Sutton Hoo – an archaeography

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Introduction

Sutton Hoo is one of the great archaeological sites of Europe and pivotal for the understanding of how early Medieval kingdom-building and Christianisation were expressed. The latest campaign of excavation there was certainly great in size – almost a hectare was opened, but the estimation of quality – which probably lies behind our convenor's title – is something else. That needs a context. What I chose to do in this chapter was to describe not just the latest campaign, but the four that preceded it, and try to put each into the context of its day. My hope is that we may discover something useful about how the results of archaeological excavation relate to its objectives, its resources, its work-force and its contemporary situation. Everyone knows these things are relevant, but their influence is not always self-evident. I offer here an empirical description of how the excavations were done – giving this study the grand epithet 'archaeography', writing about the doing of archaeology, by analogy to historiography, writing about the writing of history.

Figure 1. The Sutton Hoo burial mounds, looking west in 1982, before the start of Campaign 5 (Ph: C Hoppitt).

First Campaign, *c.* 1600

Sutton Hoo is a small group of burial mounds on the first gravel terrace on the east bank of the River Deben in Suffolk. The mounds, as we know now, were erected over a short period of about 60 years between 590 and 650 AD. They had stood during the Middle Ages in sheep pasture, rising to nearly their original height of 3–5m. Towards the end of the sixteenth century three things happened quickly: the mounds were exploited as warrens for farming rabbits, they were ploughed and they were explored by digging vertical shafts from above. These things may have been connected: in 1988 when rabbits were again rampant, they excavated a reticella bead from Mound 7, and something of the kind may have led an inquiring mind to investigate the source of a casual find in the late sixteenth century . Our own excavations showed that the technique of investigation employed was to dig a large pit from the summit of the mound, and go on digging until a treasure was sighted. Such a pit was documented in Mounds 2, 5, and 14, and is implied in Basil Brown's investigations of Mound 3, where the large depression in the top was referred to as a 'dew pond' (Carver 2005, 66–9).

Two mounds escaped: Mound 17 covered the grave of a young man in his coffin and, in another grave alongside, his horse. The merry shafters of Campaign 1 had begun as usual at the top of the extant mound and gone vertically downwards, with the result that they had arrived exactly between the two grave-pits (Carver 2005, 136). The shaft was continued into the subsoil and into the fill of a Neolithic ditch, not subsoil but apparently recognisable as not grave fill either. These excavators were not neat, but it would be a mistake to believe that they were not informed. The excavators also gave their attention to Mound 1, burial place of the celebrated Sutton Hoo treasure, which fortunately they did not find. Here medieval ploughing had so eroded and distorted the mound that its summit no longer lay directly above the burial chamber. The floor of the chamber was in any case some 3m below ground level, and the top of the mound 3m above that. The shaft gave up at 'about 10ft' from the high point of the mound in 1939. The sixteenth-century robber shaft thus missed the chamber to the east and ended about 3m short.

Evidence for the date of this first campaign, to around 1600, comes from Mound 2, where the shaft was cut by the trench of Campaign 2 and from tracks on dated maps which crossed the filled-in quarry ditches (Carver 2005, 465). Primary evidence came from Mound 1 where sherds of Bellarmine ware were found at the base of the shaft, along with the remains of a hearth. This assemblage was termed 'the lunch of the disappointed' by the 1939 excavators, who no doubt saw nothing strange in a group of robbers with a gin bottle cooking lunch at the bottom of a hole 1m across and 3m underground.

Wecan say rather little about the techniques of Campaign 1. That it was a campaign is implied by the number of shafts and the resemblance between them. One bonanza gives an appetite for more. Campaign 1 must have been a great success if the poor yield of Campaign 2 (which followed it) is anything to go by. The late sixteenth-century excavators must have hit the jackpot in at least Mound 2, Mound 14 and Mound 3, since the subsequent meagre remains of these burials suggest that they retrieved a deal of treasure. We can also deduce that the operation was fairly dangerous. The Sutton Hoo deposit does not stand easily more than about a metre high. Basil Brown brought the sides of his excavation down several times in both Mound 1 and Mound 2. Having said that, we found no evidence for the tragedy that a 3m shaft would seem to invite – no 'corpse of the disappointed'. One imagines long wooden ladders and small boys scrambling for anything bright and shiny.

This as the period for a major redistribution of the nation's assets. It is my belief that as well as

Figure 2. The 19th century excavation through Mound 7, redefined in 1989 (Photo: Nigel Macbeth).

privatising the monasteries, the late sixteenth-century government felt it had the prerogative to license the digging of burial mounds, the contents of which would otherwise revert to the king (Carver 2000, 25–7). We have some evidence that a systematic pillaging of mounds in Suffolk began around the time of the dissolution. In 1538 Thomas Toyser applied for a licence for a programme to finish off excavations at Brightwell that had been started by 'ill-doers,' that is, unlicensed diggers. The character of this type of archaeology is revealed by its local colloquial name 'gold mining'. John Dee (1527–1608), the great Elizabethan intellectual entrepreneur, may well have been involved with the Sutton Hoo Campaign 1, as he seems to have been an active treasure-hunter elsewhere. In 1574 he wrote to Lord Burghley saying that he would 'discover a mine of gold or silver in the Queen's dominions, which is to belong to her, on condition of his having a right to all treasure trove in her dominions' and somewhat unsubtly offered Burghley half the proceeds. 'Treasure trove' does not sound like an actual stratum of ore, and it seems more likely that we see here the tip of a highly lucrative portable antiquities scam, run by a gold-hungry cartel associated with the Tudor hierarchy.

From this we could also deduce that the Crown could licence landowners to quarry mounds on their own land. At Sutton Hoo, the landowners in the district were Sir Michael Stanhope, Sir Henry Wood and the Mather family, the latter being the most likely suspect. Sutton Hoo is labelled 'Mathershoe' on the early seventeenth-century Norden Map, and the Mathers lived in 'How farm', between Sutton Hoo and the river Deben. A study of the wills of the Mather family by A. M. Breen shows that while John Mather (dying 1567) and Thomas Mather (1592) are described as yeomen, Robert Mather (died

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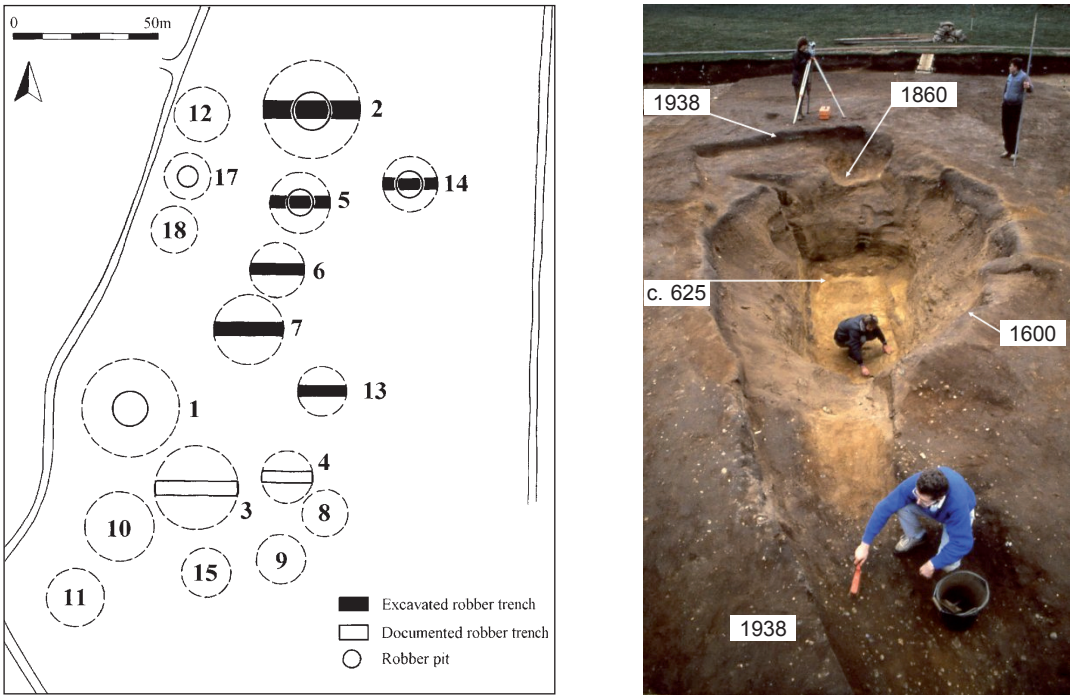

Figure 3.3. (left) Map showing located excavations of Campaign 1 (circular) and Campaign 2 (rectangular); (right) Visitations to Mound 2, Campaigns 1–3 and 5 (Photo: Nigel Macbeth).

1639) and his son are styled as gentlemen (Carver 2005, 469). The Mathers had gone up in the world, then, not inconceivably as a result of the generous 'hoo' on their doorstep.

Second Campaign, *c.* 1860

The existence of a Campaign 2, as with Campaign 1, was discovered archaeologically by excavating and recording every recent disturbance with the same level of attention as the ancient features. No doubt we would have preferred to have found our burials intact (as Mound 17), but I would recommend this 'archaeology of archaeology' as a rewarding exercise in a league of its own. Certainly excavating an intact chamber is child's play in comparison: excavating earlier excavators requires navigation through a dozen random cuts and dumps amongst an explosive scatter of smashed artefacts and splintered bone.

Campaign 2 was identified during the excavation of Mound 2, which incidentally had been also visited by Campaign 1 and Campaigns 3 and 5, thus providing a sequence of intrusion which allowed us to characterise and order them (Figure 3.2). However, the clearest signature of Campaign 2 was offered by Mound 7. The excavators here dug a long trench running W–E straight through the mound at ground level. In one place at least a large block of mound make-up had become detached and was about to fall. The spoil was extracted on the east side along a barrow run which had become compact with traffic. At the west end a flight of steps had been cut into the mound make up and through the buried soil, so that a visitor could make their way gracefully down to the central pit. The bottom of this pit was round and about 1.5m below the old ground surface, and may have actually been the

bottom of a Campaign 1 shaft. The scene is therefore fairly clear: workers excavated the dirt on one side and took the risks of collapse, while the gentleman antiquary descended the steps at intervals to see what interesting curiosities had been brought to light.

Similar trenches were cut through Mounds 2, 3, 4, 5, 6, 13 and 14. At Mounds 5 and 6, trial pits had been dug on the west side, apparently to establish the height of the subsoil.
If there had been a trench through Mound 1 it did not reach the bottom and was not seen by the1939 excavators who used much the same technique themselves. Noting the W–E linear depressions in the top of many mounds, Rupert Bruce-Mitford interpreted them as 'ship-dents,' that is, fill that had somehow collapsed along the line of a buried ship (1975, 159–60). This phenomenon was used to support the case for further excavation in the 1980s, but in reality argued against it: we know now that the 'ship-dents' were just the remains of the backfilled trenches of Campaign 2, and the mounds in question had already been excavated and contained no ships.

A possible date for this campaign was offered by the *Ipswich Journal* for 24 November 1860. This issue reported the excavation of a group of five Roman [*sic*] barrows about 500 yards from the banks of the Deben opposite Woodbridge on a farm occupied by Mr Barritt. The summary says: 'A considerable number (nearly two bushels) of iron screw-bolts were found, all of which were sent to the blacksmith to be converted into horseshoes! It is hoped, when leave is granted to open the others, some more important antiquities may be discovered'. The unimportant antiquities that were so usefully recycled were undoubtedly ship rivets, and the only known source of them would then have been Mound 2 (since Mound 1 remained undisturbed at this date). Disturbed ship rivets were found in the vicinity of Mound 5, 6 and 7, which implies that leave had been granted to open other mounds, and that the campaign had moved from north to south.

It seems reasonable to equate the set of W–E trenches of Campaign 2 with the entry in the *Ipswich Journal*, and date it to the mid nineteenth century. This was a period of barrow-digging of a new kind, the age of the antiquary. On the rising tide of archaeological curiosity the Archaeological Institute had been created in the 1840s and J. M. Kemble was publishing Anglo-Saxon objects in its proceedings throughout the 1850s. The cemetery at Snape a few miles north of Sutton Hoo had been investigated in 1827 by some 'gentlemen from London' but it was not until 1862 that a boat burial was found there. The great intact Scandinavian ships had not yet been found: the Gokstad burial was unearthed in 1880 and Oseberg in 1904 (Nicolaysen 1882; Brøgger *et al*. 1917). Thus the Sutton Hoo excavators of 1860 can perhaps be forgiven for failing to recognise their ship-rivets. All the same, the excavation team seems to have been somewhat isolated from their contemporary archaeological mainstream. The Suffolk Institute of Archaeology and Natural History held its meeting in Woodbridge in 1860, but makes no mention of the campaign; nor does the Gentlemen's Magazine in 1861, although it reported the Snape ship in 1863. Local historians Hele (1870) and Redstone (1897) seem unaware of any excavations at Sutton Hoo, although they knew the mounds well enough.

The explanation would seem to be that the excavators of Campaign 2 found very little, or recognised little of what they found; and the explanation for this would be that the excavators of Campaign 1 had already got away with the lion's share. The mounds had been ploughed again since the sixteenth century, so the shafts would not perhaps have been evident. In 1860 Robert Barrett was the tenant farmer and George Friston was the blacksmith. But there is no evidence that they, or any of the other associated farmers or landowners got rich. Besides, in this age when antiquities were regularly exhibited by the newly formed societies in village halls all over the country, the kudos would seem to lie in knowledge rather than wealth.

Figure 3.4. Excavations of 1939 (Campaign 3). (left) Basil Brown (right) The Mound 1 team in action watched by Mrs Pretty and friends.

Third campaign, 1938–39

The circumstances of Campaign no 3 have been often retold. Mrs Edith Pretty was then the landowner, and her urge to investigate the mounds that were visible from the bow-windows of her twentieth-century pile is thought by many to have come through her propensity for the occult. It is true she had a medium, who was, incidentally, instrumental in her later decision to give the Mound 1 treasure to the nation. But she had also had previous archaeological experience in Vale Royal abbey and in Egypt, and was no doubt able to imagine what the mounds might contain. All previous work having been consigned to oblivion, the expectation was that they would be Bronze Age like those on Martlesham Heath across the river. Mrs Pretty retained the services of Basil Brown, a self-taught professional excavator who worked free-lance for Ipswich Museum. His excavation technique was more or less identical to that of his nineteenth-century forerunners, although he himself considered it was state of the art citing an article in *Norfolk Archaeology* by Rainbird Clarke and Apling (1935). Brown was eulogised by later generations of East Anglian archaeologists, so it is as well to remind ourselves that a contemporary viewed his on-site performance as 'just like a terrier after a rat.' He regularly collapsed trenches and his recording was bizarre. However, his recognition of ship rivets in Mound 2 and the subsequent definition of the ship in Mound 1 were strokes of genius. He had bicycled over to Snape and found the rivets of the 1862 excavation in a drawer. When confronted with the first rivet in Mound 1 he recognised it as being *in situ* – and, aided by the

publication of the Oseberg ship burial which he had borrowed from Mr Maynard (of Ipswich Museum), was able to imagine the ghost of the ship laid out in its trench, (Brown 1974, 166).

In 1938 Brown drove trenches through Mounds 2, 3 and 4, having first found the level of the subsoil with a test pit: the one for Mound 2 contained an old bucket in its backfill. We know the Mound 2 trench very well since it was re-excavated in Campaign 5. His trenches were laid out on compass bearings, and this one is decidedly skew. He approached from the east, bumping along the surface of the buried soil as best he could, noting prehistoric material and hoping to find the brightly coloured splash of the burial pit towards the centre. In practice he did not, because Mound 2, like Mounds 3 and 4 had been excavated twice before, but he attempted to achieve definition within the fog of the disturbance by widening the trench at the centre of the mound into a square area, until he could define the original burial pit.

In 1939 he employed the same technique, starting the trench on the east side and driving it horizontally into the mound. In this case he hardly saw the buried soil or the subsoil, but happily had the rivets to guide him. The method of excavation was to slice the sand horizontally, using a shovel, or latterly, a coal shovel lashed to a long pole. When a red spot appeared, he recognised the iron of a rivet, and made the assumption that each rivet would be in situ, and must be left in situ, so he dusted it carefully with a pastry brush and moved forward. As he approached the centre, the mound got higher and the boat got lower, so the sides of the trench had to be continually cut back. The method did not give a standing section through the mound.

It is not commonly known that Basil Brown actually defined the whole length of the Mound 1 ship and the burial chamber at its centre, also having a poke at a few voids in the mat of decayed wood that covered it. Since the appointment of Charles Phillips, the Cambridge prehistorian who had got wind of the find, Brown had been repeatedly told to stop. But he continued to excavate without a pause for nearly four weeks, until he was finally dispossessed on 8 July by the Phillips team. At one time or another this team consisted of Stuart Piggott, W. F. Grimes and Peggy Guido (then Piggott) who did most of the work, with O. G. S. Crawford, Sir John Fosdyke, T. D. Kendrick, John Ward-Perkins, Graham Clarke, John Brailsford and Commander Hutchinson as visiting participants. In an incredible seventeen days, this team emptied the burial chamber of 263 objects which were then packed in moss in boxes, sweet bags and tobacco tins. An inquest held at Sutton village hall found that they belonged to the landowner, Mrs Pretty, and were not Treasure Trove because the seventh-century burial party had not planned to retrieve them. Mrs Pretty subsequently presented them to the British Museum and there they have been ever since.

Before taking leave of this well-known episode, we can note a number of things from a twenty-first century perspective. Mrs Pretty's motive was curiosity and there was no regulation to discourage her from satisfying it. The mounds were not scheduled, and unlike in the sixteenth century (when they were at the disposal of the crown), the contents were legally hers, as the inquest showed. Mrs Pretty has also been lavishly praised by posterity, so it is as well to remember that she provided very few resources for the excavation, other than a few basins. She paid Basil Brown a labourer's wage, allowing him temporary accommodation in an upper room of the chauffeur's cottage, and the assistance of her gardener and game-keeper. It was this small team and their lack of money that, more than any supposed methodology, determined the course of the excavation. Three people can only move so much earth, and the best method is the one that maximises yield for the labour available. The required yield was objects, since the commissioning agent would know little of the broader scientific potential. Brown cannot be wholly blamed for being messy and inaccurate – because he was obviously in a great hurry.

Fast digging is bad digging, and the reason, then as now, is usually money, a lack of understanding that unsuccessful excavations, done properly, take just as long – and may take longer – than finding a bonanza.

To begin with it was also a cosy affair. Robert Pretty then aged 8 would be a regular face at the trench-side, and he buried his roller skates in the back fill of Mound 2 (where we found them). The atmosphere changed with the arrival of the upper echelon from Cambridge and London, for whom Brown became a labourer again. Now the voices on site were no longer broad Suffolk, but high pitched, point-scoring academics. This resulted in no immediate increase in quality – the coal shovel was still in use, and was augmented in the chamber by the pastry brush and the 'packer's needle', a long curved steel needle for sewing hessian. Although it was intended to erect a shelter (Brown 1974, 164), in the event there arrived only a tarpaulin and twelve scaffold poles 'for which little use could be found' as Phillips remarked (Sutton Hoo archive/vol 1). The excavators moved cheerfully about the chamber treading on everything. 'I have no doubt that we did in fact stand on quite a number of the objects a number of times' remarked W. F. Grimes, but 'you could have danced on that jewellery and you would not hurt it – not that we did! The amount of gold leaf that was blowing about was frightful.' (Carver 1998, 17) These excavators too pleaded shortage of time, but unlike Basil Brown, who had laid it out for them, they could not plead ignorance of the job in hand. In spite of the involvement of the British Museum, the University of Cambridge, the Office of Public Works and Mrs Pretty, herself a millionaire, the total expenditure for the excavation of Mound 1 was £250.

There was perhaps a spirit of make-do and mend that was seriously attractive to a generation brought up on Arthur Ransome and E. E. Nesbitt. Not just too proud to ask for money but even more proud of what can be achieved heroically without it, by a group of merry chaps (who anyway have plenty of their own), and whose resourcefulness and valour were about to be thoroughly tested by a world war.

Fourth Campaign 1964–1971

The objects from Mound 1 spent their war in the London underground, and in 1945 were to begin a long process of analysis culminating in their publication in 1983 (Bruce-Mitford 1975, 1978, 1983). One of the most detailed examinations of a grave assemblage ever achieved, this 2439 page book is monument to the diligence and persistence of Rupert Bruce-Mitford and his team. Those impressed with the Management of Archaeological Projects (MAP2) procedure might care to notice the ratio of effort here between excavation (3 months) and post-excavation (38 years). In addition to conserving and studying the objects, Bruce-Mitford, who had trained with Sune Lundqvist on the Uppland ship burials (Vendel and Valsgärde), was aware of several enigmas in the Sutton Hoo record. Should there not have been horses and dogs with the Mound 1 burial? Could there have been unconsidered fragments that would make sense of helmet or shield? What was Sutton Hoo's prehistoric predecessor, that Basil Brown had noted?

So, in addition to funding the study of the finds, the British Museum launched a return excavation of eight summer seasons. In contrast to the valiant pre-war adventure, here no expense was spared. The excavation crew was put up in hotels, and in addition to the contingent from the Medieval and Later department, included prehistorians: Paul Ashbee to study the mound, and Ian Longworth and Ian Kinnes (from Prehistoric and Roman) to study the earlier site. In pursuit of the varied agenda, there were eventually eleven interventions, five in the area of Mound 1 (including the excavation of the mound and the 1939 spoil heaps) and six in echelon across the north end of the site. The ship

Figure 3.5. Excavations of 1965–71 (Campaign 4) (left) A re-excavation of the Mound 1 ship under a shelter; (right) Excavation of the spoil-heaps, using box method (P. Ashbee).

Figure 6. The sutton Hoo site cleaned up for evaluation in 1983 (Photo: C. Hoppitt).

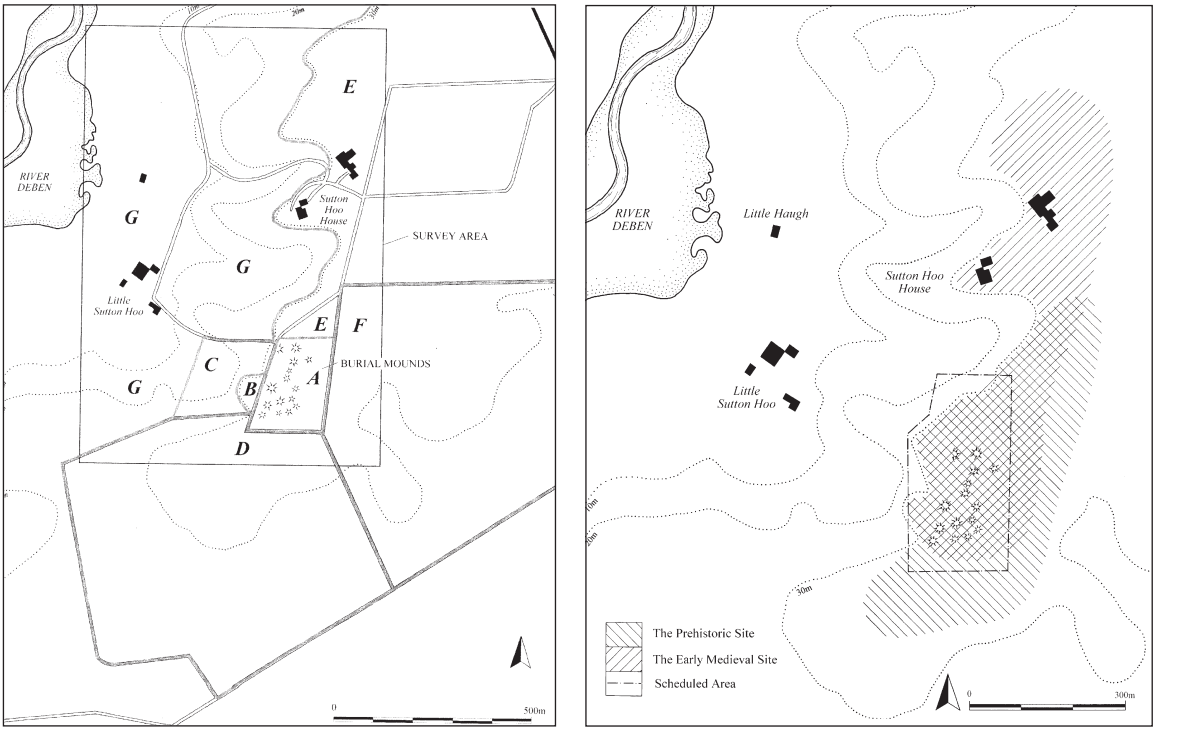

Figure 7. Campaign 5, evaluation stage (right) Zonation; (left) The main areas located. .

was reopened using 'area excavation', while the spoil heaps were excavated using the Wheeler system of parallel boxes. Area excavations were used in the northern sector to test for prehistoric settlement, and to verify the existence of Mound 5. Subsequently, trenches were used to chase the Neolithic ditch in the north and the Medieval lynchet in the south. The Mound 1 site, originally excavated in the open, was now covered by a vast shelter of scaffolding, and the ship was re-excavated, recorded again, rivet by rivet, and then lifted in a huge series of plaster of Paris casts, before being recast in fibreglass. Unfortunately the ship trench had been left open in 1939, shortly after which it had been included in a military training area and driven over by bren-gun carriers. The records made by Commander Hutchinson in Campaign 3 of the near perfect layout of the ship revealed by Brown had been lost in the war. But records of the highest precision were applied in Campaign 4 to the ship's mangled and distorted carcase. Meanwhile, the intensive sieving of the spoil heaps produced only 34 fragments of iron and pottery of which two objects may have been Anglo-Saxon (or Medieval) brooches (Bruce-Mitford 1975, 455).

Although it might appear lavish and disjointed, there could have been no question of *not* carrying out this campaign, and to the highest standards. The Sutton Hoo treasure had become famous both in scholarly and public circles, and its profile was raised further by the television film *The Million Pound Grave* made to celebrate the 1939 discovery. Just as the 1939 crew were anxious not to make too much fuss, the 1969 crew was anxious to show that they were pulling out all the stops. The world had changed, after all; this campaign was not powered by the idle curiosity of a landowner. It was led by the primary research project of a world class museum, with one eye on the Germanic continent which

Figure 8. Campaign 5: prototype ground penetrating radar, operated by Mike Gorman of the Scott Polar Institute, 1984.

envied the find and another on the USA whose museums had rather greater resources. With these academic and social pressures in mind, Bruce-Mitford's achievement can be seen as remarkable.

Fifth Campaign 1983–2005

The fifth campaign came about partly through the agitation of Bruce-Mitford who wanted to see more mounds opened, and partly through the desire of the Society of Antiquaries to launch a flagship project. The matter was raised in public discussion at the Oxford conference entitled *Anglo-Saxon Cemeteries 1979*, which soon revealed the current tensions. Since the Sutton Hoo find had gone on display at the British Museum, the archaeology field profession had been born, grown up and become distinctly battle-weary. Margaret Thatcher had come to power and the long nurtured dream of a state archaeological service, in which respected practitioners could live out their days in peace, was looking decidedly shaky. The cold wind of competition blew through the caravans, and invited us to take up arms again, this time against each other. The sensibilities of such a field profession were never going to look kindly on a project which promised to spend a large amount of money on an unnecessary dig. Moreover, the deployment of this profession in county-based 'units' meant that they were given to zealous (or jealous) defence of their turf. On the other hand the academic profession was frustrated that its research agenda was continually being upstaged by the moralising of the rescue lobby.

Although I was a member of the field profession leading my own company, rather than an academic, I was invited to apply for the job of director when it was advertised in the London Gazette. And when

offered it, I accepted, for a quite particular reason. Since 1973 I had been developing and promoting an approach to excavation which I thought ought to work in a formal research investigation, just as I believed I had made it work in the rescue theatre of the West Midlands. This was my big chance. I was also struck by how static was the excavation methodology of the 1970s, mostly but not exclusively owing to Phil Barker's unbending empiricism (Barker 1977, and White and Everill, this volume). A reasonably well-funded project ought to provide the opportunity to try new things – which might prove useful to the field profession. Similarly, the academic profession, notoriously cavalier in its treatment of sites and excavators alike, could only improve its attitude to fieldwork if caught sufficiently young as volunteers. These were some of the points I made to a meeting convened by the Society for Medieval Archaeology for all potential objectors in 1983, on which occasion I also presented a draft project design.

The project design for Campaign 5, the story of the project and its results have been published in some detail elsewhere, so I will use the space I have just to illuminate selected points and to compare and contrast with what had gone before. I will end by drawing what I hope you will accept as a general point of principle about the relationship between excavation and archaeological theory more broadly.

The first point I want to make is that Sutton Hoo was the first (and may prove to be the last) archaeological project to be awarded by design competition. Those who put in for the job had to submit a preliminary design for it and the winner then got to name the price. It was not won by preferment - the appointment of an acolyte by his deacon, nor the commissioning of a named academic by a grant-giving body, nor the result of pressure from a local authority. All these things we have seen many times before and since. Nor was it a result of competitive tender, as in the new Cultural Resource Management (CRM) profession. On this occasion, archaeological research was treated like a commissioned work of art and the archaeologist like an artist: the choice was made on the basis of an outline proposal defended at interview.

Before accepting the appointment, I was able to ask for, and obtain, a three-year period in which to prepare the programme. As I had in the West Midlands, I called this phase the 'design phase' and the preparatory process 'evaluation' and although the Sutton Hoo Trust had never apparently heard of either they were supportive way beyond my expectations. During the design phase, every type of remote mapping was applied in order to try and anticipate more precisely what lay beneath the turf, and inquiries were made at home and abroad as to the expectations of the scholarly community. The funding of this phase was particularly far-sighted on the part of the sponsors, since all of them had committees to satisfy, and in the case of the BBC, had films to make. By contrast, English Heritage, which was founded at about the time that Campaign 5 began (in 1983), and required us to apply for Scheduled Monument Consent, actively disliked my design-led procedure which they dubbed an attempt at 'creeping consent'. Curiously, in the light of their new branding, they also had no interest in acquiring the site. What a difference a decade makes! Although not owners of the site, English Heritage later helped in its protection and are now among the most ardent champions of evaluation and project design.

The evaluation had three parts to it: compiling the research objectives, creating the resource model and studying the social context. The research objectives were gathered through seminars and committees, where there soon emerged an obvious disjuncture between Sutton Hoo's various 'clients': the historians, who wanted more 'early kings'; the art historians, who wanted more treasures; the prehistorians who were most interested in the Beaker settlement; and the medieval archaeologists, who, being mainly

Figure 3.9. The social context. (above) The newly formed Sutton Hoo Society, reprsenting local residents; (below) a visit of the Sutton Hoo Research Committee, left to right: Tom Hassall, Martin Carver, Marin Biddle, Birthe Kolbye-Biddle, Rupert Bruce-Mitford, David Wilson (Photo: Nigel Macbeth).

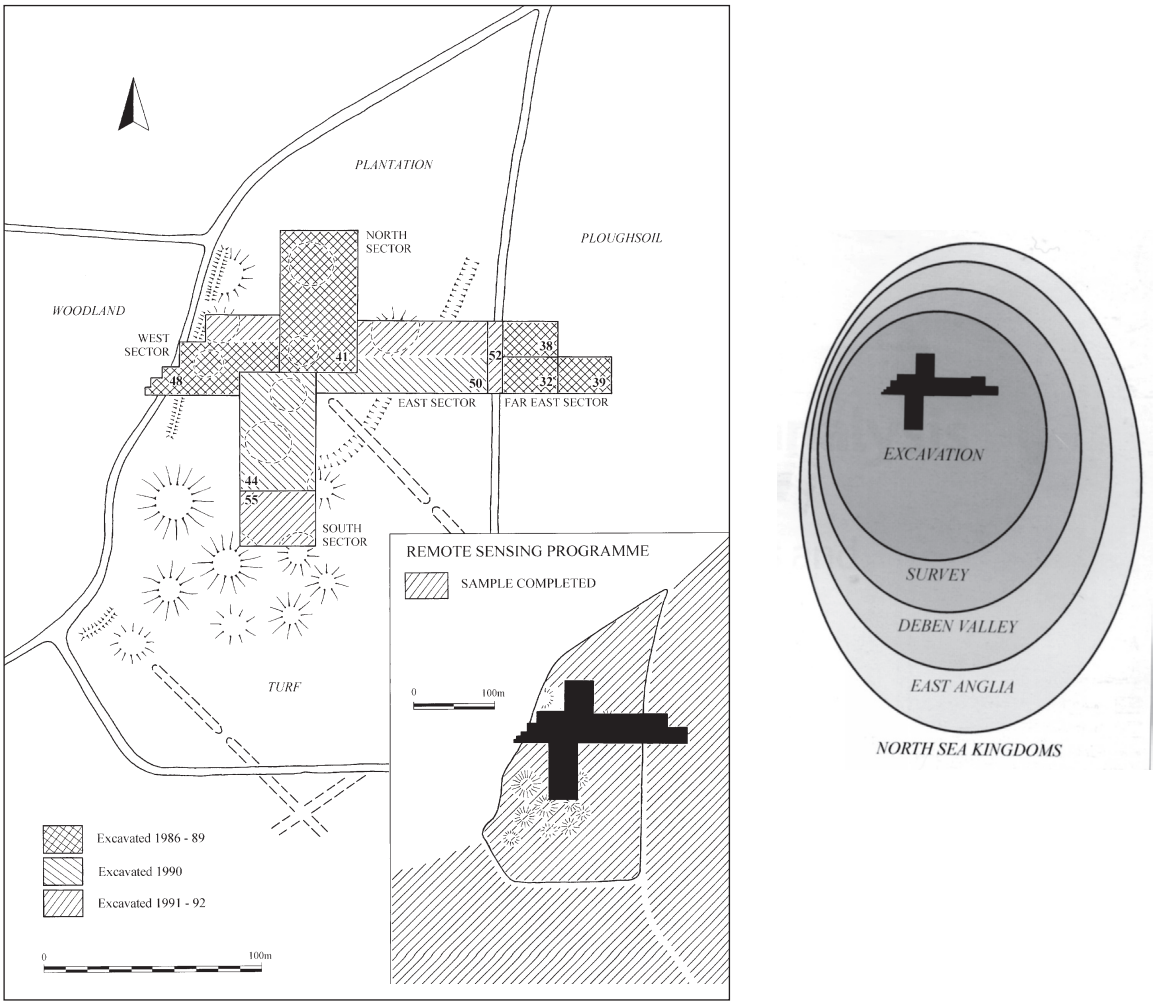

Figure 3.10. Campaign 5: Integrated design (right) Excavation and remote sensing; (left) nested surveys, local, regional, national and international.

processualists at that time, wanted some light thrown on the formation of the early kingdoms. Next came the resource model – the predictive modelling of the archaeology still underground, which would indicate how, and how far, the research objectives could actually be addressed at or around Sutton Hoo. The third arm of the evaluation was the social context, the assessment of the stake-holders, the interested parties, whose opinion and whose support was essential if the project was to survive and be productive. Among these, the state authorities were minor players, indeed at the time had washed their hands of the whole thing; but the County unit had a proprietorial attitude that needed to be mollified by acts of fealty and consultation. So had the treasure hunters, who considered their exclusion from the site to be 'elitist' since, as usual, no-one had bothered to explain the circumstances of their exclusion. As soon as I did, with an advanced copy of the project design, I was rewarded with a friendly editorial in *Treasure Hunting Monthly* with the underlying message 'back off lads'. A third group, which was

to prove invaluable in practical ways, was the cadre of local residents who had always considered Sutton Hoo their private property. As in much of England, your land is not what you own, it is where you can take your dog for a walk. Convened in a Woodbridge hotel, this group formed itself into the Sutton Hoo Society in 1984 and is still going strong.

Figure 3.11 Recovery levels in operation in Int 50 (Photo: Nigel Macbeth).

The project design was constructed by putting these three sets of factors together: the research agenda, the deposit model and the current social context. The output was to be two programmes: the research programme, comprising a six year excavation, and a survey of the Deben Valley; and a management programme, comprising rabbit-proof fences, mowing, exhibitions and public access. Also, beginning then was a long negotiation that finally came to fruition when the landowners, the Tranmer family, bequeathed the entire Sutton Hoo estate to the National Trust.

Perhaps the most important point to make about the whole design process is what was done with it. Having assembled the materials for the design as the result of three years of fieldwork and many private consultations and public seminars, the proposed programmes of research, excavation, survey, conservation and display were then published before work began (Carver 1986). Anyone and everyone could have their say on what was about to happen at Sutton Hoo. Disappointingly, this procedure has not been adopted by large research projects that came later, like Tintagel or it must be said, like

Figure 3.12. Excavations at Sutton Hoo in 1989, looking north. The rain machine is on the baulk on the right (Photo: Nigel Macbeth).

Figure 3.13. Excavating burials (left) A 'Sandman'; (right) Andy Copp taking samples for ICP mapping from the base of the Mound 2 chamber (Photos: Nigel Macbeth).

Çatal Hüyök. Hodder's version of multi-vocality can be seen as much more modest, relating only to people who work on the site, live near it or are already connected with it (Hodder 1999, 2000). Multi-vocality for me requires the publication of a proposed programme in detail and in advance – so as to argue for consensus between all parties. By the same principle, once the project design is agreed a 'social contract' exists between the archaeological team and the many potential stake holders in the site, both in its home country and abroad.

As for Sutton Hoo's subsequent history, I would like to pick out a few matters which I think are still pertinent, although much else has now become old hat. I would certainly urge the use of 'Recovery Levels' which predetermine the excavation techniques and records to be made. These proved invaluable for making comparisons and exercising quality control between one context another (Carver 1999; 2005, 25). Technically, we strove to improve visibility using chemical mapping, but I cannot claim much of a break-through except on a small scale (*i.e.* in Mound 2)(Carver 2005, 49–50, 58–64). Some progress was made in the surface mapping of the subsoil, which gives about 80 per cent of the total information in this kind of site (Carver 2005, 43). Everyone will agree that a sandy subsoil needs to be seen over a large area, but there are three requisites of which the modern profession seems to be dangerously shy: on-site water for spraying; a workforce of at least ten to prepare the surface by hand; and a tower from which to photograph the prepared surface. There is now clearly a problem in providing these things, since site after site appears without them, both on TV and in reality. Perhaps commercial companies cannot afford trowellers and find volunteers irksome; perhaps they are not

allowed to use towers for safety reasons. I cannot see why they should have to do without water. However until someone comes up with something better (and I look forward to hearing about it) these ingredients are essential for the inspection and subsequent excavation of the majority of features on sand. You will not see them in a trench and especially not a dry one, and of course if you do not see them, as part of a field assessment or mitigation, no-one is ever the wiser.

The question of the work force seems especially relevant here since it is a natural concomitant of the tendering process that a work force should be small, even when the area to be excavated is large. High precision survey means that every anomaly can be quickly plotted and subsequently rediscovered with ease, but it must be seen in the first place. It is not only a question of scale and timing as suggested above, it is also a matter of experience: excavators must serve an apprenticeship. With all their faults, the use of students, volunteers and employment schemes (a Manpower Services Scheme did excellent work at Sutton Hoo) does provide a way in for newcomers to broad sandy sites. It also provides a way for stakeholders, whether local or from overseas universities, to take part. It seems wrong to me that our profession, and its clients apparently, are reluctant to make use of the voluntary labour which would so patently give their projects added value, technically and socially.

My last indulgence is to speak yet again in support of the multi-concept recording system, in which records additional to contexts are made on site (Carver 1999). In this system, contexts are sets of observable components, and themselves belong to groups or sets (defined as features), while features belong to sets which I define as structures. Contexts, features and structures are thus independent of each other, and their records run in parallel. Contexts, features and structures represent a hierarchy increasingly rich in interpretation. The profession ditched these things for a few decades in favour of single-context systems, especially in towns, but excavators working on the flat find they cannot manage without them, and I quite agree with them (Lowe 2006, 32). Hodder (1999) rightly talks of on-site interpretation 'at the trowel's edge,' but like any other interpretation it needs recording in a disciplined manner, which is what feature and structure records are for. Similarly, there was no need to re-invent the site book and film monitoring since not only the excavators at Sutton Hoo, but the majority of research excavators in the twentieth century never abandoned them.

Figure 3.14. The Sutton Hoo site on completion of Campaign 5 in 1991 (Photo: Justin Garner-Lahire).

Conclusion

Are there messages here for the field archaeology of today and tomorrow? I believe so.
Archaeological contexts have no idea whether they are being looked for by researchers or commercial mitigators. But our chances of finding them depend on the recovery level, the effort put in. For this reason there is a standard, a 'British Standard' if you will, that curators should insist on. Obviously it is easier to come up with a winning tender if you dig with a minimal workforce, no piped water and no tower. Moreover no-one will be able to prove there were things you did not see. But if well-funded full-time research excavations have a point (other than research), it is to discover how to see more, and having done so, share the knowledge widely. These procedures should then be built into the specifications recommended – no, *demanded* - by curators from developers.

Archaeological excavation is not like doing history in a library; without a proper evaluation you cannot know what you expect to do; you cannot even fill in an application form for a grant. Since the planning and building industry has known this for decades and rightly insisted on evaluation and submitted design as a concomitant of planning permission, it is a mystery that research excavations can still get funding without it. I also do not understand how research excavations can get permission either in this country, or any other, without a conservation plan. That plan should be part of the Project Design at its outset, and the Project Design needs to be published in advance of any permission or funding being awarded. The prior publication of project designs is rare in the commercial sector, which I suppose is not surprising given its propensity to hide behind commercial confidentiality. But it is hardly ever done in the research sector either – at least I have never seen one. Was Sutton Hoo really so exceptional that alone it has felt the need for a full evaluation, a research agenda, a deposit model, an appreciation of the social context, and the additional need to expose the plan for multi-vocal comment before it starts?

This 'archaeography' has shown something of how the agenda, the techniques and the resources of archaeological investigation have changed with the times. At one level this is simply part of the broad-brush history of archaeology: looting in the irreverent age of the late sixteenth century; curiosity and kudos in the age of the antiquarian in the mid nineteenth. The excavation just before the war was driven by an inquisitive (but not acquisitive) landowner. After the war, as archaeology donned its self-important mantle, we seemed to be striving for the greater good, as expressed in research and in conservation. This was not formalised until Campaign no. 5, but then its logic was inescapable. No known site should ever be dug in its entirety. Our task was to work out the key questions that could be addressed at that site, at that time: if they required the total excavation of the whole site, they were the wrong questions and we should think again, for in this matter the ethical takes precedence over the academic. For the same reason, a research excavation should never require more than five or six years to complete. Twenty five years separated Campaign 3 from Campaign 4, and 12 years separated Campaign 4 from Campaign 5. During these intervals the agenda, the approach and the techniques changed radically. To decide to excavate any site for more than 10 years is as unethical as it is intellectually perilous (contra Cunliffe, this volume).

Students are often persuaded in their three years at university that excavation has something to do with theory – the Wheeler method pursues culture history, the random quadrat is the instrument of processualism and so on. But in my opinion excavation as a method of inquiry has its own theoretical basis. The objectives of excavation may arise from the research agenda, but its performance is rooted in its own contemporary society and has a lot more to do with people, methods and money than the

gentle theorist might wish. Moreover fashions in theory change during the time that an excavation is in progress. When Campaign 5 began we were, by and large, processualists. By the time it ended we were post-structuralists, seeing each burial as an individual expression of its own knowledge and time, its message bearing more of poetry than history (Carver 2000). But the trajectory decided by the Project Design stayed on its rails. In practice the role of theory in fieldwork is advisory, and when it becomes something more overbearing – as with single context or reflexive approaches, the resource and its potential are at risk of being diminished. No-one wants an excavation in which the developer decides the objectives and methods. But an excavation driven by theory is little better – like a film made by a novellist or an aeroplane piloted by a travel agent. Field archaeology has its own logic and duties that lie beyond its clients, whether commercial or academic.

References

Barker P. A. 1977 *Techniques of Archaeological Excavation.* London: Batsford.

Brøgger, A. W., Falk H. J. and Schtelig, H. 1917. *Osebergfundet: Utgiv av den Norske Stat.* Kristiana: Universitetets Oldsakamlung.

Brown, Basil 1974. Basil Brown's Diary of the Excavations at Sutton Hoo in 1938 and 1939, in R. L. S. Bruce-Mitford, *Aspects of Anglo-Saxon Archaeology. Sutton Hoo and other discoveries*, 141–169. London: Gollancz.

Bruce-Mitford, R. L. S. 1975, 1978, 1983. *The Sutton Hoo Ship Burial*. Three volumes, London: British Museum.

Carver, M. O. H. 1986. Project Design. *Bulletin of the Sutton Hoo Research Committee*, 4, 1–89. Woodbridge: Boydell.

Carver, M. O. H. 1998. *Sutton Hoo. Burial Ground of Kings?* London: British Museum.

Carver, M. O. H. 1999 Field Archaeology, in G. Barker (ed.), *The Companion Encyclopaedia of Archaeology*, 128–81. London and New York: Routledge.

Carver, M. O. H. 2000. Burial as poetry: the context of treasure in Anglo-Saxon graves, in Elizabeth M Tyler (ed.) *Treasure in the Medieval West*, 25–48. York: York Medieval Press.

Carver, M. O. H. 2005. *Sutton Hoo. A Seventh-century princely burial ground and its context*. London: British Museum.

Hele, N. F. 1870. *Notes and jottings about Aldeburgh, Suffolk*. London: J. Russell Smith.

Hodder, I. 1999. *The Archaeological Process. An Introduction*. Oxford: Blackwell.

Hodder, I. (ed.) 2000. *Towards reflexive method in archaeology: the example at Çatalhöyük*. British Institute of Archaeology at Ankara: McDonald Institute Monograph.

Lowe, C. 2006. *Excavations at Hoddom, Dumfriesshire. An ecclesiastical site in south-west Scotland*. Edinburgh: Society of Antiquaries of Scotland.

Nicolaysen, N. 1882. *Langskibet fra Gokstad ved Sandefjord*. Kristiana: Alb. Cammermeyer.

Rainbird Clarke, R. and Apling H. 1935. An Iron Age tumulus on Warnborough Hill, Stiffkey, Norfolk. *Norfolk Archaeology* 25, 408–28.

Redstone, V. B. 1897. Woodbridge, its history and antiquity *Proc*eedings *of the Suffolk Institute of Archaeology and History* 9, 345–58.

Sutton Hoo on-line archive: http://www.ahds.ads.ac.uk/downloads/suttonhoo (10 volumes).