Geology, archaeology, and ‘the raging vortex of the “eolith” controversy’

Anne O’Connor

O’CONNOR, A. 2003. Geology, archaeology, and ‘the raging vortex of the “eolith” controversy’. Proceedings of the Geologists’ Association, 114, 255–262. The ‘raging vortex of the “eolith” controversy’ (Sollas, 1911) once provided a focus for vibrant debate over the extent of human antiquity. These ancient chipped stones attracted a spectrum of approaches and opinions, from geologists and archaeologists, specialists and generalists, amateurs and professionals. The case for and against the human workmanship of these stones was not a purely archaeological matter, but was described by Samuel Hazzledine Warren (1940) of Loughton, Essex, as the 'Cinderella of Science', falling between the two stools of archaeology and geology. Amateur geologists, such as Warren, took a prominent position in the discussions of human and natural flaking; Warren became renowned for his observations and experiments on flint fracture. On the other side of the debate, collectors such as Benjamin Harrison, a shopkeeper from Ightham, Kent, discovered both eoliths and accepted palaeoliths on the North Downs. The different perspectives, theories and rhetoric that converged upon this issue illuminate some of the tensions that lie behind the multidisciplinary subject of palaeolithic archaeology.

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1. INTRODUCTION

The eoliths, aptly described by Bonney as ‘that geological “Mrs Harris”’—as mythical as the imaginings of a Dickens character (Bonney to Sollas, 2 June 1912, University College London MS. Add. 152), are now largely discredited. They are rarely spoken of in the archaeological departments of modern academia or in the field today. However, at one time these well-named ‘eoliths’ or ‘dawn stones’, their crude character whispering the possibility of a far more ancient human occupation of Europe, set the Palaeolithic world buzzing, and society meetings rang to the sound of furious debate. In the latter half of the nineteenth century and the first few decades of the twentieth, argument raged over the supposed human workmanship of these battered stones from Pliocene and earlier deposits. Some researchers were convinced that the eoliths were the work of ancient stone toolmakers and tried to bring them under the respectable umbrella of the accepted prehistoric stone tool sequence. Others wondered whether these had been naturally flaked by geological processes, with the amateur geologist Samuel Hazzledine Warren (1872–1958) prominent amongst those experimenting on flint fracture and observing natural fracture in the field. [Terminological note: Warren is described here as an ‘amateur’ in the sense that he received no regular income from his geological labours and had never undertaken any formal geological training, but pursued geology for the love of the subject.]

Spencer (1988, 1990) has addressed the wider implications of the eolith debates in some detail, outlining how arguments supporting the human workmanship of these ancient stones raised expectations of a longer human chronology, and how this prepared the ground for the acceptance of the notorious Piltdown forgery. This paper presents a brief overview of the social and disciplinary complexities that lay behind the debate. At this period, the boundaries between collectors, amateurs and professionals were fluid (Levine, 1986); it was a vibrant time for research and contributions were welcomed from a diverse range of individuals. These contributions were not restricted to archaeologists; high feelings were also aroused amongst geologists, inspiring some valuable research by amateur workers on both sides of the debate. Two areas have been selected for attention within a broadly chronological treatment of the subject: first, the status of flint-fraction research within the disciplines of archaeology and geology; some workers perceived this topic as a purely archaeological concern. Second, the manner in which traditional lines of Palaeolithic archaeological theory, themselves partly inherited from geology, were adapted and applied to eoliths.

2. BACKGROUND

The question of human antiquity had supposedly been resolved in the mid-nineteenth century when John...
Evans, Joseph Prestwich, John Lubbock, Charles Lyell and other stars of the geological and archaeological firmament followed Hugh Falconer to France and the Somme Valley sites of Boucher de Perthes (Riper, 1993). They noted the position of undoubted flint tools alongside the bones of extinct animals, found in situ within Pleistocene river gravels at a height that compelled their acceptance of an early date. By 1860, human antiquity had been proven: Palaeolithic archaeology could now develop as a field in its own right, but the extent of that antiquity was still an open question that would be tackled by this new discipline with gusto.

Soon after the establishment of human antiquity, John Lubbock (1834–1913) provided his colleagues with a useful chronological tool. He divided the prehistoric Stone Age into two Periods: the Palaeolithic period or Old Stone Age (which he initially called the ‘Archaeolithic’), characterized by chipped stone tools; and the Neolithic period or New Stone Age, when polished axes and domestic animals appeared (Lubbock, 1865, p. 60; 1869, p. 74). Gabriel de Mortillet (1873) supplied a further refinement of Lubbock’s Palaeolithic period with his stone tool epochs, each named after a typical locality and defined by typical tool-forms which acted like the zone-fossils of geologists. De Mortillet later added an entire new period that preceded the Palaeolithic: the controversial ‘Eolithique’, a term he coined in 1881 (Grayson, 1886).

Discoveries of eoliths in England, from the plateau in Kent or from beneath the Crag in East Anglia, were preceded by events in France. The eolith debate began on the continent with bone rather than flint when Jules Desnoyers (1863) reported cut-marks on bone from the Pliocene deposits of St Prest (Table 1). It was not long before even earlier deposits in France and Spain supposedly yielded ‘tools’, held up for inspection in successive International Anthropology Congresses where de Mortillet, although he did not accept all of them, used some as the basis for a succession of invented early hominids. A prominent anti-clericalist, de Mortillet named hominids with glee on the mildest provocation, but John Evans, another regular at these Congresses, was soon dubbed ‘the little St. Thomas’ for his doubts (Newton, 1898, p. 66). In general the British, so far as they took any notice of continental events, seem to have followed Evans’ scepticism and debate gradually died away.

### 3. THE KENT PLATEAU EOLITHS

Meanwhile, Benjamin Harrison (1837–1921), who ran the village shop at Ightham in Kent, and collected stone tools in his spare time, was quietly reviving the question of greater human antiquity. Though in correspondence with some of the most prestigious of his

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<thead>
<tr>
<th>Date</th>
<th>Researcher</th>
<th>Contribution</th>
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<tbody>
<tr>
<td>1858</td>
<td>H. Falconer</td>
<td>Suggested that Prestwich and Evans visit the palaeolithic collections of Boucher de Perthes in the Somme valley.</td>
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<tr>
<td>1863</td>
<td>J. Desnoyers</td>
<td>Reported ‘cut-marked’ bones of Pliocene age from St Prest (Desnoyers, 1863).</td>
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<tr>
<td>1865</td>
<td>J. Lubbock</td>
<td>‘Palaeolithic’ and ‘Neolithic’ divisions set out in the first edition of Pre-Historic Times (Lubbock, 1865).</td>
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<td>1873</td>
<td>G. de Mortillet</td>
<td>Provided a detailed outline of the Palaeolithic epochs (Mortillet, 1873).</td>
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<tr>
<td>1881</td>
<td>G. de Mortillet</td>
<td>Coined the term ‘Eolithique’ (Grayson, 1886).</td>
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<td>1886</td>
<td>B. Harrison</td>
<td>Accepted the Kent plateau eoliths as humanly worked (Harrison, 1928, p. 133).</td>
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<tr>
<td>1888</td>
<td>J. Prestwich</td>
<td>Accepted the Kent plateau eoliths as humanly worked (Harrison, 1928, p. 133).</td>
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<td>1889–1892</td>
<td>J. Prestwich</td>
<td>Publication of papers on the Kent Plateau eoliths by the Geological Society (Prestwich, 1889, 1891) and the Anthropological Institute (Prestwich, 1892).</td>
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<td>1889</td>
<td>A. Rutot</td>
<td>Began to support Belgian eoliths of early Quaternary age (Grayson, 1986).</td>
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<td>1905</td>
<td>M. Boule</td>
<td>Argued that Rutot’s eoliths were naturally fractured (Boule, 1905).</td>
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<td>1905</td>
<td>S. H. Warren</td>
<td>Published his first detailed attack on the eoliths of Kent and Belgium, describing natural fracture processes and experiments (Warren, 1905).</td>
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<td>1909–1910</td>
<td>J. R. Moir</td>
<td>Discovery of sub-Crag pre-palaeoliths in East Anglia (Moir, 1911).</td>
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<td>1910</td>
<td>H. Breuil</td>
<td>Attacked Rutot’s eoliths on the basis of in situ naturally fractured flints of Eocene age (Breuil, 1910).</td>
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<td>1912</td>
<td>E. R. Lankester</td>
<td>Attacked the use of the term ‘eolith’, replacing it with ‘pre-palaeolith’ (Lankester 1912a, b, c).</td>
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<td>1913</td>
<td>Members of the Geographical Society</td>
<td>Meeting devoted to the question of early palaeolithic tools, with a prominent place given to the pre-palaeolithic ‘implements’ of the East Anglian Crag (Anon., 1914).</td>
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<td>1914</td>
<td>F. N. Haward, S. H. Warren</td>
<td>Detailed criticism of Moir’s flint-fracture ‘experiments’ (Haward, 1914; Warren, 1914a, b).</td>
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contemporaries, Harrison was not a wealthy man. He could not afford to buy the latest books and, until Evans himself presented Harrison with a copy of his masterly *Ancient Stone Implements* (Evans, 1872), Harrison had been borrowing this indispensable work from his friend Worthington Smith (Harrison, 1928, pp. 82, 94; see Roe, 1981 for more on their friendship). A near neighbour, Joseph Prestwich (1812–1896), who had started off as a wine-merchant, became one of the greatest early experts on Pleistocene gravels and, by the time he met Harrison in the summer of 1879, was Professor of Geology at Oxford (Prestwich, 1899). This meeting was a pivotal moment, for when Prestwich gestured towards the nearby valley features in explanation of where the equivalents of the high-level Somme gravels lay, Harrison then realized that his finds (Fig. 1) from still higher levels suggested an even greater antiquity (Harrison, 1928, p. 84). At this time, the oldest generally accepted Palaeolithic implements came from the highest terraces of the Somme and the Thames, but with Prestwich as friend, guide and mentor, Harrison now hunted out his high-level implements across the surface of the Downs, from within temporary exposures and upon roadside heaps of flint. His labours resulted in the publication of three important papers by Prestwich in 1889, 1891 and 1892 that were to set the tone for the British side of the eolith debate. Although Harrison’s published contribution was restricted to a note on the implements appended to the 1892 paper, he was by no means averse to Prestwich’s authorship. On the one hand, Prestwich was widely renowned for his geological abilities and this would strengthen their case. But on a more personal level, Harrison suffered from deafness, a disability that made him self-conscious – he even avoided applying for membership of any of the learned societies, some of which were, in any case, prohibitively expensive for him. Although Harrison sat by Prestwich’s side when the first paper was read, he did not catch a word (Harrison, 1928, p. 143).

It was to the Geological Society of London that the first two papers were delivered (Prestwich, 1889, 1891). They both focused on the geological associations of Harrison’s ancient Palaeolithic tools, suggesting that the geological position in which they had been found supported a greater antiquity for the Palaeolithic period. It was only in 1888 that Prestwich was converted to full acceptance of the still older eoliths (Harrison, 1928, p. 133) and his third major paper, ‘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, 1892), was delivered in 1891 to an archaeological rather than a geological gathering at the Anthropological Institute. The discussions that followed each of these papers addressed the same issues that had formed the bedrock for the human antiquity case decades before (see, for example, Lubbock 1862). Some questioned the geological age of the deposits; others doubted the association between the early tools and the deposit in which they were found and considered the tools intrusive; but the key archaeological point revolved around the supposed human workmanship of the eoliths and was brought up, as usual, by that ‘doubting Thomas’, John Evans (Prestwich, 1892, p. 270).

4. GEOLOGY, ARCHAEOLOGY AND THE QUESTION OF HUMAN WORKMANSHIP

In the earlier years of the eolith debate, the task of distinguishing human from natural work was widely regarded as a purely archaeological one, geologists being the specialists who decided upon the antiquity of associated deposits (Evans, 1877/78, p. 150). However, though geologists would continue to be paraded-in for the geological part of archaeological papers, some, like Hazzledine Warren (Fig. 2), also applied geological principles directly to this archaeological question. They worked on a relatively recent, but expanding, field of research on the general principles of flint fracture (see Grayson, 1986, for a detailed study of its emergence and development).

By the end of the nineteenth century, the disagreements that had arisen from the Kent Plateau eolith dispute were joined by more heated arguments over Belgian eoliths from the sites of Mesvin, Reutel and Maffles, amongst others. These were championed by Aimé Rutot (1847–1933), curator at the Natural History Museum of Brussels, who was described by William Boyd Dawkins as the ‘chief exponent of the Eolithic cult, as it may be called, on the continent’ (Dawkins, 1910, p. 237). It was around this time that flint-fracture research moved forward apace. Three papers published during this period give some idea of the increasingly complex ways in which human work was distinguished from natural flaking in the race for ever more-convincing proof of the extent of human antiquity. Two of these appeared in the leading French...
archaeological journal, *l’Anthropologie*: Boule’s *L’Origine des Éolithes* in 1905, and Breuil’s *Sur la Présence d’Éoliths à la Base de l’Èocène Parisien* in 1910. Boule described how the rushing water and flint nodules caught up in a cement factory works at Guerville, near Mantes, produced similar products to Rutot’s supposedly humanly worked series of artefacts as a result of unguided, random fractures (Boule, 1905). Breuil announced the discovery of a site where natural flaking had been discovered *in situ* at the base of the Eocene deposits of Belle Assise. Some flakes were even found next to the block from which they had been detached, and Breuil described how tons of pressure from above could fracture flints where they lay, and even produce the bulbs of percussion and retouch that was thought to be characteristic of human work (Breuil, 1910). The third paper came out in the British equivalent to *l’Anthropologie*, the journal *Man*, and this was Warren’s 1905 attack on the Kent Eoliths: *On the Origin of “Eolithic” Flint by Natural Causes, Especially by the Foundering of Drifts*. Warren outlined six different processes by which eoliths may have been formed: human agency, various forms of water- and soil-abrasion, the drag of ice and weathering on the land surface (Warren, 1905, pp. 342–358). He 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). This paper was a major contribution to what could no longer be seen as a purely archaeological question.

Regardless of the great impact produced by these papers, the early twentieth century saw remarkably few contributions to the development of a sound knowledge of flint fracture. Warren blamed the lack of recruits on the peculiar position held by the study and application of natural flint-flaking agencies. He wrote to Kenneth Oakley at the Natural History Museum:

> As I see it, the situation repeats the ancient history of the *Homo diluvii testis* controversy. As in those days, when no one would get down to bed-rock & study anatomy, so now no one will ascertain the true facts of the dynamical geology of flaking.

The explanation of this anomalous situation in science is not far to seek. The study of the dynamical geology of flaking is, in itself, purely geological & mineralogical, whereas its practical import – its theoretical application – is as exclusively archaeological. Thus the vital problem (the reliable recognition of the human industry) upon which the major conclusions of prehistory depend falls between two stools, & no one will touch it. The result of which is that prehistory is building a superstructure of castles-in-Spain before it has got in a scientific foundation to build upon.

Until it comes to be realised that it is the very foundation of prehistory that needs impartial scientific enquiry, instead of being casually taken for granted, not much useful purpose is served by controversy over details.

There is a delightful incident in “Pickwick Papers” (Chap. XI. first half, & the conclusion) pulling the leg of the ever-credulous archaeologist, as he was a century ago – & as he still is today (The Natural History Museum Archives, DF140/7: Warren to Oakley, undated leaf of a letter, c.1938–1958).

It was the detailed research on flint fracture carried out by Warren and others, such as his friend and fellow amateur-worker, Frederick N. Haward (1871–1953), an engineer, which helped to condense and refine the criteria on offer for distinguishing the products of human manufacture from natural flaking. However, other contributions that lacked Warren’s rigour and geological understanding tended to confuse rather than clarify the issue. Warren recalled how flint-fracture studies ‘became the Cinderella of Science, casually cloaked in any rags and tatters of misleading half-knowledge’ (Warren, 1940, pp. 13–14). One creator of such half-knowledge who would have been uppermost in Warren’s mind was James Reid Moir (1879–1944), a gentleman’s outfitter, who on 3 October, 1909 discovered the British successors to the Kent eoliths in a brickfield stone-heap near Ipswich, tracing similar specimens to earth in the eroded basement bed of the Red Crag (Moir, 1911). Moir was encouraged to carry out some of his own
experiments to prove that Nature could not have simulated the design evident in these sub-Crag flints, and to counter the flint-fracture arguments of Warren and Haward. In his sack experiment, Moir put ten broken flints in a sack and shook them to simulate a sea beach or river agencies (Moir, 1911, 1912). Haward was incredulous, crying ‘Nature does not confine stones in a sack’, and adding that Moir’s dragging of a stone across a concrete floor with tongs was a far cry from the massive pressures of a glacier (Haward, 1914, pp. 347–348). Unlike Moir, Haward and Warren were not trying to reproduce natural conditions (Haward, 1914; Warren, 1913, 1914a). Their aim was to investigate the physical properties of flint and to train the eye to recognize the characteristics of chipping produced by different methods (see Figs 3 and 4). Many people failed to recognize this distinction between their aims and this led to mistrust (on both sides) of experimental research (Warren, 1914a, b). Haward hinted that the source of such misguided attempts to replicate nature was due to lack of ‘extensive geological experience’ and announced ‘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, p. 349).

5. THE ARGUMENTS USED BY LANKESTER AND MOIR

Moir may not have matched up to the scientific standards of Warren and Haward, but he and his influential supporter, the biologist Sir E. Ray Lankester, found other ways of persuading themselves and others that these flints were reputable. Brands of theory and rhetoric commonly associated with the interpretation of palaeolithic implements were carefully applied to the eoliths. This comes across most clearly in their approach to terminology, typology and the assumption of evolutionary progression in tool-making ability.

Moir and Lankester were swift to distance their ‘implements’ from the term ‘eolith’ with its now dubious and disreputable connotations (Lankester, 1912b, c). Moir’s specimens were not eoliths: he termed them ‘pre-palaeoliths’. In the Saturday Review for 16 March 1912, Lankester expressed great fury that William Sollas in his 1911 book, Ancient Hunters, not only used the term ‘eoliths’ but also applied it to Breuil’s natural flakings from Belle-Assise, declaring, ‘It is not the part of a man of science to sweep away all such evidence with the exclamation “Eoliths!”’ (Lankester, 1912a, pp. 333–334).

Another persuasive argument used by Moir and Lankester was to observe in these fractured stones the same features that had secured the Palaeolithic industries their place. Distinctive stone tools had long been treated as type-fossils – this was a recognized method of providing an industry with some form of identity. Lankester now introduced, named and described a new implement type from the sub-Crag: the rostro-carinate or eagle’s beak implement (Fig. 5) (Lankester, 1912c). He thought the supposed makers of these keel-shaped stones held a preconceived plan; a picture of what the finished tool should look like that was as distinct as that in the mind of any maker of an Acheulian hand-axe (compare Figs 5 and 6).

Lankester wrote 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 March 1913, Brit. Lib. Ms Add 44969/59-60).
Moir fully believed in the rostro-carinate, but he also took a different approach to the pre-palaeoliths – one which Lankester did not initially agree with (Lankester, 1912c, 1914). It was widely accepted that ancient stone-industries would evolve from cruder towards more finished forms. In line with such sentiments, Rutot, years earlier, had claimed evidence of progressive improvement in his Belgian series of ‘implements’ (Rutot, 1900, pp. 712–713). Moir now argued that the pre-palaeoliths also conformed and contributed to this progressively evolving vista of Palaeolithic and pre-Palaeolithic artefacts, an answer to attacks of those like Warren, who 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, p. 551).

Moir was working on a succession of deposits to the north of Ipswich where the Middle Glacial Gravel lay above the Red Crag and was, in turn, overlaid by the Chalky Boulder Clay. It was not long before he found ‘implements’ in each successive stratigraphic horizon, and he proceeded to divide the flints into a series that fully matched his expectations of progressively developing tool-working ability (Moir, 1913). Moir linked his pre-palaeoliths to the base of the established Palaeolithic sequence and was convinced that, in both age and form, the pre-palaeoliths successfully plugged the immense gap between the ancient eoliths of Kent and the more recent river-drift specimens – indeed, he argued that the previous absence of a convincing intermediate form was one of the main reasons why the Kent eoliths had failed to attract wider support (Moir, 1912). Moir was fond of remarking that the variety shown by these successive types from successive geological horizons would not be present had they been naturally formed, since Nature would flake in a uniform manner (Moir, 1913, p. 370). He took no notice of Warren’s point that Nature did not flake in a uniform manner but followed uniform principles.

However, Warren was also attacking Moir on another front and he seemed to reach into the heart of Moir’s case in his careful consideration of the power of selectivity: ‘It is so easy to argue in a circle to show that our series of flints indicates intelligence, and not to see that the intelligence has been put into them by ourselves, in our own selection out of the infinite variety of Nature’ (Warren, 1914b, p. 434). Haward reinforced these views, describing the process of selection from the labourer to the collector, to the large collector – each discarding less favoured specimens: ‘In the course of years this process of “selection” MUST result in an assembly of almost identical shapes’ (Haward, 1919, p. 136. italics in original).

This is a very interesting point. The process of selection had always formed an integral part of the practice of classification – the palaeolithic industrial sequence was founded on such activity. One could draw a parallel between selectivity of eolithic flints and selectivity by collectors of the more finely worked Palaeolithic implements, particularly during the earlier years of Palaeolithic archaeology (Kendall, 1921). At that period, crudely worked flakes were often ignored: de Mortillet himself had believed that the hand-axe was the only implement of his Chellean: ‘Un seul outil en pierre, l’instrument chelléen, toujours en roche locale’.

‘A single tool in stone, the Chellean implement, always made from local rock’ (Mortillet, 1883, p. 131). Similarly, Lankester and Moir now managed to select a number of rostro-carinates from the fractured flints of East Anglia and Moir was convinced that he...
could see evidence of progressive development in these naturally broken stones.

6. CONCLUSIONS

This brief survey of the pro- and anti-eolithic arguments employed by various individuals in these debates has highlighted a number of points, some of which are still of relevance today. First, a wide range of individuals were attracted to both sides of the debate: collectors, amateurs and professionals — all contributed their expertise. Second, the ensuing arguments underlined the importance of multidisciplinary research in the Quaternary — Harrison relied on Prestwich to build the geological foundations for the antiquity of the Kentish eoliths; later, Warren and Haward applied geological research more directly to the archaeological problem through their flint-fracture experiments. Finally, the styles of argument employed in these debates can clarify some of the underlying theoretical assumptions of palaeolithic archaeology.

Moir may have lacked 'extensive geological experience' (Haward, 1914, p. 349), but in their attempts to demonstrate that the sub-Crag pre-palaeolithic fulfilled general archaeological expectations, Moir and Lankester were using criteria inherited by Palaeolithic archaeology from a number of disciplines, not least of them geology. In the nineteenth century, as the science of Palaeolithic archaeology emerged, it adopted a methodology that was firmly enmeshed within a geological and palaeontological frame of reference (see Sackett, 1981, 1991). However, when Palaeolithic archaeologists employed the concept of progressive development through time, they were dealing with a vastly reduced time-scale compared to the evolutionary vistas enjoyed by geologists. Although the archaeologists invoked type-fossils, these were not animal species but artefact types, such as the Acheulian hand-axe: their shape and technology providing a clue to age and affinities. Lankester, a biologist by profession, was also following the traditional approaches of Palaeolithic archaeology when he defined the rostro-carinate as a pre-palaeolithic type-fossil. His initial disapproval of Moir's picture of evolving industries reflects a wider tension within Palaeolithic archaeology between the chronological order provided by such geologically based classifications and the anthropological reality of regional variation where artefacts did not evolve like species but were made by individuals as members of social groups. Echoes of a geological heritage have always characterized Palaeolithic debate and these tensions are familiar today. The geological discipline provided far more than a stratigraphical context for archaeological discoveries. The social reliance between practitioners of the two disciplines, as witnessed by the relationship between Prestwich and Harrison is still recognizable, but geology also contributed to the philosophical framework of palaeolithic interpretation and classification. The arrival of absolute-dating methods has still not entirely dislodged the legacy of the type-fossil approach, and the uncritical assumption that 'crude equals early' has not yet disappeared.

The eolith debate was a free-for-all: collectors and experimenters, geologists and archaeologists, professionals and 'amateurs' — all collided in a controversy that exposed such preconceptions to public view. Assumptions about the Palaeolithic industrial succession and the relationship of geology to archaeology were shaken up to the surface by this debate, which was not just a hiccup in the history of Palaeolithic archaeology, but, as Spencer (1988, 1990) has shown, moulded the direction and perception of research.

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