuil's ideas thus became an integral part of much Quaternary research over the 1930s and early 1940s (Dewey 1932, 45; Smith 1935, 417; Bury 1935, 63; King and Oakley 1936, 52-53; Lacaille 1936, 430; Lacaille 1940, 262).

Soon a quiet whisper that 'L'Abbé Breuil' happened to approve of your interpretation was a popular and effective means to stifle opposition, and in 1938, Grahame Clark noted: 'in matters general the great French prehistorian has become something of an unofficial referee' (Clark 1938, 340). In the midst of all the excitement, Breuil wrote to Oakley, expressing caution about his scheme. In this private letter he noted, 'I don't think the division in 7 stages of the Acheulean is quite satisfactory; it was a trial essay' and adding 'our knowledge of many details of levels and types is always in flux' (Breuil to Oakley, 11<sup>th</sup> December 1936: BM(NH): KPO, DF140/6).

Nonetheless, Breuil's conception had by now infiltrated British research on a number of different levels and soon became fossilised into a standard for interpretation on both a global and a regional scale. In the excitement of correlation and classification, there had been a tendency to forget that this was a framework based on work in progress, centred upon a regionally-specific Western European sequence, which had been focused mainly on the Somme Valley. The overextension

of the scheme would lead to its downfall, echoing the collapse of de Mortillet's version decades before.

#### **5.4: The downfall of Breuil's vision in the late 1940s and early 1950s: the core-flake dichotomy, industrial subdivisions, and variation on global and regional scales**

The downfall of Breuil's scheme and of his hold over British Palaeolithic research over the 1940s and 1950s was closely connected to the over-application of the parallel culture concept, and this provides the focus for the final section of this chapter. Paterson's criticism of Breuil's scheme in the late 1930s and early 1940s and his adaptation of industrial terminology to suit both the global and the local scale of analysis will be examined first, to highlight the problems with the ideas that Breuil had popularised in the 1930s. This is followed by two different perspectives on subsequent criticisms of the parallel culture concept in the late 1940s which will draw together the various different aspects of the arguments above: first, the attacks directed at the generalized idea of a hand-axe / flake dichotomy by researchers who had developed a global perspective of the Palaeolithic; and second, the suspicions of the time-specific industrial subdivisions of the Breuil-Kosłowski framework articulated by researchers working on the British Palaeolithic. We will then return to the Clactonian of the late 1940s and the 1950s, for a final glimpse of how these attacks had influenced the perception and interpretation of this industry.

##### ***5.4.1: T.T. Paterson, the global application of parallel flake and hand-axe cultures, and the problem of describing regional variation***

T.T. Paterson (1909-1994), who had gained a global overview through his work on the Palaeolithic of India, was also working on the regional scale in his research on the Clactonian industries of East Anglia, an interesting combination of approaches that led him to adapt Breuil's scheme for both scales of interpretation. He criticised Breuil's definition of the Clactonian, arguing that it was too general in scope to correspond to his own observations of regional variation, and did not match his series of industries at Barnham, or Oakley and Leakey's industry from Jaywick Sands at Clacton (Paterson 1942, 184-185). He observed that the 'Clactonian has come to be more a technical term than a cultural' term (Paterson 1937, 135).

Paterson therefore took up what had begun as a casual habit, and applied the term 'Clacton' to one of two great Palaeolithic industrial 'families' of global extent, the other being the 'Acheul' (Paterson 1940-41, 378; 1942, 184-185). He gave the original Clactonian a new, regionally specific name: the 'Brecklandian' (Paterson 1942, 184), which now became just one of many industries grouped within the Clacton family of industrial traditions. However, despite his criticism of Breuil's generalisations, Paterson developed a global picture of parallel flake and hand-axe lines that drew upon and even exaggerated the same distinctions that had driven the interpretations of Breuil, and of many previous researchers.

Paterson, like Breuil, maintained the great distinction between hand-axe and flake tools: his 'Clacton' culture was a flake industry that focused on the production of *flakes* from cores; an 'Acheul' culture concentrated on the production of *bifaces* – although these could be made on flakes (Paterson and Fagg 1940, 6). He also recognised the value of technological analysis in determining cultural influence amongst the flake cultures, and his Clacton family was defined solely on technique, which again reinforced the distinction from the Acheul family. Paterson observed 'In the so-called "flake culture", technique is the most important distinguishing criterion, whereas, in the "hand-axe culture", form is equally so' (Paterson 1945, 5).

The idea of progression was also employed by Paterson to order his two families through time (McNabb 1998, 10). The Clactonian family of industrial traditions was thus characterised by a great variety of cores demonstrating progressive improvement through time in preparation and complexity, and in the intentions of the maker about what type of flake was desired. Hand-axe types featured largely in Paterson's definitions of the Acheul family of traditions, and again, he described a definite advance in the extent and quality of secondary work, retouch, and the regularity of hand-axe edges over time (Paterson 1940-41, 378, 380-383).

<i>Family</i>	<i>Genus</i>	<i>Species</i>
Acheul	Acheul	Kenya Acheul Madras Acheul Stellenbosch North European Acheul, etc.
Clacton	Levallois	-----? Tayacian
	Moustier	
	Breckland-----	
	Soan-----	Anyathian Pre-Stellenbosch and I Kafuan Oldowan
	Chou-kou-tien	

Table 5.13: Families of Traditions (after Paterson 1940-41, 379).

Finally, alongside such methodological and terminological distinctions between the flake and hand-axe lines, Paterson adopted the idea that the flake-cultures were made by a different race to the hand-axe cultures. As we have seen in previous chapters, this idea provided a way of maintaining a distinction between contemporary industries, but also gave an opportunity of explaining industrial variety through culture-contact, or by a later divergence in the biological line. Paterson was drawing on ideas developed over the previous few decades.

During the 1930s, the concept of parallel and divergent lines of hominids had become very popular (Oakley 1937, 187). The flake line had generally been linked to the Neanderthals (who had long been associated with the Mousterian industry) and their ancestors (Sturge 1912, 213-214), leaving our own ancestors as the hand-axe makers. Until 1935, no reliable representatives of 'Acheulian man' had been discovered (Boule 1888, 679; Burkitt 1933b, 126), but it was generally assumed that they would have been very humanlike hominids. The intrusive finds of 'Ipswich Man', (Moir 1912c; Keith 1912, 209) and the Galley Hill skull (Sturge 1912, 214); the big-brained *Eoanthropus* from Piltdown (Spencer 1990, 37); and two modern-looking hominid finds made by Louis Leakey at Kanam and Kanjera, supposedly of Early Quaternary age (Leakey 1934, 209; 1935, 4, 11) were all put forward as possible ancestors on the hand-axe-making line. When the Swanscombe skull was found in 1935 and 1936, the modern aspect was again frequently emphasised (Keith

1938, 468; Clark and Morant 1938, 469). The associated industry was often identified as Acheulian (Marston 1937, 339-340; Hawkes 1938, 468; Garrod 1938b, 469; McNabb 1996, 43-44) rather than as Clactonian III, although both industries had been recovered near the skull (Oakley and Leakey 1935, 916).

Paterson was one of those who associated the Swanscombe skull with our own line and with the Acheulian hand-axes (Paterson 1940a, 167-169). In his opinion, a Palaeoanthropoid line made the flake industries, and a distinct Hominoid line made the hand-axe cultures, although both industries and hominids could hybridise with contact (Paterson 1940b, 50, 52). Paterson used this idea of 'fusion, by contact, of separate cultural entities' (Paterson and Fagg 1940, 22) to explain the presence of both Acheul and Clacton elements in specific localized industries, such as the Elveden series (see Fig. 5.13).

Paterson's picture of distinct 'Clacton' and 'Acheul' industrial families, with cultural interflow producing hybrid cultures such as the 'Clactonian Acheul' of Elveden (Paterson and Fagg 1940, 22-23), was widely accepted by Palaeolithic archaeologists (Movius 1953, 164). This scheme could be adapted to suit any possible regional variation. Oakley explained how 'The modern tendency is to expound their relations with the parallel Acheulian stages by recognising a varied range of industries locally hybridized between the two cultures' (Oakley *et al.* 1948a, 22). We shall see below how this great variety of possible culture-contact scenarios would spell the end for a broad application of the detailed time-specific industrial subdivisions of Breuil and Koslowski. Paterson's solution was to introduce a 'binomial system' to describe the variety of influences between his two families on a regional scale, a diversity which had provoked his dissatisfaction with Breuil's terminology. The 'Brecklandian Acheul', for instance described a dominant (generic) Acheulian influence, and a secondary (specific) Brecklandian influence (Paterson 1940b, 49).

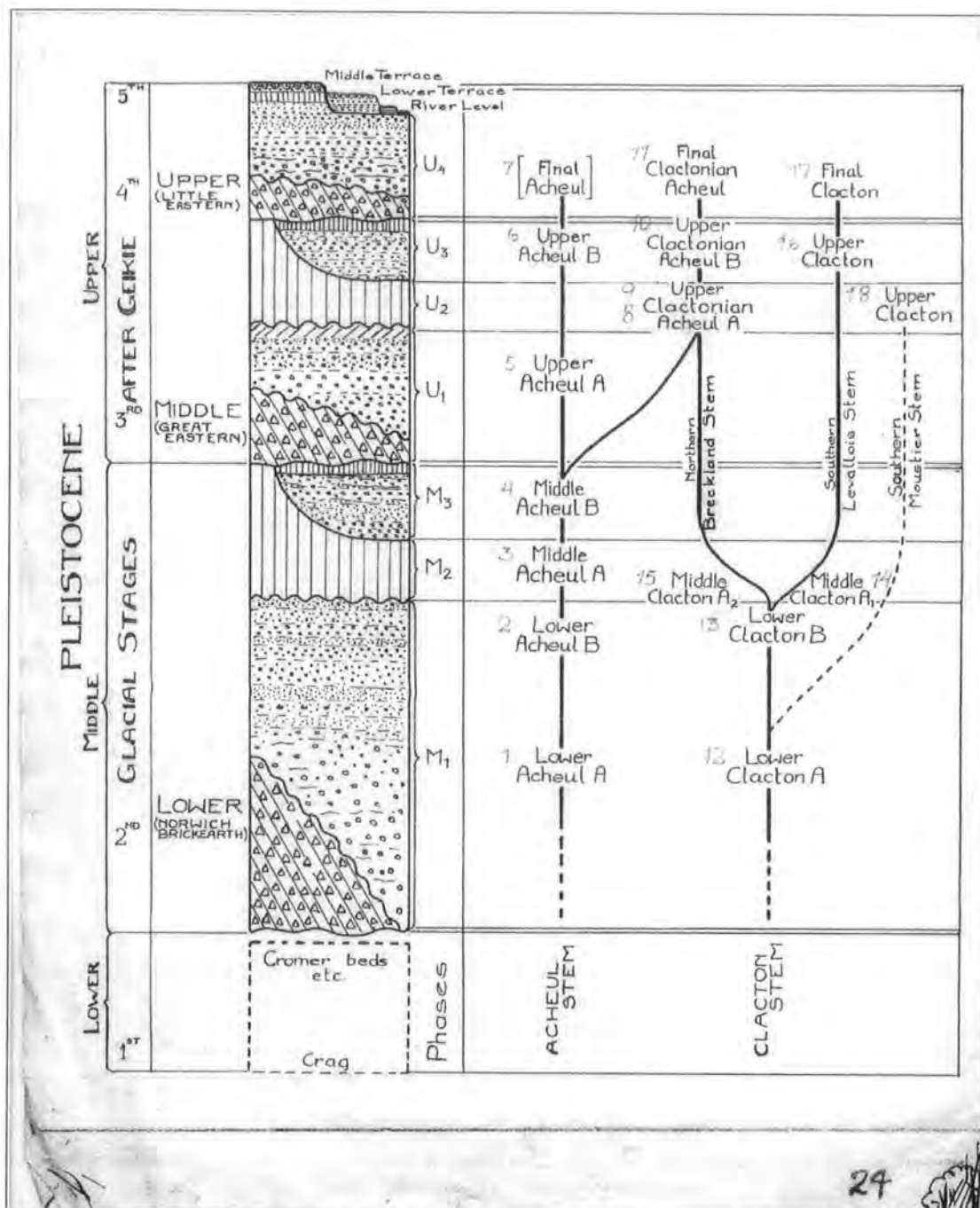


Fig. 5.13: Diagram by T.T. Paterson, illustrating the development of the Acheul and Clacton families. These are positioned in relation to the Thames Valley terraces and the East Anglian glacial deposits, which have been linked to the five glaciations of James Geikie. A wavy line in the section indicates a period of erosion. The Lower terrace is also known as the 100-ft.; the Middle terrace as the 50-ft. For the East Anglian glacial terminology, see Table 5.9, particularly Solomon 1932 (papers of T.T. Paterson, undated, *c.* 1945; AAM: W 21/1/3). Reproduced courtesy of the Archaeology and Anthropology Museum, Cambridge.

This adaptation of the old terminology – Paterson’s binomial system for description of regional variety and the re-application of ‘Clacton’ and ‘Acheul’ on a global scale – highlights the growing unease with industrial terms that had originally been created to describe the Western European sequence and which, through over application, were starting to lose their specificity and their cultural overtones (Movius 1953, 188). Paterson’s version of contemporary cultures was an explicit articulation of the perennial conflict which had also dogged Breuil’s vision. How were similar common assumptions and the same industrial terminology to be divided between two different scales of analysis: the interpretation of local sequences, and the task of ordering those sequences within a more general and broadly applicable classification?

Criticisms of the most popular schemes of the moment were often directed in the first case at the terminology. However, researchers from other areas of the world, who had developed slightly different expectations, saw a more fundamental problem, which required more than a temporary terminological modification. As in previous chapters, researchers working on different data-sets, or at different scales of analysis, would bring about new ways of looking at the past.

#### ***5.4.2: Problems of terminology and the confusion between culture and technology***

Certain difficulties with industrial terminology had emerged as the idea of contemporary hand-axe- and flake-making populations developed over the 1930s. One of the most important problems with this global picture of a flake and a hand-axe family was that the widespread use of technique (on the part of researchers like Paterson and Breuil) to tie flake industries from around the world into general ‘cultural’ lines had so diluted the meaning of ‘culture’ that it was in danger of becoming just one more classificatory device (Movius 1953, 167, 188). Concerns had been voiced for some time that the term ‘culture’ should imply some kind of ethnological connection between the makers of the industries, not some chance application of the same technique (Burkitt 1936a, 103; 1936b, 216; Oakley *et al.* 1948a, 22-23).

This was particularly problematic for researchers working outside Europe when they came to name their industries, as the term they employed might give a false

suggestion of a cultural (i.e. ethnological) connection. Thus Louis Leakey qualified many of his African industries with a prefix giving a regional label, such as 'Kenya Chellean', to avoid implications of date and culture-contact (Leakey 1931, 27-28). A.J.H. Goodwin and Clarence van Riet Lowe had been similarly cautious in their use of European industrial terminology to describe South African industries (Goodwin and Lowe 1929, 78, 96). Such self-conscious attempts to stand aside from conclusions based on the European sequence, and research carried out beyond Europe, contributed to suspicions that the flake / hand-axe distinction (which was now also commonly articulated as the flake / *core* distinction) was perhaps not of global applicability (Childe 1944, 19). Researchers working in Africa and Asia during the 1940s and early 1950s offered several damning criticisms of Breuil's parallel culture concept and the elaborations of Paterson.

#### *Opinions from beyond Europe: Africa*

Since 1929, Breuil had also taken an interest in the South African sequence. During the Second World War, having escaped from France to Lisbon, he was invited by General Smuts to spend the war in South Africa with C. van Riet Lowe (Fig. 5.14) at the University of Witwatersrand as a Research Officer on the Archaeological Survey (Mason 1965, 142). This move would later make Breuil unpopular with those who had remained in Paris under German occupation (Leakey 1974, 195). Perhaps more notably, it would substantially reduce his immediate personal influence on British researchers and on their interpretations of the British sequence. However, it also gave Breuil a look at another country that held a different pattern to the European sequence; he had already spent time in Central and Eastern Europe, and China in the 1920s (Smith 1962, 204).

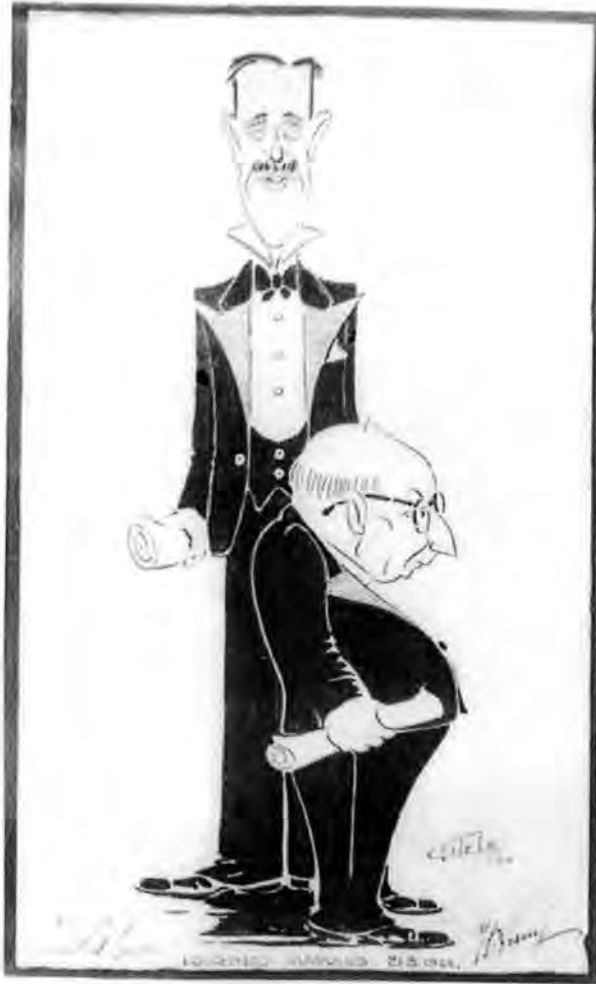


Fig. 5.14: Cartoon of Henri Breuil (short) and Clarence van Riet Lowe (tall) (1894-1956), drawn in 1944 (postcard from Lowe to Burkitt, 24<sup>th</sup> November 1950: ULC: MB, Add.7959, Box 1). Reproduced courtesy of Cambridge University Library.

The Levalloisian of Europe was thought to have developed independently from the Acheulian with culture-contact occurring later in the Palaeolithic. In South Africa, however, the Levallois technique seemed to form an integral part of the core (hand-axe) culture from the beginning of the Lower Palaeolithic to the end of the Middle Palaeolithic (Lowe and Breuil 1945, 50-51). This cast doubt on what Lowe described as the 'extraordinary two-stream development in Europe' (Lowe and Breuil 1945, 54), and meant that Lowe and Breuil had to employ an amazingly unwieldy terminology, such as 'core-cum-flake Stellenbosch I of Clacto-Abbevillian facies' (Lowe and Breuil 1945, 50). There was still a preoccupation with tracing the origins of the flake culture and the core (hand-axe) culture, their geographical movements

and mutual influences. However, Lowe hoped for 'a final interpretation which will strike a less discordant note with the state of affairs we know existed in Africa during Old Palaeolithic times' (Lowe and Breuil 1945, 54).

Mary Leakey later became concerned at Breuil's use of terms such as 'Clacto-Abbevillian' to describe techniques in African assemblages that only bore a superficial resemblance to the Clactonian, which she saw as a distinct cultural entity from western Europe. Mary Leakey wrote to Oakley 'I'm so glad you agree about the Clacton in Africa, or rather the lack of it.' Referring to Lowe (see Lowe 1932), she exclaimed 'he for one hasn't the vaguest idea what we really mean by the Clacton & has been led off on red herrings by Breuil & Co.' (M. Leakey to Oakley ?1949: BM(NH): KPO, DF140/7). Having been forced by Mary Leakey to read an offprint of the excavation at Jaywick Sands, Clacton-on-Sea (Oakley and Leakey 1937), Lowe confided to her that 'the Jaywick report is the only one which has convinced him of the existence of the Clacton culture', suggesting that Oakley do his bit, take Wayland, who was working on the African Palaeolithic in Uganda, in hand, 'and convince him that the Clacton is a thing on its own & not a waste product of other cultures' (M. Leakey to Oakley ?1950: BM(NH): KPO, DF140/7).

#### *Opinions from beyond Europe: Asia*

Research in Asia also contributed to a new perspective on the European industrial sequence. Hallam Movius attacked the rigid conception, recently expressed by researchers like Paterson, of two separate parallel industrial lines coursing through the millennia. Although he saw a fundamental difference between the Western hand-axe culture tradition and the Eastern chopper-chopping-tool tradition (Movius 1948, 350), he differed from Paterson (1940-41, 379) who had the early Asian choppers and chopping-tools at the base of the Clacton family line of flake industries (parallel to the Acheul line); Movius (1948, 410) saw them as a more ubiquitous 'basic cultural substratum' to *both* traditions.

Movius also remarked that although flake implements were present in all chopper-chopping-tool cultures of the Eastern complex, this was a *core-tool* complex, contemporary with the Western core-tool tradition characterized by hand-axes

(Movius 1948, 410-411). Later in the Pleistocene, the choppers 'became of secondary importance' in areas where the hand-axe had been developed, but persisted in the East, where the hand-axe innovation failed to penetrate (Movius 1948, 410).

'It cannot be too strongly emphasised that it is the *absence* of certain characteristic types of implements, as much as it is the *presence* of others, that identifies the Lower Palaeolithic chopper-chopping-tool complex of Southeastern Asia, Northern India, and China' (Movius 1948, 408; italics in original).

#### *Summary of criticisms from beyond Europe*

Observations from around the globe had revealed a 'bewildering multiplicity and geographical complexity of primitive stone cultures, industries and groups', which still failed to fit into any convincing general scheme (Caton-Thompson 1946a, 87). As we shall see below, a growing number of researchers were bringing different questions to the Palaeolithic research, and had started to argue that technological and typological change should be explained in more ecological and anthropological terms (Caton Thompson 1946a 88; Goodwin 1946, 91; Garrod 1946, 12-21; Movius 1948, 330-331, 349) rather than turning to 'the overworked conception of culture-contact to account for modifications and new introductions within a given stone industry' (Caton-Thompson 1946b, 57).

It was now evident that, in global terms, the old generalised distinctions claimed for the Western European sequence that tended to circle around the hand-axe / flake dichotomy were somewhat anomalous (Childe 1944, 19; Lowe and Breuil 1945, 54; Caton-Thompson 1946a, 87; Movius 1948, 409). Paterson's distinction between flake-cultures (Clacton) and hand-axe cultures (Acheul) was attacked by a number of critics (Paterson 1947, 187; Childe 1944, 19). Movius noted the nearly coincidental distribution of hand-axe cultures and the Levalloisian technique, and asserted: 'it is only in Western Europe, on the extreme northwestern periphery of their distribution, that hand-axes of the great Abbevilleo-Acheulian complex and the Levalloisian flake industries can be segregated' (Movius 1948, 408-409).

These perspectives from beyond western Europe, and a few considered attacks from closer to home (see Bordes 1950a, for example) seemed to have successfully

returned Breuil's (1932b) hand-axe / flake dichotomy to the area it had originally been intended for: western Europe. However, this worldwide overextension of an old scheme had brought about a reaction which would encourage new ways to perceive the Palaeolithic in Europe and new questions to ask.

#### ***5.4.3: British Palaeolithic research and criticisms of the Breuil-Koslowski framework: the case of Baker's Hole***

Gertrude Caton-Thompson had described the old theories of culture-contact as 'theories of the moment, numerous and occasionally persuasive, but against which the rumble of dissent may be already heard' (Caton-Thompson 1946a, 88). Returning now from this global foray to research in Britain, it seemed that the downfall of the more detailed aspect of Breuil's conception was partially due to the rise of such culture-contact theories in explaining regional diversity. Indeed, so much regional diversity had now been recognised in the later Pleistocene industries from western Europe that these could no longer be reconciled with the detailed time-specific industrial subdivisions of the Breuil-Koslowski framework. The relevance of Breuil's scheme was returning not just to western Europe, but (for the later industries at least) was now shrivelling further back to the Somme, where it had begun.

Oakley was still generally happy that the early stages of the well-established Acheulian (core or hand-axe) and Clactonian (flake) industries in Britain were mutually exclusive (Oakley *et al.* 1948b, 81). However, the lines of Clactonian and Acheulian industries became increasingly bushy and complicated as they progressed through time. Oakley suspected that they also left much more regionally-specific signatures, stating that 'The younger the deposits the shorter the correlating range of any archaeological evidence they may contain' (Oakley 1943, 30). The industrial result of culture-contact, a device used so often in the past to prop up the more general conception of parallel flake-making and hand-axe-making populations, could have differed between regions. There might, therefore, be considerable differences between the British sequence and Palaeolithic patterns in the Somme Valley, which had originally provided the basis for Breuil's scheme.

*The Baker's Hole industry: a warning to geologists*

One favourite example to illustrate such cultural complexities, the regional and temporal diversity of industrial stages, and the danger of using archaeological zone-fossils to build a relative Quaternary chronology, was the status of the Baker's Hole (Northfleet) industry (Oakley in Zeuner 1944; Oakley and King 1945; Oakley *et al.* 1948a, 1948b; Movius 1949, 1447; Bridgland 1994, 272). In 1931, Breuil had considered the Levalloisian of Baker's Hole (Northfleet) to be comparable to Levalloisian I-II of the Somme (Breuil to Sollas, 24<sup>th</sup> August 1931: BGS: GSM1/445). As we have seen above, this correlation had pushed the Coombe Rock back one glaciation, to the Riss (see Table 5.10), and altogether, this seemed to provide a reliable glacial and industrial link to the Continental sequence (Oakley in Zeuner 1944, 19; Movius 1949, 1447). Indeed, at the time, it made the Thames valley deposits fit more neatly within Breuil's glacial-industrial scheme.

However, a reanalysis of Spurrell's old Northfleet assemblage (Tramway Cutting west of Ebbsfleet) in the mid-1940s revealed Acheulian influence (hand-axes) which suggested that the industry was in fact much later: a contact-culture between Levalloisian and Acheulian populations, equivalent to Breuil's Levallois V of the Somme Valley – which in turn meant that the Coombe Rock was later than the Riss and was probably Würm I (Oakley 1943, 31; Oakley and King 1945, 51-52; Oakley *et al.* 1948a, 23). This re-dating shook the fragile sequence of other Quaternary correlations that had also been based on Baker's Hole, 'for the position of the deposit in the geological sequence had been largely inferred from an archaeological "fact"!' (Oakley in Zeuner 1944, 19) Archaeologists developed an increasing reliance on *geological* age determinations to direct interpretations of the palaeolithic sequence (Oakley *et al.* 1948b, 80; Movius 1949, 1446-1447).

Incidents like the Baker's Hole re-dating led geologists to become increasingly suspicious of the chronological value of Palaeolithic industries, and therefore of Breuil and Koslowski's framework. If the original age of the deposit (Riss) was to be maintained, then Acheulian-Levallois contact had to have occurred earlier in the Thames than the Somme, making the Thames Valley Levallois V contemporaneous with the Somme Valley Levallois II: 'Such possibilities make many geologists distrust the use of archaeological evidence' (Oakley 1943, 31).

Rather than re-date the deposit as Würm and maintain the temporal implications of his industrial sequence, Breuil argued that the Coombe Rock was indeed Rissian, explaining that it was only to be expected that regional differences would arise as the climate changed and migration occurred; a Levallois V stage was indeed reached in Britain when a Levallois I-II stage prevailed in the Somme (Breuil 1947, 831). Similar problems were encountered in interpreting German material, such as the Levalloisoid flake industry from Markleeberg (McBurney 1950, 169; 1956, 217). Suspicions were growing that Palaeolithic industries, or the later ones at least, only had chronological value within an extremely restricted geographical range, which cannot have increased the geologists' confidence in their potential as zone-fossils.

*Suspicion of the Breuil-Koslowski framework and the use of Palaeolithic industries as a fine-grained relative chronology*

Oakley now admitted 'that he was less confident than he was in 1936 about the value of palaeoliths for close dating, in view of the cultural complexities within the Lower Palaeolithic, which recent studies had revealed' (Oakley in Hare 1947, 337). Similarly, one reviewer of Zeuner's 1945 book, *The Pleistocene Period, its Climate, Chronology and Faunal Successions*, observed that Zeuner had turned aside from previous 'unhappy attempts to base a chronology in part on conjectural views of human cultural evolution' (Hollingworth 1947, 187). Zeuner based his Quaternary chronology on stratigraphy and climatic indications, and observed:

'If we are to obtain a clear idea of the sequence, overlap, alternation and duration of the industries of the Palaeolithic, it is absolutely essential to keep apart the geological (and palaeontological) evidence for the climatic chronology from the typological classification of the industries of early man' (Zeuner 1946, 146).

A broader atmosphere of disenchantment was gathering around the old industrial subdivisions centred upon the Somme sequence (Bordes 1956, 1). Oakley only recognised three of Breuil's seven Levalloisian stages in Britain (Oakley *et al.* 1948a, 23), and the broader application of Breuil's seven Acheulian stages from the Somme Valley was brought into doubt (Kelley to Oakley, 11<sup>th</sup> March 1949: BM(F):

misc. correspondence; Oakley 1952, 285; Movius 1953, 165; McBurney in West and McBurney 1954, 145).

Breuil also abandoned his parallel phyla once it became evident that this theory no longer neatly explained the industrial patterning of western Europe (Smith 1962, 203), and he had already admitted that his typological stages were likely to shift, 'As our knowledge of many details of levels and types is always in flux' (Breuil to Oakley, 11<sup>th</sup> December 1936: BM(NH): KPO, DF140/6). In 1948 Kennard informed J.W. Jackson, a fellow palaeontologist and conchologist:

'I am afraid that I have some bomb-shells for you. The first is that the Abbé Breuil has given up his scheme of typology. He told Marston "Geologists must tell us the age of the beds" if he had said Palaeontologists he would have been nearer the truth' (Kennard to Jackson, 26<sup>th</sup> January 1948: BMD: JWJ, Box 48).

#### ***5.4.4: New interpretations of the Clactonian in the late 1940s and the 1950s***

'It is not enough, to-day, to have out-grown the one-track Palaeolithic scheme of before 1925: the two track scheme – "core" and "flake" of the 1930s has been transcended too' (Hawkes 1951, 7).

After a brief episode of more global prominence and technological usage, the scale of the 'Clactonian' was retreating back to its original dimensions, and the term was once again becoming used to refer to a discrete regional culture, thanks to researchers like Mary Leakey and her attempts to convince the African researchers to stop using the term, as described above. Movius also saw the Clactonian as a distinct, localised industry (Movius 1953, 166). We have discussed Breuil's influence over cultural and chronological interpretations of Palaeolithic industries, particularly the Clactonian, when his Palaeolithic conception was in its ascendance. We shall now examine some of the changes in perspective on the British Clactonian over the late 1940s and early 1950s as the influence of his scheme declined. This illustrates some of the broader changes in perception of the British Palaeolithic that resulted from these criticisms (outlined above) on a global and a regional scale.

*The flake / hand-axe division and Warren's biface core implements*

The new perspective from outside western Europe exposed certain flaws in the idealised distinction between flake industries and hand-axe industries. Warren's core implements, once the subject of great controversy, were now becoming more acceptable in the light of the discovery of chopping-tool and pebble-tool traditions from Africa and Asia, a change that reflects the success of recent attacks on the old flake / hand-axe dichotomy. Warren was soon presenting the Clactonian as an offshoot from these primitive industries, which had finally provided him with a measure of support for his distinctive Clactonian core implement form, the side-chopper (Warren 1951, 109; 1958, 128). He stated:

'My primary definition of the industry rests on the nodule-tools, particularly the chopper, pointed and axe-edged. In the past these have been widely classed as cores, but now that the early pebble-tool industries of Asia and Africa are accepted, I feel that the two groups are so similar one to the other that the primitive Clacton bifaces must be placed on the same footing' (Warren 1951, 132).

Warren still made it clear that he preferred the term 'biface-work' to 'core-implement' (unless utilized cores were being described), 'as core is a more appropriate definition of waste' (Warren 1951, 111). Distinctive retouched tool types had always had a place in Warren's conception. As far as he was concerned, although the Clactonian had no *hand-axes*, the Acheulian did not have a monopoly on *bifaces*:

'The pointed (proto-boucher) group comprises a systematic repetition of a well-defined type, and I am fully persuaded would never be questioned as a true biface if found in anything other than a Clacton association' (Warren 1951, 111).

Oakley was also growing more doubtful about the distinction between flake-tool cultures and core-tool (hand-axe) cultures. Louis Leakey had privately informed him that he wished Oakley had avoided mention of flake cultures in his *Man the Tool-Maker*, since 'There is far too great a confusion already existing from the use of the terms "core" culture and "flake" culture' (Leakey to Oakley, 20<sup>th</sup> January 1950: BM(NH): KPO, DF140/7). Oakley conceded, 'I am sure that many of the Clactonian cores were chopper-tools (cf. Soan).' (Oakley to Leakey 20<sup>th</sup> February 1950: BM(NH), KPO, DF140/7).

As we have seen in the first section of this chapter, Warren's definition of the Clactonian had never dovetailed neatly with the generalised division between core (hand-axe)-cultures and flake-cultures of the 1930s and early 1940s. We suggested that this disjunction was partly due to the difference in scale between Breuil and Warren's conception of the Clactonian. With the recent attacks on generalised pictures of the past and the recognition of greater industrial variety, on a global and on a regional scale, Warren's version of the Clactonian now fitted perceptions of the 1950s much better than it had done before, and received a more welcoming reception.

*The Clactonian subdivisions: Clacton I, IIA, IIB, III*

Turning now to the subdivisions of the Clactonian discussed in sections 5.2.2 and 5.3.2, these had risen in popularity partly because of their use as a fine-grained Quaternary chronology. However, now that their chronological reputation had been tarred by cases of re-dating at sites like Baker's Hole, these subdivisions were falling into disuse. Associated with this demise was a sense of doubt over the Thames – Clacton correlations which had been built on this scheme.

In the early 1950s, Louis Leakey was revising his book *Adam's Ancestors*, and this led to an interesting three-way correspondence on the Clactonian between Leakey and Oakley, and Oakley and Warren. Leakey suggested an alternative to Oakley's succession. As we have seen above, Oakley's interpretation relied on a down-cutting event between the deposition of the Lower and Middle Gravels of Barnfield pit, Swanscombe, within which Oakley had placed the Clactonian IIB deposits from Clacton and Little Thurrock (Louis Leakey to Oakley, 17<sup>th</sup> January 1951: BM(NH): KPO, DF140/7). However, Oakley was now more cautious about his succession, writing to Warren:

'I am inclined to leave the problem of the relation of the channel to the Swanscombe terrace as an open one. I have not abandoned my original theory to the extent of suggesting that it can't be right; but I want to emphasize that it is by no means proved, and that it is even possible that the C. channel is later than the Middle Gravels' (Oakley to Warren, 12<sup>th</sup> February 1951: BM(NH): KPO, DF140/7).

In the course of his argument, Leakey asserted that the character of these three assemblages, ranged through the Thames and Clacton-on-Sea deposits, seemed to fit his suggested sequence better than Oakley's previous view of the case. However, Oakley was by now demonstrating more caution about using typology as a basis for constructing a different sequence:

'since the typological advancement of any particular "Clacton II" industry appears to vary horizontally (from place to place) as much as it does vertically, I would not be prepared to say on typological grounds whether the Clacton channel represents the low sea-level at the end of the Mindel glaciation or the low sea-level at the initiation of the Riss glaciation' (Oakley to Louis Leakey, 25<sup>th</sup> January 1951: BM(NH): KPO, DF140/7).

Leakey had already confided to Oakley that Clacton I did not exist since it included no 'genuine Clacton tool types', and the technological criterion of 'flakes made by "block on block" technique, with wide-angle platforms and unfacetted butts with semi-cones of percussion' 'are not in themselves any criteria of the Clacton culture and occur commonly in Abbevillian assemblages' (Louis Leakey to Oakley, 17<sup>th</sup> January 1951: BM(NH): KPO, DF140/7). In reply, Oakley agreed that there was no good evidence for Clacton I or Clacton III (Oakley to Louis Leakey, 25<sup>th</sup> January 1951: BM(NH): KPO, DF140/7), and described the position in diagrammatic form:

Clacton II	Acheulian III
Clacton II	Acheulian II
Clacton II	Acheulian I

The old Clactonian subdivisions, which Warren had never been happy with, were being reabsorbed within an ubiquitous Clactonian II, a significant shift from Oakley's earlier viewpoint (outlined in Table 5.6 above). In the same format, Oakley's original interpretation would look something like this (based on King and Oakley 1936, 55, 60):

Clactonian III	Acheulian III-IV
Clactonian II	
Clactonian I	Acheulian I-II, Abbevillian

The idea of culture-contact was clearly still popular for interpretations on a regional scale. However, as we have seen in the Baker's Hole case (section 5.4.3), this idea allowed for an amount of regional variation which probably contributed to the compression of the Clactonian subdivisions back within a single industry. Researchers became more wary of assuming that a more technologically advanced industry was necessarily later than a cruder industry. Perspectives of the Quaternary sequence, which had been built upon such assumptions in the past, now inherited these doubts. Interpretations of the Quaternary record shifted, but they also moved further away from their old reliance on the Palaeolithic industrial chronology – a tendency that would be further enhanced by the development of absolute dating techniques which were soon to become an important part of such research.

*The new perspectives on the past, and the Clactonian as a discrete regional entity*

One more aspect of the new perspectives that developed over the 1940s still requires some consideration. By the late 1940s and into the early 1950s the restrictions of the old tradition of British Palaeolithic research, inspired and dominated by Breuil's concept of parallel cultures and the Breuil-Koslowski framework, had been recognised. Childe (1944), Garrod (1946), Goodwin (1946), Movius (1948), McBurney (1950) and others argued that a different approach was needed, a more global, ecological and anthropological one which permitted an interpretation of the diversity of human activity in the past. This had an important influence upon the changing interpretations of industries such as the Clactonian.

A.J.H. Goodwin (1900-1959) stressed that prehistory was not a science, and that a more humanistic approach was needed (Goodwin 1946, 91). Garrod thought that the classification of archaeological industries as zone-fossils and the focus on the development of relative chronologies had now gone as far as it could, and it was time for archaeologists to recognise the human and social side of the various industries (Garrod 1946, 8-11). Typological classification, though a necessary preliminary, should only form the first stage of a fuller interpretation that explored how Man used these tools as he adapted to his environment (Garrod 1946, 12-21). Movius echoed this reaction against the straitjacket of zone-fossil classification, emphasising the

dangers lurking in regional variability, and calling for more concentration on the artefacts as social rather than as natural phenomena (Movius 1948, 330-331, 349).

Moving into the early 1950s, Charles McBurney (1950) presented an early attempt at a geographical or environmental approach to the Palaeolithic problem (Movius 1953, 169). Bordes and Bourgon (1951) carried out a statistical analysis of the Mousterian Complex, and argued that the presence of hand-axes was not necessarily of chronological significance (as had been assumed in the past) but could vary with seasonal activities (Movius 1953, 168).

Gertrude Caton-Thompson (1888-1985) agreed that the reaction against de Mortillet's provincial linear scheme of typological evolution had gone too far, asking 'Are we now slaves or masters of the broad classification of palaeoliths into so-called pebble, core, flake and blade *cultures*, rather than techno-typological devices?' These kinds of questions laid the framework for the modern school of Palaeolithic archaeology, which inherited the old conflict between regional and global scales of classification and interpretation – between the identification of cultures, and the techno-typological description of patterns. However, they also ushered in a number of ways of interpreting the Clactonian. Movius, like Warren, saw it as a distinct regional entity (Movius 1953, 166). However, in her attack on the idea of the Clactonian as a generic flake industry, Caton-Thompson suggested that, like many other industries, it might simply reflect 'the environmental conditions, and the local need at that remote moment for one sort of artifact rather than the other' (Caton Thompson 1946a, 87).

The very idea of culture had become debased during the recent exaggeration of a flake / hand-axe dichotomy, as had the reliance on technique to draw supposedly cultural connections between industries from around the world. Although the reaction of the 1940s and 1950s introduced an alternative approach based on ecological, functional and anthropological perspectives on the past, a suspicion of cultural interpretations seems to have survived alongside these new approaches, and this would colour interpretations of the future.

#### *5.4.5: In summary*

Breuil's conception of the Early Palaeolithic had been built on several levels; Paterson had also managed to elaborate this scheme on a number of levels; and it was eventually attacked from both ends of this spectrum. These were blurred and complicated arguments, which have been separated out in the discussion above for the sake of clarity. The old generalised divisions between hand-axe and flake were starting to break down and the chronological relevance of the finer industrial sub-stages was drawing back to the restricted regions of western Europe whence Breuil's scheme had originally emerged. The Clactonian ended up much as Warren had initially described it: as a varied industry with both flake and core-tool elements, distinct from the Acheulian. Although Warren had suggested a connection to the distant industries from Africa and Asia, the Clactonian of Britain was becoming regarded as a specific regional entity. However, it would be for future researchers to decide whether or not this was an anthropological or an ecological entity.

## 5.5: Conclusions

Breuil's scheme did not usher in an entirely new perspective on the past, and researchers such as Warren and Obermaier had proposed earlier versions of the parallel culture concept. However, Breuil's version enjoyed far greater popularity. The success of his scheme of contemporary Early Palaeolithic industrial cultures was partly due to historical tradition (the British sequence had enjoyed a long connection to the Somme Valley succession on which Breuil based his ideas). Social and institutional factors also helped to position Breuil in the front row of academic respectability. However, the success of his theories also owed much to timing and scope: his ideas arrived in Britain when some Quaternary researchers had begun to use the same standard glacial chronology as that used by Breuil for their Palaeolithic deposits, but had entered a period of doubt about the value of industries as zone-fossils. It was only a short step to the Breuil-Koslowski scheme, which, after all, used the same glacial chronology and promised a level of industrial detail which would put an end to much uncertainty. With the consequent reordering of the Quaternary context according to Breuil's views, his interpretation of the Early Palaeolithic was able to penetrate British research from many different angles.

One common theme though this and previous chapters has been the problem of scale and the tendency to overextend industrial sequences which had been largely based upon a restricted geographical area (see Geikie *versus* Dawkins in Chapter Two; the need to adapt de Mortillet's sequence in the 1910s, and the use by Smith and Dewey of Commont's Somme Valley sequence to interpret the Thames Valley sequence in Chapter Four). Breuil's vision worked on two different scales of detail: the chronological rigidity of the Breuil-Koslowski framework, with both glacial and industrial levels of detail; and the more general conception of Breuil's scheme, which described two distinct flake and hand-axe cultural entities. One reason for the success of his version of the parallel culture concept was that it could be interpreted differently to match a variety of observations; this, however, was also an important reason for its downfall. As his vision rose in success and popularity, it was stretched too thinly and the flaws showed through.

Attempts to apply the broader generalisations of this European-based sequence to more varied industrial patterns from around the world eventually weakened the parallel culture concept and its conceptual, methodological and biological division between a hand-axe line and a flake line. At the other end of the spectrum, the chronological bonds between Breuil's rigid industrial subdivisions were broken down by the recognition of greater regional variety between areas such as southern England and the Somme. A new perception of the Palaeolithic was now developing, more familiar to our current view of the past. It would, however, still be grounded firmly upon many of the old assumptions and expectations.

## CHAPTER 6

### Conclusions

What has been learned about the driving forces which lay behind the interpretations of Early Palaeolithic stone tools, offered between the 1880s and the 1950s? In Chapter One, this question was presented in two related parts.

- The first part concerned interpretations of the Palaeolithic, and enquired whether any assumptions and expectations about Palaeolithic artefacts could be identified which influenced the character of interpretations and the tone of intellectual arguments.
  
- The second part related to the context of research, and asked how the varied social and institutional backgrounds and different respective research agendas influenced the kinds of arguments and interactions which lay behind these interpretations of the British Early Palaeolithic record.

The conclusions offered below approach these two aspects in turn. The interpretations put forward between the 1880s and the 1950s are drawn together first (6.1) in order to identify general patterns in assumptions, expectations and interpretations. The context of these interpretations is then examined to unearth some of the more detailed and idiosyncratic influences on the character of these arguments (6.2). Admittedly, many of the most interesting aspects of the body of research examined in this thesis lie in the details of the chapters above, and some of the more general points made below might seem self-evident. However, since much of this information has never been treated in detail before, it is important to attempt a broader assessment of this fascinating period of scientific research.

## 6.1: Assumptions, expectations and patterns of Palaeolithic interpretation

Research into the British Early Palaeolithic was directed towards two linked goals. First, the development of broad classifications or rules of Quaternary patterning which could facilitate discussion and comparison of Palaeolithic deposits from different areas. Second, the task of interpreting the industrial patterning and suggesting implications this might have for the movement and activities of past groups of tool-makers. Some of the general trends observed in preceding chapters are drawn together below in order to identify recurrent patterns in classification and interpretation. Some suggestions are made about the way in which expectations and interpretations changed through time in line with prominent general theories, and how these also varied simultaneously with different scales and aims of research.

### *6.1.1: General assumptions and expectations of the British Early Palaeolithic*

The historical prominence of the palaeolithic sequence of western Europe fuelled certain basic assumptions that often recurred through the decades covered by the preceding chapters. The fact that the hand-axe was the most distinctive and, indeed, was the first recognised artefact of the Palaeolithic, provided an important stimulus to three major assumptions which will by now be familiar, and which directed expectations of Palaeolithic industrial patterning over this period.

- First, the assumption of progression is evident in many approaches to the British Early Palaeolithic. The persuasive nature of this concept, which also provided a helpful rule of thumb for placing various industries in relative chronological position, is most clearly demonstrated by the 'observation' of progression in the naturally-fractured eoliths and pre-palaeoliths (see Chapter Three).
- Second, the use of type-fossils offered valuable assistance in developing local industrial classifications and provided chronological markers which could be used in the construction of broader patterns. The most prominent type-fossil of the Early Palaeolithic was undoubtedly the hand-axe, and the rostrum-carinate

also received much attention on the part of researchers who were using similar principles to classify and interpret pre-palaeolithic industries.

- Third, the distinction between hand-axe and flake industries: initially a chronological distinction (see Chapter Two), this became more of a cultural distinction after the confusion over contemporary flake and hand-axe industries (see Chapter Four) and consequent shifts in perception and methodology. Researchers then started to describe and classify contemporary, but distinct, cultural lines of flake and hand-axe industries.

These assumptions were used in combination to classify discrete industries and build up generalised expectations of Palaeolithic patterning. Such expectations were often linked to particularly famous type-sequences, or type-regions. They were also connected to interpretations of palaeolithic tool-makers, and to reconstructions of Quaternary geology.

Taking interpretations of the tool-makers first: discontinuities between successive or contemporary industries were solved through an association with ethnically- or biologically-discrete groups, with the tools following a similar evolutionary trajectory to the tool-makers (see Sackett 1981, 90-93; 1991, 127-128). Culture-contact and divergence provided a flexible explanation for industrial variety as well as offering a reconstruction of the movements of palaeolithic tool-makers. Moving on to the Quaternary context of Palaeolithic discoveries: the value of the Palaeolithic industrial sequence as a relative Quaternary chronology also influenced expectation and interpretation, of which more below. These cultural-industrial and chronological-industrial links, together with the assumptions described above, drove expectations of the British Early Palaeolithic and reinforced the interpretations presented between the late nineteenth and mid-twentieth centuries.

### *6.1.2: The scale of analysis, paradigms, and changing theories of the Palaeolithic*

#### *The scale of analysis*

On a very general level, most of the classifications and chronologies mentioned in the preceding chapters followed a similar trajectory. They started off as small-scale conceptions; most were developed initially to explain a pattern in a small geographical region. As these localised theories rose in popularity, the associated terminologies and assumptions became imposed upon palaeolithic sequences from more distant regions. A common criticism of once-popular theories was that such schemes had been applied beyond the bounds of the data-set or geographical locality upon which they had originally been founded. This pattern can be seen in the palaeontological classifications of Lartet (1861), the industrial classifications of de Mortillet (1883; 1900), and the aspersions thrown back and forth between James Geikie (glacial geology) and William Boyd Dawkins (palaeontology) discussed in Chapter Two. The scheme of parallel cultures developed by Breuil seems to have followed a similar course.

#### *Kuhn's theory of scientific revolutions*

It is interesting to look a little further behind such general patterns and ask how such schemes gained and maintained popularity, and the reasons for their decline. On a very general level, Kuhn's theory of scientific revolutions seems to provide a neat explanation for the rise and fall of prominent general theories over the decades of research covered in the preceding chapters.

Kuhn (1996) uses the notion of a 'paradigm' (i.e. previous scientific achievements which define the problems to be explored by a scientific community and the methods used to explore them) to explain how a group of researchers, who could not necessarily articulate any hypothetical rules underpinning their research, could tackle a coherent range of problems successfully. Expectations drove researchers to suppress novelties or align them with their expectations in defence of these prior assumptions (Kuhn 1996, 5-6). At first, there was little awareness of anomalies which remained unseen since they did not fit expectations (Kuhn 1996, 64). With increased exposure to anomalies, awareness increased, and conceptual

categories then became adjusted to make what was anomalous become part of future expectation (Kuhn 1996, 64).

It was not just the discovery of anomalies that might lead to such a 'paradigm shift', since discrepancies could, after all, be modified to fit the existing paradigm; in addition, an alternative, better-fitting paradigm had to be available to take the place of the old one (Kuhn 1996, 77-78). Kuhn also notes how new theories were generally preceded by a period of professional insecurity, resulting from the failure of the existing paradigm to account for observations, and describes how this led to a search for new rules (*i.e.* methodology and the kinds of problems addressed); while the paradigm was accepted, there was rarely any concern over rules (Kuhn 1996, 47, 68).

When taken in combination, the general assumptions described above, the question of the scale of interpretation and Kuhn's theory of scientific revolutions together provide a good picture of how assumptions and expectations of the British Early Palaeolithic record developed, and how these influenced the interpretations and arguments that were put forward over the decades. A brief summary of these broader patterns of British Palaeolithic research will now be offered in answer to the first half of the question posed in Chapter One.

#### *Changing expectations of the British Palaeolithic as paradigm shifts?*

The theories, type-sequences and expectations which culminated in Commont's Somme Valley sequence of the early 1910s and those which became associated with Breuil's more audacious scheme of the 1930s are treated in the manner of Kuhn's 'paradigms' in the following discussion. Discoveries in other areas were frequently accommodated to fit expectations derived from these high-profile schemes, many researchers being blind to anomalies that did not match the language and expectations of the moment (Kuhn 1996, 64).

Taking Commont's scheme first, the expectations raised by this particular interpretation of Palaeolithic patterning might well be described as an important paradigm for early twentieth-century British Palaeolithic research. Commont drew on the widespread assumptions outlined above in developing his Somme Valley type-

sequence in which the hand-axe had an important place as a chronological and cultural indicator. His conclusions had a strong influence on interpretations of the British sequence, particularly on the famous type-sequence of the Thames Valley developed by Smith and Dewey, described in Chapter Four.

In addition to Commont's type-sequence, many researchers were drawing on broader expectations inherited from the theories of the nineteenth century discussed in Chapter Two – the concept of a single progressive industrial sequence – and many took Commont's industrial sequence as confirmation of such expectations. These expectations and Commont's useful type-sequence acted as theoretical and methodological blinkers, and researchers selectively observed, described and interpreted the palaeolithic record according to such expectations. Descriptions of 'anomalies', such as non-hand-axe, flake-rich Early Palaeolithic flake industries like the Clactonian, emerged when the expectations derived from the dominant paradigm were beginning to be questioned. Whilst some continued to adapt their observations to fit the paradigm, others attempted to adapt the paradigm to fit their observations.

Smith and Dewey, for example, reacted with surprise to the flake-rich industry from Swanscombe which lacked the usual diagnostic Early Palaeolithic type-fossils (Smith and Dewey 1913, 182). They nonetheless attempted to describe and interpret it within the dominant unilinear framework. Moving beyond Commont's sequence and the classic research area of northern France, they turned to the flake-rich industries of Belgium for an analogy. Like Rutot, they decided that this non-hand-axe industry must precede the Chellean, and saw it as an early stage on a single industrial line. In the absence of distinctive diagnostic implement types such as the hand-axe, they tentatively drew attention to the Strépy nodules to classify the industry. These 'artefacts' approached their expectations of what possible precursors to the Chellean hand-axe might look like (Smith and Dewey 1913, 183; Dewey and Smith 1914, 93).

Warren was also initially puzzled as to the affinities of his industry from Clacton-on-Sea. However, unlike Smith and Dewey, he suggested this was a non-hand-axe industry: contemporary with the Acheulian, but occupying a separate line. In the 1920s, his suggestions often met with bemusement or misunderstanding on the part of peers who were versed in expectations of a progressive unilinear sequence

(Warren 1912a, 15; 1922a, 597; Johnson, cited in Anon 1923, 56; Sollas, cited in Warren 1923d, 634; Sollas 1924, 157). Such industries did not fit well with the general conception of the Early Palaeolithic and, when identified, were frequently perceived as anomalies, even into the late 1920s.

Through the 1910s, it seems that exposure to such 'anomalous' Early Palaeolithic non-hand-axe industries was increasing. Various researchers suggested alternative, better fitting paradigms, adjusting the old scheme to incorporate such anomalies and make them part of future expectation (see Kuhn 1996, 64). The separation of the flake-rich industries from the hand-axe industries, with a progressive sequence of industries running through both lines, was an obvious solution to the problem. It is no surprise that a number of researchers developed this idea before Breuil's more famous version described in Chapter Five. A trend did not necessarily begin with the first articulation of a theory; the atmosphere also had to ripen for acceptance. Some of the ways in which a conducive atmosphere might be nurtured will be discussed in more detail in section 6.2.

Early suggestions of an alternating or parallel line of flake industries were made by Obermaier (1906b; 1908; 1919) and Warren (1922a, 599), while Moir even managed to describe two branching lines for his expanding number of eolithic industries, both emerging from the Kent eoliths. One was characterised by flakes and progressed towards the Mousterian; the other by hand-axe precursors such as the rostro-carinate, culminating in the true palaeolithic hand-axes (Moir 1919, 48, Fig. 7; Moir 1924a, 15). Another attempt at modification of the old scheme was the expansion of the chronological implications of the term 'Mousterian' by British researchers, who referred vaguely in the late 1920s and early 1930s to an 'Early Mousterian', when speaking of pre-cave Mousterian industries. However, these efforts were soon overshadowed by Breuil's version of parallel industrial cultures.

Breuil's scheme arrived at a period of some confusion over the chronological positioning and relationship between flake and hand-axe industries. Palaeolithic researchers were faced with the problem of how to adapt their old expectations, classifications and interpretations to encompass the new variety of contemporaneous industries. Various geologists had used the chronological indications of the old

unilinear sequence to assist their correlations of different Quaternary deposits. The existence of *contemporaneous* type-fossils therefore raised unease over such practices. Thus Peake (1930, 383) attacked Boswell's use of the industrial chronology (see Chapter Five). Breuil's theory of parallel industries supplied a timely answer to these problems and was rapidly accepted. His scheme was associated with a detailed new type-sequence (the Breuil-Koslowski framework) which invested the industries with greater accuracy as chronological markers. Breuil was also a prominent and respected figure and he had built his scheme on the foundation of the famous type-sequence developed by Commont, all of which contributed to the success of his scheme.

Most researchers now accommodated their observations and interpretations to expectations derived from this new paradigm. Breuil's scheme brought new methodological constraints that required the modification of some old ideas. The most evident shift was an emphasis of distinction between a flake-rich, non-hand-axe line and a line characterised by hand-axe industries. The flake line displayed progressive improvement in industrial technology, and attention was focused upon characteristics of flaking and analysis of cores; the hand-axe line was dominated by distinctive type-fossils. Earlier efforts to identify distinctive retouched types in flake-rich industries (characteristic of a time when such industries were being incorporated within the single unilinear line of the preceding paradigm) now faded. Warren's continued defence of distinctive tool-types received little support, as these tended to blur an important boundary between the flake and hand-axe lines. Warren's biface core *implements*, for example, were commonly regarded as anomalies within the Clactonian flake industry. They became generally described as *cores* to fit the neat pattern and internal logic of Breuil's scheme.

The old links between industries and races were also adapted to this new pattern. Interpretations tended to separate the ancestors of the Neanderthals from our own, hand-axe-making forebears. The flake-industries from central and eastern Europe, further east of the traditional core-area of western Europe, were now assigned to a distinct, but contemporary, race hailing from a different geographical source area (Obermaier 1919, 146-147). Breuil took up the idea of the movement of these two cultural groups in and out of northern France and southern England to explain the

chronological overlap between distinct industries on a general European scale. The concept of culture-contact between these two lines, or between their various branches, was employed to explain more localised industrial variety that could not be encompassed by the broader classifications then in vogue.

Breuil's scheme did not permeate Palaeolithic research only; it also coloured interpretations of Quaternary geology. This is evident from the manipulation of the East Anglian and Thames glacial associations described in Chapter Five. The circular process of geological-industrial correlation sustained expectations based on Breuil's progressive sequence of time-specific industrial sub-stages, each with its characteristic type-fossils or techniques. In the absence of absolute dating, this process acted as what John McNabb (1996, 39, 47) has aptly described as a conceptual lock on interpretation, and led to what Robin Dennell (1990, 550) has characterised as the age of the 'Big Sequence', where local details were subsumed under a generalised picture of progressive industrial evolution. The kinds of interpretations which could be put forward were severely restricted by the constraints of such famous, overextended industrial type-sequences, and the use of these type-fossils in developing wider chronologies and assisting correlations. Culture and chronology had become closely interlinked, and the idea of contemporaneous industrial variety sat uneasily alongside the concept of generalised industrial progress.

Dennell (1990) has given a convincing account of how the evolutionary assumptions of the 1930s, which Breuil brought to the British Palaeolithic, were linked to broader social and political factors, and he traces the downfall of Breuil's 'Big Sequence' in part to the advent of absolute dating techniques (Dennell 1990, 555). However, when Breuil's ideas emerged, they were not particularly unusual for their time. Others, too, had come up with the idea of parallel industries. This was, after all, a fairly logical outgrowth of longstanding assumptions about industrial patterning and the problems posed by anomalous flake industries. Turning to the downfall of Breuil's scheme, heavy criticisms were starting to fall in the late 1940s and early 1950s, before absolute dating techniques became prominent. Breuil's version of Palaeolithic patterning seems to have ended on a similar note to previous theories, with the suggestion that it had been over-expanded from regionally specific sequences. His industrial chronology seemed unreliable over a broad scale and it was

this, rather than the presence of an acceptable alternative, that spelt its end.

The explicit attempts made by T.T. Paterson in the early 1940s to adapt the old terminology to both the local *and* the global scale provide a clear indication that Breuil's scheme was becoming overstretched. Paterson requisitioned the old cultural terms of Acheul and Clacton for two world-wide flake and hand-axe families, employing the idea of cultural hybridisation between the two to encompass any nuances of industrial variation, and invented new 'cultural' names for the now-debased local industrial terminology, such as the 'Brecklandian' (Paterson 1940-41, 379). However, Paterson's idea of flake and hand-axe families was attacked with even more fervour as another over-extended generalisation. The ethnographic concept of culturally-significant behaviour on a regional scale had become debased. By the late 1940s and early 1950s, many considered that interpretations of Palaeolithic patterning had become subsumed under the task of classification.

Looking back to when Breuil's theories and his type-sequence had been rising in popularity, they had soon become employed to describe and interpret industrial patterning in areas increasingly distant from the region where this scheme had originally been formulated. Anomalies then became more apparent, and some researchers began, once more, to cast about for a different version of, or an alternative approach to, the Palaeolithic record. In other words, the way was open for a paradigm shift. Researchers working in Africa and Asia found it difficult to accommodate their finds to the concept of twin industrial streams. Nearer the classic sequence, anomalies in the more detailed industrial-chronological framework had been identified at sites like Baker's Hole. Once again, uncertainty was growing, not only amongst Palaeolithic researchers but also within the ranks of Quaternary geologists who had relied on this industrial chronology. Breuil's theories seemed over-generalised, and his key sequence appeared to be relevant only to a regionally-specific area.

In the ensuing uncertainty, many researchers expressed a similar desire to move away from the constraints of chronology and typology and towards more ecological and anthropological perspectives that would permit regionally-varied and humanistic interpretations of the past. The atmosphere was ripe for a new paradigm to be accepted. A swift glance beyond the scope of this discussion suggests that the

suggestions put forward by François Bordes (1950b; Bordes and Bourgon 1951) and later by Louis Binford (1962) would become closely associated with the next phase. It is interesting that neither had a close connection to British Palaeolithic research, where the impact of Breuil's scheme had been felt most strongly.

From this overview, it appears that that the observation of, classification of, and interpretation of palaeolithic industrial patterning were intricately interlinked and heavily dependent on prior expectations. On a very general level, these expectations were driven by the idea of progress, and by the fact that the perception of the Palaeolithic arose in western Europe where the hand-axe was the most distinctive type-fossil. Certain groups of expectations were drawn from these general assumptions and achieved popularity for a decade or so, emerging and declining in a complex manner which permits their designation as general theories, trends of research, or 'paradigms'. These comprised groups of expectations that were generated by and helped to reinforce the industrial classifications and methodologies of the moment, they were often linked to famous type-sequences and to the name of a prominent individual, and the expectations put forward by these paradigms also enjoyed a symbiotic relationship with Quaternary chronologies and theories of cultural and biological patterning. A complex interdependence between interpretations associated with various different research areas is apparent even on this very general level of analysis.

Some of the reasons behind the emergence, temporary survival and eventual decline of these theories have been supplied by Kuhn's model of scientific revolutions. However, the question of the scale of interpretation was central to these patterns of expectations and to the changing character of arguments. Interpretations differed with the region, research focus and scale of analysis, differences that often inspired doubt about the effectiveness of the current paradigm in explaining industrial patterning.

#### *Back to the question of scale*

The different scales of analysis evident in these past patterns of research demand a little more critical consideration. Current theories of science and past theories of

Palaeolithic patterning both suffer from a similar conflict: on the one side there is the desire to explain generalised patterns and develop general theories; on the other, there is the threat of criticism on the grounds that these interpretations do not provide sufficient explanation for specific local idiosyncrasies. It seems impossible to draw a conclusion that will satisfy both the larger pattern, such as that described in the summary above, and the smaller scale of varied local interpretations. We may at least take a quick glance at expectations and interpretations at the other end of the scale to clarify a few more of the discrepancies and details raised in the preceding chapters.

It is worth making the rather obvious point that not all researchers agreed with the prominent theories described above as 'paradigms'. The interpretations of the Clactonian offered by Warren, for example, were initially directed towards a relatively small scale of analysis. For Breuil, on the other hand, the Clactonian formed part of a much broader theory of industrial patterning. Warren defended his interpretation of a flake-rich Clactonian with distinctive core-tool type-fossils (such as bifaces and side-choppers) at a time when others were taking up the increasingly popular, but incompatible, concept of a generalised distinction between a flake line defined on the basis of technique and a hand-axe line with distinctive type-fossils. The question of whether these were cores or choppers is not important in itself. This was merely the point of articulation of a more general conflict which was fuelled by a lack of empathy between two sides working on different scales of analysis with consequent differences in aims and assumptions.

This lack of engagement between researchers who were apparently arguing over precisely the same specimens brings to mind the image presented in Chapter Three of the differences in initial starting-point of those who accepted the eoliths and the flint-fracture experts who opposed them. Those who supported an Eolithic period emphasised the 'archaeological' features of the eoliths. However, the flint-fracture experts were working from the assumption that these features were natural, and drew attention to the presence of fracture characteristics that they thought were attributable to natural forces. Neither side engaged fully with the other, the arguments continued and, in such circumstances, discussion could not resolve such differences.

Even in cases where researchers seemed to be in agreement over the dominant paradigm, groups with different connections to palaeolithic research would each have a different vision of this paradigm (see Kuhn 1996, 50-51), based on their own inherited series of problems to solve and on differences in their research focus (be that in subject matter, locality, or scale of analysis). They may only have engaged with the paradigm on a very superficial level: in the use of the same general terminology, for example. Only occasionally would the mismatches be so apparent as to cause major disagreement, as in the Clactonian biface question. It is therefore no surprise that the attacks on Breuil's theories of the 1940s and 1950s stemmed from a variety of reasons and emerged from several different research areas, although they were generally subsumed within the criticism that the scheme had been over-extended.

Stepping down another level from small-scale research groups to the viewpoint of the individual, it is self-evident that even if researchers enthusiastically accepted a popular theory, such as Breuil's scheme in the 1930s, they still would have perceived and explained Palaeolithic data through their own differing perceptions of this scheme. Personal character and motivation might stimulate completely different reactions to the popular views of the time. Certain theories were so familiar and widely accepted that they were rarely questioned. However, others may have had more of a personal interest in seeking out problems with the traditional views of the Palaeolithic: young researchers with a name to make, for example, or the desire to counter the theories of a rival researcher. It is intriguing to speculate how many researchers referred to the major theories of the moment quite self-consciously, simply because everyone else was following them and not to do so might mean that one's own interpretations would 'not be taken seriously'. A reference to popular papers of the time could also enhance the standing of an argument, and might therefore be offered on the mildest excuse. For a recent illustration of this effect, take a close look at the context and value of many of the briefer recent references to the work of Michel Foucault over the past decade, particularly in the introductions to Ph.D. theses.

In concluding this section, it must be emphasised that although the spectrum of past research seems to describe a pattern of different successive research trends, on a more detailed level of analysis these become more scattered and appear as a

spectrum of more diverse contemporary approaches. All these levels of interpretation can provide helpful representations of the past, but the intended scale of analysis must be made clear.

## **6.2: The context of interpretations and the character of debate**

This section examines the way in which ideas were developed and articulated in the competitive arena of day-to-day research in order to answer the second part of the question outlined at the start of this chapter. To reiterate, it was asked how the varied social and institutional backgrounds and different respective research agendas influenced the kinds of arguments and interactions that lay behind these interpretations of the British Early Palaeolithic record. The consideration of some of the more specific and idiosyncratic details upon which Palaeolithic research was ultimately founded also provides an interesting counterpoint to the conclusions drawn above on the general patterning of expectations and interpretations.

It is evident from the discussions in preceding chapters that interpretations of the British Early Palaeolithic often relied upon the negotiations of small groups of researchers, and that the prominence of their interpretations was partly dependent upon their ability to make a persuasive case to an influential audience. Three aspects which have recurred through these chapters provide a focus for the following conclusions. First, the daily activity of research was not just restricted by expectations, but also by more mundane factors such as pressures of employment. Second, interpretations were conceived in day-to-day interaction between researchers with different research agendas. Third, a generally acceptable interpretation relied on more than the quality or content of academic work. Proponents had first to convince the wider research community of its viability and much support could be gained from a careful selection of where, how, and to whom the argument was presented. Together, such restrictions, negotiations and social tactics had a tremendous influence on the character and direction of arguments and interpretations of the British Palaeolithic.

### ***6.2.1: Research opportunities: pressures of employment and access to information***

The process of gathering, sharing and interpreting information related to Palaeolithic research was influenced by prior expectations, but was also subject to constraints of a more personal nature which could restrict access to information about the Palaeolithic. A primary requirement for research was access to Palaeolithic sites. The importance

of the locality of the researcher, the whereabouts of recent exposures of Palaeolithic sites, and the ability of the researcher to visit such exposures have been discussed in detail by Rob Hosfield (1999). However, the nature of employment also had an immense influence on the kind of research that could be undertaken by a potential Palaeolithic researcher with convenient access to such sites.

We have seen in Chapter Four how Reid Moir was oppressed by expectations connected to the family business of tailoring and outfitting, and his desire for employment related to the study of prehistory. Many were in a similar position, fighting for the time to indulge an interest in palaeolithic research. Even the minority who had managed to gain the few posts offering some form of related employment were dependent on the flexibility of their job and the sympathy of their employers to undertake research in their work hours. Henry Dewey's collaboration with Reginald Smith on the Thames Valley sites, for example, was finally ended by the disapproval of his superiors, and others were hemmed in by administrative affairs that kept them from the field, the library or discussions on the subject with research colleagues.

Alternatively, access to a particularly rich, prominent or famous site could add enormously to personal prestige and the perceived value of interpretations. The names of Smith and Dewey, for example, rose to fame with the Swanscombe sequence; Moir became prominent in his local society with his Pre-Palaeolithic Crag and boulder clay sequences; and Breuil's Early Palaeolithic theories were boosted by the fact that his research was based on the Somme sequence. In addition to sites, access to type collections, books, or knowledgeable colleagues was indispensable in formulating palaeolithic interpretations: those with wealth or social standing, or who lived in a convenient location for access to such resources, were greatly advantaged in this respect.

### ***6.2.2: Inter and intra-disciplinary dynamics, expectations and interpretations***

A researcher with the time to study and the luck to make some discoveries was then faced with the task of deciding the context and relative date of the artefacts. The kind of reconstructions of industrial patterning that would be suggested might have been influenced by a variety of factors. On a very broad level, expectations were driven by

prominent theories and famous type-sequences, as discussed in the preceding section. However, those interested in Palaeolithic research would also have drawn upon an array of different research areas. These might include glacial geology, the geology of Quaternary rivers, palaeontology or anthropology. Indeed, an interest in the Palaeolithic may well have been stimulated by prior expertise in one of these research areas. Each of these areas might inspire a slightly different perspective on a local sequence. In addition, each nurtured its own distinct research agendas and internal conflicts, and Palaeolithic discoveries might therefore be used to attack or defend such positions. The consequent interpretations of the Palaeolithic could therefore be coloured by a stance taken on another area of Quaternary research, and vice versa.

Some of the variety in approaches to the British Early Palaeolithic can be traced to the decades in the mid-nineteenth century when this subject was first emerging as a field of study in its own right. The earliest Palaeolithic researchers lacked a set of ready-made intellectual traditions and institutions for the interpretation of palaeolithic artefacts within their Quaternary context. As described in Chapter Two, researchers emerged with expertise drawn from various different branches of research, and this knowledge was therefore requisitioned in an *ad hoc* manner to supply the deficit and provide information, methodologies, analogies, as well as societies and institutions where Palaeolithic and related subjects could be discussed. This was the source of the unusual variety nurtured within Palaeolithic research, a variety which would stimulate discussion and direct future interpretations.

We have seen in Chapter Three, for example, how James Geikie, who supported the idea of interglacial man at a time when many considered the Palaeolithic to be post-glacial, was also amenable to a more ancient appearance of tool-makers in Britain, and was therefore sympathetic towards Harrison's eolithic discoveries. In Chapter Five, it also became apparent that Kenneth Oakley's belief in the Clactonian I-IIA-IIB-III progression influenced his reading of the geological succession in the Thames Valley. Indeed, it has been suggested in the same chapter that the widespread popularity of Breuil's version of the parallel-culture concept in Britain was boosted by the value of the finer industrial subdivisions of the Breuil-Koslowski framework as a fine-grained chronology for Quaternary geologists. The discrete fields of archaeology, geology, palaeontology and anthropology break down

on the more minute levels of specific arguments, and of personal aims and expertise. Once again, the terms of discussion and the character of conclusions differ with the scale of analysis. The dynamics involved between researchers working on these various connected fields of research were very complex, and their interpretations were intricately linked together.

One of the most interesting patterns evident from the preceding chapters is the way in which informal collaborations developed between researchers working on different aspects of research. They could bring diverse sources of expertise to a Palaeolithic problem based on knowledge of a specific field of research or site locality, and might draw quite different benefits from such alliances. Most researchers would have general knowledge drawn from a range of areas, but would have been valued themselves and would have respected others for their expertise in certain more specific areas (see Rudwick 1985 for a discussion of a similar dynamic in nineteenth-century geology).

Certain groups stand out from previous chapters for their collaborative interpretations of the British Early Palaeolithic. Their specific area of expertise has been suggested below in brackets (although it must be observed that their overall knowledge and the character of their private discussions would have been less respectful of such convenient disciplinary boundaries). For example, an early team comprised John Evans (archaeology), Joseph Prestwich (geology) and Hugh Falconer (palaeontology), whose discussions and public presentations set the scene for many of the early expectations of the British Palaeolithic. Turning to the eolith debates, Joseph Prestwich (geology) famously teamed up with Benjamin Harrison (local knowledge of the archaeology). The work of Reginald Smith (archaeology) and Henry Dewey (geology) became renowned for their Thames Valley researches in the early twentieth century. James Reid Moir (archaeology) and Percy Boswell (geology) were a prominent team working on the East Anglian sequence, particularly in the 1920s and 1930s. Between them, they developed a well-known and widely accepted industrial-glacial sequence. Hazzledine Warren (archaeology, geology, and local knowledge) often worked with Martin Hinton and A. Santer Kennard (palaeontology, geology) in assessing the position of his discoveries at Clacton-on-Sea within a broader Quaternary picture. Personal likes and dislikes also entered the equation. For example,

Smith was in close contact with Moir, whereas Warren had differences with Moir over the coliths, and Smith was therefore loath to talk to Warren (Kendrick 1971, 5).

Looking in more detail at the conclusions reached by such teams, these were not simply the result of a disinterested amalgamation of information from different but compatible research areas. Each field and sub-field of research fostered its own uncertainties, expectations, and a variety of internal debates. Amongst the range of possibilities on offer from the various areas of expertise, some solutions would be closer to personal expectations than others and these would generally be selected, as they could provide support for a stance taken on such a debate. These (generally unofficial) partnerships arose as researchers discovered colleagues with different areas of expertise but with expectations not opposed to their own. Following private discussion over how best to reshuffle the various lines of evidence to their mutual satisfaction, a relatively seamless conclusion could be presented in public, protected from attack by multiple, interconnected, and mutually supportive lines of evidence. Such small-scale, self-supporting scientific networks were not necessarily part of a conscious plan, but emerged organically from genuine beliefs and expectations about Palaeolithic and broader Quaternary patterns.

### ***6.2.3: Winning support: selection of an arena and audience for interpretations***

These interpretations, gathered in the light of the restrictions described above, and interpreted according to individual motivation and group negotiation still had to face a final hurdle before they could win widespread acceptance. A wider audience had to be convinced of the veracity of such conclusions. The simplest way to gain the ear of a large audience was by presentation at regular gatherings such as society meetings and through publication in books or in society journals. A less direct route was to convince a prominent and respected individual whose standing was such that others would follow.

Taking the delivery of papers at societies first, a suitable arena had to be selected from the large array of local and learned societies where the Palaeolithic formed a topic of discussion. Such a variety of societies reflects the variety of research areas that were drawn upon in discussion over the Palaeolithic, and the

selection of an arena might be based upon the angle of the argument, the societies most familiar to the individual, and also the prestige of the society. A more prestigious arena might reflect equally prominent approbation for the viewpoint expressed in a paper delivered under its auspices. This is evident from the tactics and reception of Ray Lankester's Royal Society papers on the pre-palaeoliths, discussed in Chapter Three. However, membership to the more exclusive societies was dependent upon the support of socially prominent friends or colleagues, and the financial means to pay the requisite fees. The most exclusive learned societies were only frequented by a small minority of Palaeolithic researchers.

Once an interpretation had gained the acceptance of a few prestigious supporters, this might well enhance the standing of that interpretation. The change in opinion regarding the pre-palaeoliths following Breuil's conversion in 1920, for instance, provides a superb example (Osborn 1922, 440; Warren 1922b, 87). However, returning to a theme discussed earlier, the failure of an earlier effort by Moir and Lankester in 1912 to convert Boule and Breuil demonstrates the power of expectation over observation. Their failure was probably exacerbated by a clash of personalities, a reminder that such forgotten but tiny details often influenced the course of research.

#### ***6.2.4: In summary***

The general patterns of expectation and interpretation discussed in the previous section have now been overlaid by a finer mesh of more specific and personal details which also influenced readings of the British Early Palaeolithic. Personal circumstances such as leisure, location and wealth dictated the ease with which research could be carried out. Palaeolithic interpretations varied with motivation and research agendas, and these were often generated through the complex research dynamics of small-scale symbiotic alliances. Each researcher could strengthen personal lines of argument, and in the process build a strong defence of an overall picture of the Palaeolithic from interdependent lines of arguments. If wealth, location, personality and social standing permitted, these conclusions could be presented to more prestigious societies, and might glisten a little brighter with reflected glory. However, notwithstanding the careful selection of an advantageous and persuasive

arena, proponents of a new idea still had to confront preconceptions and expectations held by their audience that might preclude acceptance of their viewpoint.

Even if researchers lived in the centre of this hubbub of research, and had leisure, wealth, social standing and access to Palaeolithic sites, a great variety of other aspects might also disrupt the formulation, and obstruct the acceptance, of such interpretations. There were personal dislikes, cliques, power games, and poor reviews. There were also chance disruptions, such as the Great War, which hastened Commont's death, and ended with Obermaier far from the rich and famous Early Palaeolithic sequences of northern France; or the Second World War, when Breuil took up the South African sequence but loosened his personal hold over interpretations of the British sequence. Finally, the successful promotion of a certain view of the past did not necessarily add to the accuracy of the Palaeolithic picture. The pre-palaeoliths are a case in point, and Moir and Boswell's picture of East Anglian industrial-glacial observations, which had such a great influence on reconstructions of the British sequence in the 1920s and 1930s, has now been largely dismissed.

### 6.3: Conclusions

To conclude this exploration of the driving forces which lay behind the interpretations of Early Palaeolithic stone tools offered between the 1880s and the 1950s, we must jump between several different scales of analysis. Prominent theories, of the kind described above as 'paradigms', seem to have risen and fallen in a series of cycles through the decades covered in this thesis, each made up of neatly fitting and logically consistent clusters of arguments. Such theories were built upon and conformed to general assumptions which had been moulded by a long familiarity with the industrial patterning of western Europe. They were also influenced by expectations derived from other fields of Quaternary research, particularly the various relative chronologies. Observation and expectation were intricately linked: observations were twisted in line with expectations, and the theories that set out such expectations shifted in line with observations. Every few decades witnessed a slightly different set of expectations associated with a new paradigm, or general theory, and the anomalies of one era (such as Warren's side-choppers) might well become incorporated within the expectations of the next.

To understand these changes in greater detail, one has to drop down to another level. Only a minority of workers aimed to produce such general theories of palaeolithic industrial patterning, and most researchers were busy on the development and explanation of local Palaeolithic sequences. They would have been influenced by the popular paradigms of the time, whether their response took the form of an implicit acceptance, a more self-conscious adaptation of popular views or a direct reaction against them. Interpretations were also moulded by the negotiations which took place within small-scale research networks composed of a few colleagues with expertise in different but compatible areas. Local sequences, like the general patterns described above, also involved the creation of logically-consistent clusters of arguments. Researchers from these different areas of Quaternary research would twist local conclusions, either to fit their own series of expectations, or to assist counter-attacks on opponents. Turning back again to the more general theories of the time, this variety of views on the local scale might well have contributed to a sense of unease with the currently prominent paradigm, perhaps supplying the germs from which future paradigms could grow.

An enormous variety of interpretations was offered to explain the industrial patterning of the British Early Palaeolithic record between the 1880s and the 1950s. There do seem to be some clear patterns behind the way in which these changing and often contradictory interpretations were developed, offered, and received. Interpretations were based upon a complicated knot of knowledge, in which expectations informed observations, and observations reinforced those expectations. A whole range of personal idiosyncrasies also contributed to the character of these interpretations, including the style and manner of dissemination, and the attributed weight given to these arguments by the wider research community. Once again, this involves an intricate loop of argument in which the personal position ascribed to a researcher within the social hierarchy of research could influence the weight given to their interpretations, and the interpretations (particularly if these were based upon dramatic new discoveries or on detailed or famous type-sequences) could, in turn, ease the path to a prominent position in the research hierarchy.

The character of expectations and interpretations of the British Early Palaeolithic was assembled from arguments founded upon many different scales, and which have now been followed down a series of twisting and often circular routes. Beneath the general explanations and reconstructions offered above lay a multiplicity of expectations of the British Early Palaeolithic. These were held by a varied army of researchers, often hard pressed for time, who were all engrossed in their own personal interests, concerns and expectations. Their local researches, general expectations, and a range of collaborations led to the making of the British Palaeolithic, in all its diverse incarnations, between the 1880s and the 1950s. It is fitting to conclude this exploration of the character of such research with a comment made by Warren in 1941:

‘a flint implement [...] is not a fact, but an inference’ (Warren 1941, 90).

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AMO Ashmolean Museum, Oxford  
BGS Library of the Geological Survey of Great Britain, Keyworth  
BLL British Library, London  
    JL John Lubbock papers (Add. 49638-81; 62679-93)  
    RL Ray Lankester papers (Add. 44968-44970)  
BLO Bodleian Library, Oxford  
    CH Christopher Hawkes papers (un-numbered at time of research)  
BMD Buxton Museum, Derbyshire  
    WBD William Boyd Dawkins papers  
    JWJ J. Jackson papers  
BM(F) British Museum (Franks), London  
    WAS W. Allen Sturge papers  
BM(NH) British Museum (Natural History), London  
    WLA W.J. Lewis Abbott papers  
    KPO Kenneth Oakley papers (DF140/7)  
CCC Clare College, Cambridge  
    CL Charles Lyell papers  
FMF Falconer Museum, Forres, Grampian  
    HF Hugh Falconer papers. Numbers refer to Boylan's catalogue (Boylan 1977)  
GSL Archives of the Geological Society, London  
ICL Imperial College Library, London  
IAL Institute of Archaeology, London  
NLW National Library of Wales, Aberystwyth  
PRM Pitt-Rivers Museum, Oxford University  
    EBT E.B. Tylor papers  
SMC Sedgwick Museum, Cambridge University  
    TGB T.G. Bonney papers  
    TMH T. McKenny Hughes papers  
UCL University College London, Manuscripts Room  
ULC University Library, Cambridge  
    MB Miles Burkitt papers (Add 7959)  
ULE University Library, Edinburgh  
    CL Charles Lyell papers  
ULL University Library (Brotherton), Leeds  
ULLiv University Library, Liverpool  
    PGB Boswell papers  
UMO University Museum, Oxford  
    TGB T.G. Bonney papers  
    WJS William Sollas papers

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