

## The Royal Society and the origins of British archaeology: II

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*Part I of this article was published in our June number (1971, 113–21). The second part of Mr Hunter's article on the Royal Society and archaeology examines the limitations of the Society's archaeologists and their mixed legacy to the eighteenth century. Some bibliographical references have been repeated for the convenience of the reader.*

Sir Thomas Browne's *Hydriotaphia* has been called 'the first English excavation report' (Evans, 1956, 25–6)—misleadingly, for although this was the first English book wholly inspired by archaeological finds, there is no evidence that these were deliberately made, and *Hydriotaphia* contains none of the accurate detail of the modern archaeological treatise. Besides, as Browne's title suggests, his concern is 'Urne-Buriall' in general rather than the finds at Old Walsingham, and, although he describes the urns and the circumstances in which they were found, his interest in mere antiquities is limited: 'who were the proprietaries of these bones, or what bodies these ashes made up, were a question above Antiquarism' (Browne, 1658, 71–2). This is a long way from that objective and limited study of antiquities in their own right which distinguishes the archaeological work of the Royal Society, and *Hydriotaphia* survives for its literary pretensions rather than for the finds that inspired them. Indeed, Browne's book is antithetical to the Royal Society's antiquarianism, for whereas the Society was interested in antiquities for their own sake, Browne's essay on the relics is only slightly associated with them, and his hypotheses are almost always theories argued from literary parallels rather than from closer examination of the finds. Browne's Notes 'Concerning some Urnes found in *Brampton-Field*, in *Norfolk*, Ann. 1667' (Browne, 1712), on the other hand, show more of the influence of the modern tradition of archaeological investigation, for his

description of this find is in complete contrast to *Hydriotaphia*, quite free from its metaphors, literary parallels, and philosophical speculations.

This is an interesting point, for it links the Royal Society's tradition in archaeology with the prose-style which it championed. Sprat, its historian, and his fellows denounced violently the use of metaphor, preferring a style close 'to the primitive purity and shortness, when men deliver'd so many *things*, almost in an equal number of *words*' (Sprat, 1667, 113). The change in style of Browne's archaeological writing possibly shows the influence of this manifesto, rather like its more extreme effect on Joseph Glanvill—who completely rewrote his *Vanity of Dogmatizing* (1664), before it was republished in 1676, to fall in line with this new plainness of language (Glanvill, 1676; see Jones, 1930, 992–8). Walter Charleton, the author of *Chorea Gigantum*, also wrote prose of the earlier tradition, and R. F. Jones has used his *Physiologica Epicuro-Gassendo-Charltoniana* (1654) as an example of the earlier 'Imaginative Atomist' prose which Sprat and his fellows attacked (Jones, 1963). Charleton's antiquarian scholarship matched his style, for his work on Stonehenge, like *Hydriotaphia*, stood in a tradition alien to the Royal Society.

But here the parallel ends, for Charleton was a prominent member of the Royal Society, and in 1663, the year in which *Chorea Gigantum* was published, was the first person to read an archaeological paper at a meeting of the Society, a

paper which, Joan Evans claimed, illustrates its 'scientific spirit at work in the archaeological field', because Charleton exhibited a plan of Avebury and suggested that its original function could be discovered by excavation (Evans, 1956, 28). This certainly resembled modern antiquarianism. At Stonehenge, his approach was its opposite, and the dichotomy shows that the gulf between the Royal Society and its predecessors was not absolute.

This appears constantly in the Royal Society's archaeological work. Many did not stand by Plot in his sharp differentiation between 'persons and actions' and 'things'. Both Thoresby and Machell contributed articles on antiquities to the *Philosophical Transactions*, but they also collected miscellaneous materials for County Histories in the Camdenian tradition, and Thoresby issued in 1715 his *Ducatus Leodiensis*, which is topographical and historical in emphasis, although he includes detailed reports of a few antiquities (cf. Machell, 1963). Likewise, Aubrey's *Natural History and Antiquities of Surrey* (1718–19) (see Enright, 1956) and Ashmole's *History and Antiquities of Berkshire* (see Josten, 1966, 1000–1001) are collections of genealogical, historical and miscellaneous information like Camden's or Weever's. The Royal Society archaeologists' attitude to Gibson's 1695 edition of Camden's *Britannia* is also instructive. The book stands firmly in the Camdenian tradition; the material added by three-quarters of a century of scholarship was mainly historical and fitted well into the framework of the original book—so that only in Gough's 1789 edition was a mass of archaeological matter incongruously appended to Camden's narrative to turn it into a gazetteer of British antiquities. There were, however, several archaeological additions to the 1695 edition, and more in that of 1722, in which the contributions of Lhwyd on Wales and Sibbald on Scotland began to show the archaeological bias of Gough (See Piggott, 1951, 212–13; Emery, 1958). These men were polymaths, and it was as reasonable for them to infuse Camden's historical antiquarianism into their archaeological work as to combine science with archaeology.

Nor should one forget that archaeology was

neither a major interest for the Royal Society at any stage, nor one that developed very quickly in its programme. The Society always lavished its main attentions on the natural and mechanical sciences, which were its only concern in its early years. There is no evidence that archaeology was a talking point either for the 'incunabile' of the Royal Society during the Commonwealth period (Weld, 1848, 31–2) or for the Society itself in the early years of its incorporation (cf. Birch, 1756, I, 406–7). The *Transactions*, indeed, contain no archaeological articles until the 1680s, and are sometimes specifically non-antiquarian, though this point should not be overemphasized because they were personally compiled by Henry Oldenburg, the Secretary, whose interests did not include archaeology\*, and the Minute Books of the Society suggest that antiquities were occasionally discussed in the 1660s and 1670s (Birch, 1756, I, 272; II, 104, 185, 274, 301, 305, 347, 462; III, 430; IV, 88, 116, 118, 180, 214, 286, 291, 303, 369, 448, 461; cf. Aubrey, 25, 2). It would, however, be rash to overemphasize the archaeological commitments of the Society.

Perhaps it is of greater significance that the Society's archaeological interest did not always produce work of a high objective standard. Dr Robert Plot devoted a whole chapter to antiquities in both his *Natural History of Oxfordshire* (1677) and his *Natural History of Staffordshire* (1686), and in each he surveys the 'things' of the past to the exclusion of 'persons and actions'. But he was evidently dissatisfied with conclusions based wholly upon things, and therefore linked them to historical sources, just as Jones and others dealt with Stonehenge in historical terms although they approached it as an antiquity. The result is fatal: Plot credulously repeats Camden's absurd claim (on etymological grounds) that the Rollright Stones were

\* Of 'Inquiries for Aegypt' framed in 1667, none is archaeological, and when in 1668 mention was made of a house at Rome 'which for above 10 ages had been buried', the interest of the contributor was only in the lamination of glass (*Phil. Trans.*, II, 470–2; III, 783). When in 1671 Oldenburg answered accusations that the *Transactions* 'neglected and despised all Antiquities', he only claimed not to have ignored ancient authors (*Phil. Trans.*, VI, 2089f).

a memorial to Rollo the Dane (Plot, 1677, 336f.; Camden, 1610, 374), and he quotes uncritically from other earlier writers who had produced neat historical explanations for antiquities. Worse still, Plot adds to this collection of misleading parallels: citing Suetonius's life of Julius Caesar, he argues that tessellated pavements were the floors of the tents of Roman generals, a claim that was to stunt the development of Romano-British civil archaeology for years (Plot, 1677, 327).<sup>\*</sup> Similarly, he divides hillforts of the pre-Roman Iron Age into 'Saxon' and 'Danish' types, on the authority of 'a MS History of *Ireland* by E.S.', whose authority he fails to examine at all (Plot, 1677, 334), and he goes on to correlate individual hillforts with historical events: Tadmarton Castle in Oxfordshire, he declared, was erected in 914 (Plot, 1677, 334) whilst Castle Ring at Cannock, Staffordshire, belonged to the campaigns of 1013 or 1016 (Plot, 1686, 418).

Plot shared this foible with Aubrey, who may have suggested the hillfort hypothesis, for Aubrey proudly claimed that 'Dr R. Plott . . . knew not how to distinguish a Roman camp from a Danish-camp, till I told him' (24, 158).<sup>†</sup> The section on 'Campes' is perhaps the poorest part of Aubrey's *Monumenta*: the material was collected, and Aubrey struggled to interpret it, but he never produced an all-explanatory theory of camps as he did for megaliths. He accepts throughout that 'the Roman Campes are allwayes square, or at least squarish; and a single worke' (24, 250) (though unfortunately most of his examples are 'squarish' and not really Roman at all), but on camps that were 'Round, or roundish, and double or treble Workes' his ideas are less clear: sometimes they are claimed as British, whilst elsewhere they are Danish

<sup>\*</sup> Sir Christopher Wren and Aubrey believed that tessellated pavements were the remains of Roman villas (Aubrey, 24, 243, 247) and both Morton (Morton, 1712, 510) and Tabor (Tabor, 1717, 559) came to the same conclusion after weighing this theory against Plot's: so it is not easy to claim that Plot could have known no better.

<sup>†</sup> The source for Aubrey's Danish hypothesis was, like Plot's, Irish: 'Mr. . . . Gethyng of the Mid. Temple (an Irish gentleman) assures me that in Ireland are a great number of *Danish Camps*, wch are all round and with double or treble workes' (Aubrey, 24, 152).

('me thinks they are too artish for the old Britaines' (24, 152)), and it is impossible to know which notion Aubrey held first—he held them both during 1668 (24, 152, 250).

Inconsistencies appear elsewhere. Treating barrows, Aubrey sometimes states the orthodox theory that they contained the bodies of men slain in battle: 'so great a signe of Slaughter I never found' (24, 254; cf. 24, 139, etc.). But elsewhere he denies this: 'My conceit is that the Seaven Barrowes &c (where are severall together) were not tumulii or barrowes erected upon the account of any great person slain there in Battle: but in those times they chose to lye drye upon such hilly ground: and those of the same familie would desire to be neer one another; as the Kings at Westminster abbey, and at St Dionyse in France' (25, 18). So with the Uffington White Horse, which he claimed was 'made by Hengist' (24, 179; cf. 25, 62), although later, when discussing a coin of the Belgic king Arviragus found at Colchester, he remarked that the horse on the reverse 'putts me in mind of the White Horse cutt in the hill called White-horse hill in Berks: which some will have to be made by Hengist or Horsa' (25, 122). Instances could be multiplied, both here and in other works—'he wrote down what he was told, without having at the moment any means of sifting it, and afterwards either forgot or neglected to do so' (Jackson, 1862, iii)—and Gibson found the *Monumenta* 'a mere Rhapsody' (Piggott, 1951, 211). But, in spite of the difficult circumstances in which it was compiled (two pages were once misplaced 'through haste upon my removal from my chamber for the Smallpox' (Aubrey, 25, 18)), his *Proposals for Printing* the book (1693) prove that he intended to publish the manuscript as it stands, and it must be taken at its face value.

At times, Aubrey shows remarkable credulity: the worst of his work is exemplified by his interpretation of the antiquities near Wayland's Smithy, which he thoughtlessly linked with Hengist, Horsa and 'Uter-pen-dragon' (25, 62), whilst elsewhere he accepted a Gloucestershire barrow as the site of St Oswald's martyrdom and a Worcestershire hill as his grave (25, 19). There are gaps, too, in his antiquarian studies:

in spite of his discussion of medieval ecclesiastical architecture in the 'Chronologia Architectonica', he had no knowledge of medieval military architecture at all (all are Roman in his 'Of Old Castles' (24, 198f.)), and some of the stone-circles in the 'Templa Druidum' (Book I) comprise two stones, whilst one 'temple' he reconstructed from only one (24, 67). These are mixed up with his Druidic fancies: the mortices of the mortice-and-tenon joints at Stonehenge he interpreted as the nesting places for the holy birds of the Druids, the 'aves Druidum' (24, 89), and Batteley in his *Antiquitates Rutupinae* tells an anecdote reminiscent of the famous day in 1754 when the Princess Dowager consulted Stukeley about a hoard of Bronze Age tools (Piggott, 1950, 169–70): Batteley showed Aubrey a bronze object which he had quite reasonably identified as a strigil, and was horrified at Aubrey's fanciful reply, 'qui, conspecta strigili nostra, protinus exclamavit, en auream falcem, qua viscum *Druides* demetere solebant' (Batteley, 1711, 77).

One can only conclude that Stukeley owed his credulity as well as his early objectivity to his predecessors in the Royal Society. From these foibles of Plot and Aubrey (who were among the most prominent of the Royal Society's archaeologists) there is a clear line of descent to Stukeley, who was uncritical in his early work as well as later.\* For almost all the archaeologists of the early eighteenth century struggled with this double legacy of objectivity and speculation, combining the two elements in varying degrees in their work, though few went so far as Charles Leigh, who evidently owed something to the Phoenician theories of Aylett Sammes and who attempted in his *Natural History of Lancashire, Cheshire and the Peak in Derbyshire* (1700) to demonstrate

\* For example, Stukeley dated Roman camps with embarrassing historical precision in his *Caesar's Camps* of the early 1720s (Bodleian Library, Oxford, MS Gough Gen. Top. 24), and his fanciful connexion of the 'Pudens' (the reading was a conjectural one of Gale's) of the Chichester Minerva inscription with the New Testament (*Itinerarium Curiosum, Centuria I* (London: for Author, 1724), 193) survives today in the lunatic fringe of the archaeology of Christian Roman Britain.

'from the *Armenian, British and Phoenician* languages compared together and examined, their *Deities*, the *Asiatick* Manner of Fighting, the *Eastern and British* Way of computing *Time*, the *Reverse of a Coin* and diverse other Things, that not only . . . these counties, but the whole Island was chiefly and primarily inhabited by Colonies from *Asia* long before either the *Greeks* or the *Romans* came hither' (Leigh, 1700, preface). Thomas Hearne used the Roman generals' tent theory to link finds even more closely to historical narrative than Plot, relating Roman military campaigns to sites in certain areas (1712, passim), whilst he exceeded Plot's perversity in declaring that a hoard of Bronze Age socketed axes were '*Roman Chissels*, which were us'd to cut and polish the *Stones* in their *Tents*' (Hearne, 1710, 109).<sup>\*</sup> Equally unfortunate (though less culpable) was John Bagford's interpretation of the mammoth found associated with a Palaeolithic hand-axe near Gray's Inn Lane as an elephant imported by Claudius, which appears with other facile interpretations of archaeological evidence in a letter addressed to Hearne (Bagford, 1715), whose ideas it may reflect. Hearne's approach to Romano-British antiquities influenced John Pointer and Nathaniel Salmon in the following decades,<sup>†</sup> as well as Stukeley himself.

Other writers on antiquities connected more or less closely with the Royal Society inherited both its poor and positive aspects, like John Batteley, whose careful collection of antiquities from Reculver was admirable, though limited in technique—he failed to study the finds in context (Batteley, 1711); and William Musgrave, at one time secretary of the Royal Society, whose *Antiquitates*

\* Hearne claimed that the '*Instruments* [were] also us'd in making the *Roman High-ways*, and in *draining* their *Fenms*' (Hearne, 1710, 111).

† Pointer attacked Hearne's views on the site in his *Account of a Roman pavement lately found at Stunsfield* (1713), and he also published a mainly numismatic *Britannia Romana* (1724). Salmon's principal works on Roman antiquities are *Roman Stations in Britain* (1726), *Survey of the Roman antiquities in some Midland Counties of England* (1726), *History of Hertfordshire* (1728) and *A New Survey of England wherein the defects of Camden are supplied* (1728–9).

## THE ROYAL SOCIETY AND BRITISH ARCHAEOLOGY

*Britanno-Belgicae* (1711–20) is of variable value. Even John Morton\* in the chapter on antiquities in his *Natural History of Northamptonshire* (1712) and John Tabor in his essay on Roman remains from Eastbourne (Tabor, 1717) have to waste space determining the difference between Saxon and Danish camps, or the function of tessellated pavements, in the middle of otherwise admirably detailed and objective antiquarian studies, and, indeed, only one work really escaped it all, *Britannia Romana* (1732), by John Horsley, whom R. G. Collingwood praised as a scholar no less indispensable for his period than Gibbon for his (Collingwood, 1921, 52). Elsewhere the double influence was all-pervasive: so John Wood gives the first accurate plans of Stonehenge in a pamphlet as rich in Druidic fancies as Stukeley's (Wood, 1747), and the same dichotomy recurs in Stukeley's own *Stonehenge* (1740) and *Abury* (1743). Even a century after Aubrey's death the theory that Roman fortifications must be square had such wide currency that Sabatier in his otherwise sound description of the Chichester entrenchments claimed that Chichester could not be Roman since the shape of the city was 'evidently either Saxon or Danish' (Sabatier, 3). And Druidic theories fathered by Aubrey had

\* Morton was a keen member of the Royal Society circle, who corresponded with Lhwyd; over thirty pages of his *Natural History of Northamptonshire* were devoted to the Roman antiquities of the county.

at that time still to reach their full development.

Thus the truth is more complex than the simple contrast between the Royal Society and its predecessors. Just as there were precedents for its archaeological studies, so the society's legacy was not without shortcomings, and these unfortunate developments must be assessed as part of its contribution to archaeology. But it was no more than an historical accident that the weaker parts of Aubrey's *Monumenta* received wide circulation whilst the larger bulk of brilliant work, far outweighing these foibles, was almost forgotten; and one can afford to be charitable even to Plot, although his publications may have done more harm than good. The worst shortcomings are perhaps symbolized in the archaeological map of Wessex which Aubrey included in his *Monumenta*—for the brilliant idea of a specialized plan showing ancient remains (in contrast to earlier, general county maps), including the Roman roads and earthworks of the area, was only partly marred by his arbitrary allocation of hillforts to different periods (Aubrey, 24, 250–1). The best scholarship, on the other hand, including the untrammelled objective fieldwork of Lister, Conyers and Lhwyd and Aubrey's consolidation of his material into the 'Chronologias', attained an excellence in archaeological studies that was hardly equalled before the last century. It is this that should stand as a memorial to the archaeologists of the Royal Society.

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