

# The Development of Post-Medieval Archaeology in Britain: A Historical Perspective

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## Introduction

The post-medieval period was long regarded in Britain as something of a Cinderella of the archaeological world. However, as we approach the end of the first decade of the new millennium, it has become as routine a part of most generalist field practitioners' work as any other. The study of the archaeology of the latest period was previously regarded as optional, often being undertaken or not depending simply on the enthusiasm or lack of it on the part of the archaeologists in the particular area. The founding of the Society for Post-Medieval Archaeology (SPMA) in 1966 is an obvious watershed in the British national recognition of the subject. With the support of other specialists, SPMA developed from a relatively small organization, the Post-Medieval Ceramic Research Group, which was founded 3 years previously and whose main interest was specifically pottery (Anonymous, 1967; Barton, 1967). There were some initial concerns by members that the subject would not produce enough high-quality data year by year to sustain an annual journal, but the first issue of *Post-Medieval Archaeology* was duly published in 1967, and 2008 saw the journal's forty-second volume. Focusing on evidence for production in the period ca. 1750–1950, the Association for Industrial Archaeology was founded in 1973, with the first issue of its journal, *The Industrial Archaeology Review*, coming out in 1976. The overlaps between, or sometimes uniting, “post-medieval” and “industrial” archaeologies have been much

discussed, but any boundaries remain debatable, and this question intermittently resurfaces.

The appearance also in the 1970s of county-based and urban archaeological units across most of England and Wales, with less full coverage for Wales and Northern Ireland, took the majority of archaeological field investigations out of the hands of museums, where an appreciation of the value of investigating the post-medieval period, at least through its material culture, had been developing for some time among curators and other staff (e.g., Celoria, 1966; Noël Hume, 1955, 1956, 1962; Oswald, 1960; Oswald and Philips, 1949). The advent of the units, whose field staff were recruited mainly from recent graduates, arguably brought a temporary delay to the development of post-medieval archaeology, which was then outside the experience of most university-educated practitioners. For example, the most significant post-medieval site excavated in the central area of London in the 1970s was only investigated by the fluke interest of the 18-year-old supervisor (working immediately after having left school) at a location where the intention was simply to excavate a Roman cemetery. In the event, the post-medieval deposits proved very interesting to the excavation team, and these were the only ones examined in detail (the site was published as Thompson et al. [1984]).

Despite the launching in the past decade of a number of undergraduate courses specifically dealing with the post-medieval period, there is still a significant gulf in trained expertise for fieldwork as the earlier generation works toward retirement. An acute lack was reported by O'Sullivan (1999) a

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decade ago, but the situation has improved steadily since then. One can anticipate that for the next practitioners this should no longer be a widespread problem.

The publication in 1990 of David Crossley's innovative book—*Post-Medieval Archaeology in Britain*—concentrating on the period ca. 1500 to the beginning of “industrialization” in the mid- to late eighteenth century was another major milestone. The pace of development of the subject meant that it was followed by a second “handbook” just over a decade later—*The Historical Archaeology of Britain, c1540–1900*—the writers of which took ca. 1900 as their end date and featured evidence particularly from the north and west, while somewhat playing down the discussion of finds, as a balance to Crossley's emphases (Newman et al., 2001). In response to Crossley's focus on production sites, David Gaimster attempted the first overview of material consumption in southern Britain ca. 1450–1750 (Gaimster, 1994).

One characteristic of a mature subject is that it generates a variety of viewpoints. From the mid-1990s, following a long gestation period when there was almost no significant diversity of opinion save in details of interpretation of small points relating to individual projects or finds, this has become a healthy feature of the archaeology of the latest period. Most notable among the recent fresh approaches are those of Matthew Johnson and his following of university-based researchers (who are unrestricted by the need to work to the agenda of local-authority planning departments or central government directives) and for many of whom theory is of central importance (e.g., Johnson, 1996, 1999a; Tarlow and West, 1999).

A view that “industrial archaeology” can be defined as a separate chronological period within the discipline has recently been promoted as some practitioners have developed an interest in the living conditions and other social aspects relating to the workforce, as well as broader environmental and landscape studies (e.g., Gwyn and Palmer, 2005; Palmer and Neaverson, 1998; also see Martin, this volume). The terminology used to refer to the latest period and its precise duration continues to provide a subject for debate, with a wide range of possibilities now devised for both aspects. Again, the stimulating discussion engendered by this

debate is surely to be welcomed. This is surely a much better situation than when the main focus of contention for the subject was simply whether or not the post-medieval evidence at any given site did or did not merit any investigation.

### **From Limited Beginnings: A Main Focus on Artifacts**

This chapter inevitably draws heavily on the author's background working in the archaeology of London for over 30 years. It can be claimed, with some justification, that the post-medieval archaeology of the capital has been and more arguably continues to be central to several of the developments within the artifact study and fieldwork sides of subject, though there are of course many highly significant developments elsewhere in the United Kingdom that have little or nothing to do with the metropolitan area. As a colleague from the United States recently remarked to me, “You have far too many objects here”—and for all the rights and wrongs that others may perceive in this, it is why several London practitioners have tended to concentrate on this aspect.

Although it was exceptional until the middle of the twentieth century for any formal fieldwork to be undertaken at a professional level on deposits from the post-medieval era, this does not mean that archaeological attention of a kind was previously absent. Some early reports of early modern discoveries now considered significant were put on record almost anecdotally (e.g., the find of a seventeenth-century toy gun noted in a diary of 1799 [Lindsay, 1970:13]).

The first book entirely devoted to a post-medieval archaeological subject (apparently by a factor of almost 40 years) is a 1928 report written by Mortimer Wheeler on a large hoard of late-sixteenth- to early-seventeenth-century jewelry found in central London in 1912 (London Museum, 1928)—an eyeful and more of treasure in the popular sense. Interestingly, most pieces have not since been paralleled among finds anywhere, because they are from a social level that is neither sufficiently elevated to survive in

aristocratic and institutional collections above ground, but with real stones (though neither large nor of the best quality) set in precious metal, they are of significantly higher status than the few gold or silver items that are recovered from the ground tend to be.

As suggested in the introductory section, the earliest sustained, serious attention to post-medieval material from the ground came from ceramic historians' and collectors' enthusiasm for their subject rather than from a developed archaeological perspective. The site of the Chelsea porcelain factory, for example, was searched for evidence of wasters and industrial material within a century of its closure (Toppin, 1931). This was followed as occasional redevelopments allowed on several other known pottery-manufacturing sites. In 1946, the distinguished ceramics researcher, Garner (also from an art-historical background), mapped pottery factory locations for future investigation in London—in effect the earliest field strategy document for any aspect of the capital's archaeology of any period, and probably the first for any aspect of the post-medieval period in Britain (Garner, 1946).

It took a relatively long time (and some lobbying from the SPMA) for an archaeological post to be established for the City of Stoke-on-Trent in the British Midlands, which would be bound to have seventeenth-century and later ceramic production sites as an obvious priority for field investigation. This was achieved with a permanent archaeological post in the mid-1990s, following a period with a series of end-to-end contracts for separate undertakings. Previously, fieldwork had been carried out by the curatorial staff of the local museum, or by bringing in a team from the British Museum in London (see Cherry and Tait, 1980; Tait and Cherry, 1978). By the mid-1990s, however, an integrated archaeology service has come into being, combining curatorial and contracting roles, responding to all threats to Stoke-on-Trent's archaeology and influencing local development plans. At the time of this writing, there have been staff changes and restructuring, but the now-separate curatorial and contracting roles are firmly embedded within the City Council's establishment.

The archaeological volume on the potter William Greatbatch brought home the complexity of his production of a variety of wares at a single, small,

eighteenth-century factory in Fenton, Stoke-on-Trent, as established from stratified waste dumps (Barker, 1991; for a broader synthesis of some of the work in the Potteries District, see Barker and Cole [1998]). Similar studies might be produced on several other producers in the same area and elsewhere (see, for example, Coleman Smith and Pearson, 1988).

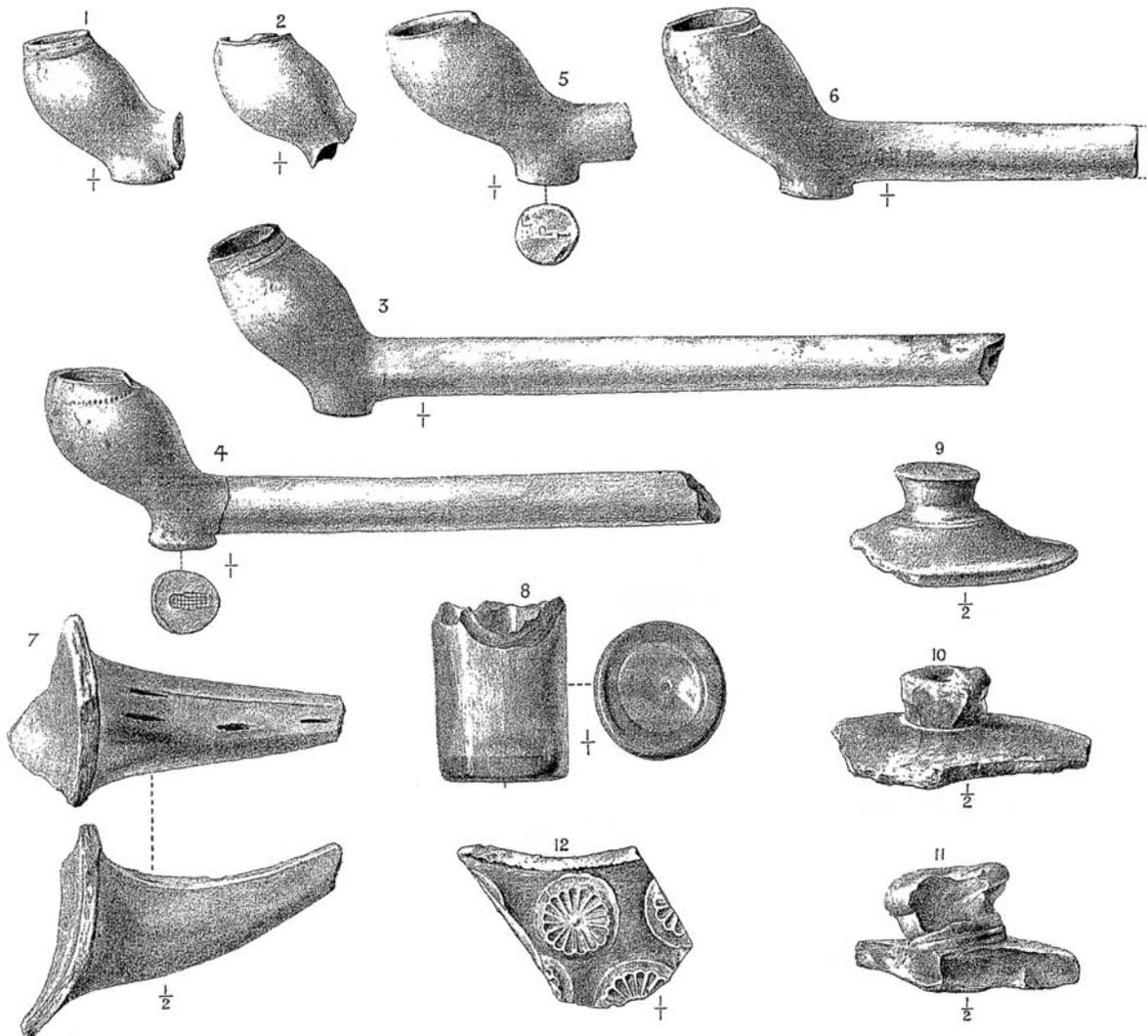
Two publications deal with evidence for the Limehouse porcelain factory in London's Dockland, investigated in the late 1980s (Drakard, 1993; Tyler and Stephenson, 2000). The first—a high-quality, hardback publication—was paid for by the ceramics trade, who were keen to see new field evidence, which would for the first time definitively identify the products of this short-lived factory, set out as soon as possible. It is impressive just how quickly new publications in the extensive collectors' literature on porcelain assimilated the results presented. The second much more detailed publication undertaken as part of a general archaeological post-excavation program is a more considered if less-lavish product in cardboard covers, which provides a more thoroughly digested view of the field evidence. It is remarkable that the key archaeological feature, the kiln, is located in different (adjoining) properties in the two publications, though this point appears not to be made explicit. This is a vital feature in archaeological terms, but nonarchaeologists might not necessarily be as worried by such a detail. It has been estimated that the few items of Limehouse ware that were provisionally identified before the excavation and in the event vindicated by it (most of them held by one of the main supporters of the first publication, and sold at auction a little while after the fresh information had been assimilated within the trade) commanded a price about 10 times what they would have done without the definitive identification from the fragmentary archaeological parallels found at the site. It may come as a surprise that the sale rooms might have some potential influence over archaeology through their ability to fund projects. The scope for fostering mutual interests is welcome, but as with all such arrangements, it is necessary to look out for any particular pitfalls that might arise from differing agendas.

Ceramics, routinely recovered in quantity at most excavation sites, inevitably continue to be an

important part of post-medieval studies (Fig. 1). The subject has, unsurprisingly, produced more archaeological monographs than any other aspect of post-medieval studies (e.g., Gaimster, 1997; Green, 1999; Pearce, 1992). The nearest thing to a sourcebook, though it only considers evidence up to the mid-seventeenth century, is Hurst et al. (1986). Gaimster and Redknap (1992) is wide ranging, taking a number of specific themes, with ca. 1900 as its end date, and the most recent synthesis is Gaimster (1994). English Heritage recognized a skills gap in the study of post-medieval ceramics, funding

training courses for professional archaeologists at Stoke-on-Trent in 1999 and 2001 and subsequently commissioning David Barker to produce a substantial volume, the working title of which is *Staffordshire Ceramics—A Guide to the Identification and Interpretation of Staffordshire and Related Ceramics, c. 1600–c. 1900* (Barker, in press).

Post-medieval glassware was long neglected, with only one significant, major publication (Powell, 1923) during the time the interest in the potential contribution of fieldwork to ceramic studies was developing. This is probably because the



**Fig. 1** Ordinary post-medieval finds, but some beautifully engraved, clay tobacco pipes and pottery from King John's Hunting Lodge, Tollard Royal, Wiltshire (from Pitt Rivers, 1890:Plate 17) (not to scale)

collecting fraternity could not get interested in glass waste, and the formulas of most early post-medieval glass meant that the fabric of finished vessels usually decayed in the ground to an unattractive brown opacity with unsightly corrosion or staining. The very translucence that was a major asset for this newly popular medium in its own time meant that even small blemishes were very prominent. To compound this, the inherent fragility of the material ensured that most discarded vessels broke into tiny, dangerously sharp fragments that could not even be glued together so as to hide the breaks. The very few early post-medieval finds that emerged from the ground in a state to appeal to collectors seem not to have been enough to stimulate a ready market.

A find of thousands of seventeenth-century glass drinking vessels in London at the start of the Second World War was given the opportunity only for a summary account to be published in a nonspecialist art magazine (Oswald and Philips, 1949), and so the best chance for the stimulation of publication studies came at a difficult time. As a result of all this, it has taken until after the millennium for a basic sourcebook for this staple category of archaeological finds to emerge for the period. Godfrey's (1978) very competent documentary history served to fill the gap until Willmott (2002) provided a specifically archaeological treatment. The scientific interest in the development of glass furnaces meanwhile became a thriving aspect of field studies. It featured, for example, in the first volume of *Post-Medieval Archaeology* (Crossley, 1967). The most recent of several syntheses is Willmott's (2005) *A History of English Glassmaking AD 43–1800*.

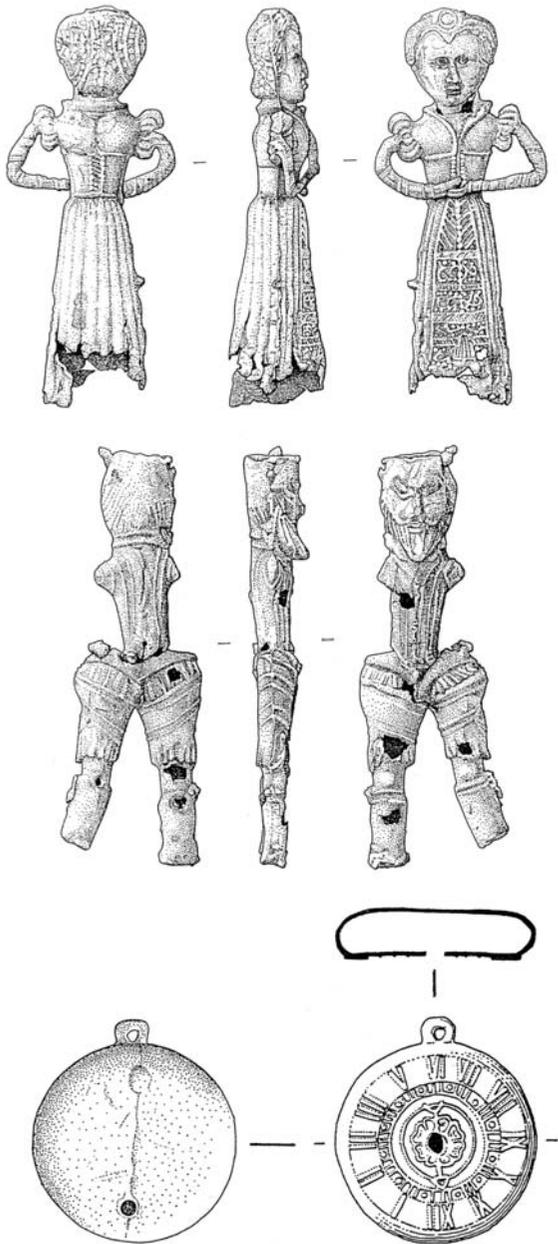
The third major branch of post-medieval material culture, with an enthusiastic specialist group that sustains a periodic newsletter, is the clay tobacco pipe, arguably the most intensively studied of all artifacts throughout the period (see Fig. 1). With an extensive specialist literature, this field of study has a momentum and an international—indeed worldwide—network all its own (see, for example, Atkinson, 1975; Davey, 1979; Higgins, 1999).

Ivor Noël Hume, who figures at several points in this chapter, began his archaeological career working for the Guildhall Museum, and famously championed fieldwork on post-medieval sites, until,

among other reasons, disappointed at the slow progress being made toward a promised publication on post-medieval ceramics, he moved across the Atlantic to continue this line of work in Virginia. His remarkable beacon publication on post-medieval finds of all kinds, *A Guide to Artifacts of Colonial America* (Noël Hume, 1969), which includes as examples many finds unearthed in London, significantly remains in print as a place of first resort for both European and American students more than 35 years after its first appearance, especially for material culture dating prior to ca. 1850. More than a generation after its first publication, it has no rival in single covers as a general guide to one of the main aspects of the archaeology of the British and their colonial empire. No subsequent publication has matched the range of material covered at a basic level with comparable authority.

Progress has inevitably been made, though slowly, on specific themes within the vast range of metalwork. Dress accessories have surprisingly not received a full consideration, and the literature is very scattered (e.g., Egan and Forsyth, 1997; Egan [2005a] includes a range of early post-medieval accessories). Ushering in the previously neglected archaeology of childhood, early toys (Figs. 2 and 3) have also received detailed attention from an archaeological perspective (Egan, 1996; Forsyth with Egan, 2005). Spoons of pewter and brass, illustrated mainly by excavated specimens from the sixteenth and seventeenth centuries, were the subject of an early study (Hilton Price, 1908), which has now been superseded (Homer, 1975). The most recent synthesis of excavated pewterware in general has come from the collectors' ambit (Homer and Shemell, 1983), while a recent monograph on bronze cooking vessels was written about a sale-room collection supplemented by detailed historical research into the foundries and some archaeological evidence, together with scientific analysis of the alloys, producing a valuable synthesis by collating evidence from all sources (Butler and Green, 2003).

For metal artifacts of the eighteenth and nineteenth centuries in general, the fullest guides currently available are written by metal detectorists who have similar research interests to those of archaeologists (see Read, 1995, 2001). The archaeological side has been slow to give much attention to everyday metalwork of this date. The two volumes



**Fig. 2** Late-sixteenth-century hollow-cast, lead/tin Tudor children's toys in the shape of human figures, found in London (drawings by Terry Shiers) (not to scale)

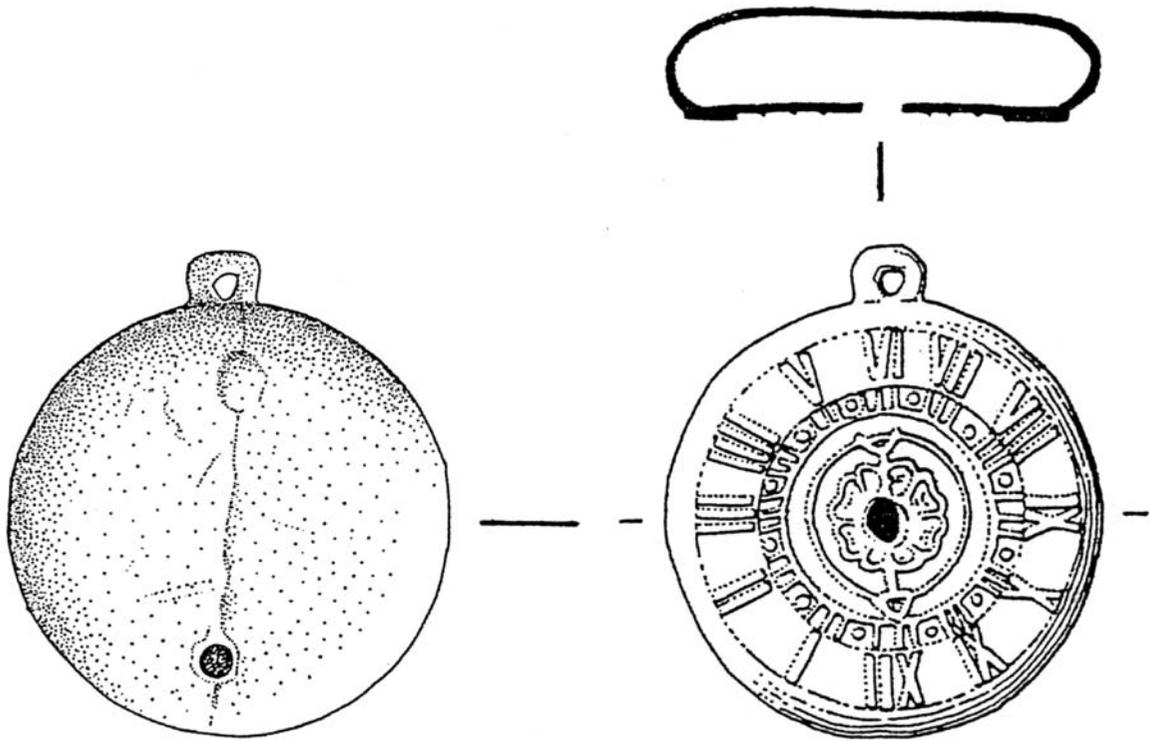
on finds from Winchester are among the few major urban finds monographs to include items from the 1700s and 1800s on the same terms as earlier ones, but the small sample and lack of parallels fully published in similar detail restricted the scope for useful inference (Biddle, 1990). Way ahead of his

time (indeed, still to find a follower for his most innovative work in the 1970s) was Francis Celoria. Some of the nineteenth-century material culture he insightfully tackled from an archaeological viewpoint, such as early service fittings in the buildings partially demolished during the redevelopments that gave most other investigators the occasion to concentrate on the underlying deposits from earlier eras, still await further enthusiasts to come forward (Celoria, 1974).

Leather and textiles, less frequently encountered than the preceding materials, inevitably have a more limited but important literature. Leather has been analyzed more frequently than textiles, but the chronological cover is still patchy (see Gardiner, 2005; Nailor, 2005). Analysis of early post-medieval dyes in excavated textiles has produced some surprising results, with colors from lichens as well as the more obvious "industrial" plants (Pritchard, 1992; Walton, 1981, 1987).

The marking of products for quality control is another significant aspect within artifact studies. Clay pipes, pewterware, late-seventeenth-century glass vessels, textiles, and precious metals had different traditions of marking, and these markings occur on many excavated items (Endrei and Egan, 1982; for lead seals on textiles, see Egan [1995]). This byway of artifact research has, despite the massive efforts of documentary historians, produced much new information that would not have come to light without finds from the ground.

The study of production—industrial archaeology in the broad and simplest sense of the term—was an obvious and very important theme right from the first issue of the journal *Post-Medieval Archaeology*, in which the first two papers dealt, respectively, with the manufacture of ceramics and glass (Brears, 1967; Crossley, 1967). Manufacturing sites and other evidence of production processes remain a very significant part of post-medieval studies, as every issue of this journal demonstrates. The detailed charting of trade, primarily through the distribution of ceramics, glass, cloth seals and some categories of metalwork, was another early theme, which continues to develop as an integral part of finds studies. The surprisingly frequent instances where the actual objects supplement or even contradict the wisdom received from the long-studied documentary side are among the



**Fig. 3** Late-seventeenth-/early-eighteenth-century stone mold for making pewter toy watches, found in London (drawn 1:1, drawings by Nicholas Griffiths, Museum of London Accession Number A20772)

most useful contributions made by post-medieval archaeology. The study of the ceramic trade of Exeter, for example, in which the origin of a variety of goods was shown to differ from documents that gave only a ship's last port of call, often one or more stops after the one where a cargo was taken on board, is a salutary warning (Allan, 1984). Finds from wrecks are important, often very closely datable assemblages (for a synthesis, see Redknap [1997]). A range of unparalleled evidence from a period generally poorly represented elsewhere comes from the 1545 wreck of the *Mary Rose* (see papers in Gardiner [2005]).

The study of cargos of merchant vessels has developed as an adjunct to the field of shipwreck archaeology, which is avidly pursued in its own right for the post-medieval period, as for others. Excavations in ports and dockland areas are recognized as providing links between ships, through waterfront installations like wharves, and through exotic goods (Divers, 2004; Douglas, 1999; Killock

and Meddens, 2005; Tyler, 2001). Several assemblages of materials from waterfront areas feature not just indications of import/export wares but the first couple of properties inland regularly furnish foreign small change and other "souvenir" items like coral and ethnographic items. The homes of retired sailors within sight of the water may partly explain concentrations here of exotic pottery and other objects that are anomalous when set against more widespread assemblages farther inland.

### **Buildings, Structures, Landscapes, and Environment**

Vernacular buildings have been a continuing theme in post-medieval studies, with the specialist Vernacular Archaeology Group catering specifically for this lobby (see Quiney [1994] for a recent synthesis). As elsewhere, new, wider and theory-based interest

has expanded the scope of these studies in recent years (e.g., Johnson, 1993). Farm buildings have seen relatively little sustained work on post-medieval phases and more is needed across the country to elucidate regional developments (e.g., Cunliffe, 1973). Aristocratic houses and royal palaces have long been the subject of mainstream archaeological research (e.g., Biddle, 2005; Thurley, 1999). This is true, too, of castles and forts, many of which have played significant roles in national history. Since the nineteenth century, the study of early modern fortifications was considered an unarguable extension, along with Tudor defenses (e.g., Biddle et al., 2001) and (English) Civil War works, taking the tradition of studying medieval remains into the later period without any need to consider its validity. Here too the focus of study has moved on (Coad, 1994; Johnson, 1999b). The Civil War in particular has been a focus of the study through archaeology of warfare in the post-medieval period, with investigations of battlefield sites as well as defensive and offensive works (e.g., Ellis, 1993; Mayes and Butler, 1983).

Theaters of the late sixteenth/early seventeenth century (almost exclusively a London phenomenon from the archaeological perspective) have been the subject of some of the most high-profile fieldwork for the entire period (Bowsher, 1998). The excavation of the Rose Theatre from the period of Marlowe and Shakespeare captured the public imagination with the dispute over what was best for the remains. The site had been considered for redevelopment in the 1960s and was then judged too sensitive for that particular project to go ahead. Bad advice seems to have led to the point during the works undertaken in the late 1980s, where I was able to follow daily the unfolding story of on-site protests by leading actors, from a local English-language newspaper, while on holiday in Mainland China. This sorry tale dramatically demonstrated to the public worldwide the folly of not taking sympathetic advice pertaining to the archaeology of some remains of post-medieval date. More positively, the investigation at the Rose and limited fieldwork at the Globe established conclusively the locations of both playhouses (previously a much debated question for each). The new information opened up a range of detailed fresh points of contention about

the precise nature and use of the buildings, structures, and spaces available for acting.

Churches and religious houses have been prominent subjects for above- and belowground investigation from the nineteenth century, with their individually evolving structural changes (e.g., Parsons, 1994). Evidence for the Reformation, particularly in the case of religious institutions that were closed down, is a specific theme, with particular attention being paid to the subsequent adaptation of the structures for other purposes (Schofield and Lea, 2005; Thomas et al., 1997). Originally a facet of these investigations, burial studies have become very prominent in recent years. The publication of the investigation of the human burials in the vaults at Spitalfields just outside the City of London was very influential (Molleson and Cox, 1993; Reeve and Adams, 1993), stimulating several similar projects (Brickley and Buteux, 2006). Investigations of burial grounds not associated with religious structures and ones for nonconformists are also routine fare now. More specialized related topics such as gravestones and other memorials are increasingly carried out across Britain (e.g., Mytum, 2006; Tarlow, 1999).

Garden archaeology has been one of the striking growth areas in British archaeology from the 1980s onward (see overviews by Currie [2005], Dix [1999], and Williams [1999]). The nature of the evidence means that larger early modern period horticultural remains have been emphasized to a greater degree in these innovative studies, which are often conducted in the context of the accurate restoration and appropriate replanting of upper-class gardens of a specific period. The study of wider landscapes and the dynamics of large, aristocratic estates is an extension of this kind of work. Landscape studies are fortunately increasing, not least on the part of archaeologists concerned with industrial complexes. Several papers in a recent monograph deal with this aspect (Barker and Cranstone, 2004).

Environmental investigations came late to post-medieval archaeology, and appreciation of their value remains very patchy. A paper published in 1981 on the contents of a London cesspit, which included the environmental evidence (plant remains and animal and fish bones), was at that time an isolated instance among post-medieval studies (Vince and Egan, 1981). In the first issue of *Post-Medieval*

*Archaeology*, there was an appeal for bones from the period to study, because “Surprisingly little is known about the domestic livestock of this period” (Noddle, 1967). The sustained work of Philip Armitage (based in London and then the United States) through the 1970s and 1980s has shown the way for the present generation’s now almost-routine inclusion in appropriate programs of archaeological investigation of early modern period animal-bone studies (Armitage, 1978, 1982). I recall an instance in the early 1980s, when the scope of what became the SPMA’s priorities document (SPMA, 1988) was discussed at by the organization’s leadership, being greeted with considerable skepticism on the part of several of the then-leading advocates of the early modern period that plant remains merited mention. It was presumed they would have no significant contribution to make to the study of the period. There is now a limited appreciation and inclusion of botanical studies, but environmental studies overall still receive far from satisfactory cover. Until very recently, it is probable that more was published on post-medieval plant material from London than elsewhere in the United Kingdom put together (e.g., Giorgi, 1997, 1999). The reports on environmental evidence and medicinal provision from the wreck of the *Mary Rose* (1545) may be prominent enough to make the general point that these studies are worth undertaking for the early modern period, when survival allows the right questions to be posed (see Gardiner, 2005).

### **Growth: Multidisciplinary Approaches, International Views: Tackling Colonialism and Archaeologies of the Eighteenth to Twentieth Century**

Gaimster (1994:304–305) has stressed, in the context of material culture, the need for studies that take a multidisciplinary approach. Now that a prolonged period of definition of just what the material culture comprises and of working out dating frameworks for the most frequently encountered post-medieval finds has produced basic parameters (though there is more to be done) the most stimulating advances are being made by combining information from different

specialists and viewpoints. Thinking further along these lines, a series of monographs have resulted from conferences held by the SPMA jointly with other special-interest groups in order to tackle broad themes more effectively than from a single point of view. These include collected papers on the Reformation (with the Society for Medieval Archaeology; see Gaimster and Gilchrist, 2003), industrial archaeology (with the Association for Industrial Archaeology; see Barker and Cranstone, 2004), and early modern archaeology in Britain and America (with the U.S.-based Society for Historical Archaeology; see Egan and Michael, 1999). Another recent volume from a different source combines papers on the production of ceramics and glass in Venice, Antwerp, and England, seen from an international perspective, which reflects the movements of the skilled workers who made these products from Italy to northwest Europe (Veeckman et al., 2002). A similar marrying of evidence from two countries, this time across the Atlantic and in the context of production and consumption, underpins an earlier volume on tin-glazed ware (Noël Hume, 1977).

Links between British and North American archaeologists were already strong in the 1960s, as is apparent in the first issue of *Post-Medieval Archaeology*, with a paper from the United States (Noël Hume, 1967) and a Canadian contribution and a note on evidence from Australia in the next issue (Birmingham and Hewitt, 1968; Sutermeister, 1968). The worldview advocated by several commentators (e.g., Orser, 1999) has come to be much more in evidence in the journal of late, with interest in Continental European, as well as African, Caribbean, and Antipodean archaeology, though engagement with Asian evidence has yet to receive similar emphasis. Links with southern Africa, too could be strengthened. In the past decade a surge of interest in colonialism in Africa and in the Caribbean has come to the fore (e.g., Leech, 2008). This interest applies both to the colonies themselves and to the estates in Britain of the colonist landowners, and also to the buildings and infrastructure of the cities, all of which benefited from the wealth these enterprises raised. A new awareness of how slavery in the colonies was central to the funding of many familiar landmarks and much else in London, Bristol, Liverpool, and elsewhere, not least the country houses of successful merchants, has recently appeared, and

this is certain to become an even more prominent theme in future studies (e.g., Dresser, 2001:96–128; Lawrence, 2003). The colonial plantations in Ireland meanwhile continue to provide material for investigation of a somewhat different phenomenon (e.g., Brannon, 1999; Horning, 2006).

It has taken time for the late 1700s and the most recent two centuries to come to be widely considered by archaeologists in the United Kingdom (apart from industrial archaeologists focused closely on production) to provide significant information. Routine fieldwork, with all the considerable efforts involved, has dragged behind college-based researchers. Critics of the development of the archaeological investigation of the past two centuries, when explicit in conversation, tended to use exactly the same accusation as that formerly leveled at later medieval archaeology: at such a late date our subject is the handmaiden of written history, and an expensive way of telling us what we already know. Recent initiatives have made a virtue of selecting a range of nineteenth- and twentieth-century sites, buildings, and other subject matter for study. This trend has been brought still closer to the present by others (Buchli and Lucas, 2001). A major project that will test how readily traditionally collected finds can illuminate particularly communities of the eighteenth and nineteenth centuries is currently underway in London (Jeffries and Hicks, 2004). It is not true, however, that post-medieval archaeology had previously not ventured into the era after 1750 (see e.g., Dawson, 1972; Corbishley, 1976; Emery, 1999; Guilbert, 1975; Webber, 1991). As a former compiler of the annual summary of fieldwork for *Post-Medieval Archaeology* through most of the 1980s, I was only too willing to include such material, but apart from occasional Napoleonic-period fortifications and a few sites investigated because they had been mistakenly identified when fieldwork began as being much earlier, very little was submitted—an accurate reflection of the amount of genuinely archaeological work tackling evidence this late that was then being carried out by those who submitted reports. Investigations of World War II aircraft wrecks, for example, would have been welcomed (and indeed were sought), but the fieldwork considered at that time tended to have been undertaken more with treasure-hunting/souvenir-retrieval motives than in a genuine pursuit

of new knowledge. By contrast, more recently the Council for British Archaeology's long-term national project "The Defence of Britain" mapped the survival of thousands of fast-disappearing twentieth-century defenses through the efforts of hundreds of volunteers, and in doing so established the validity not only of the subject but also of mainstream official support for the investigation of field evidence from the 1900s on the grand scale. It is encouraging to see that the archaeology of both World Wars, and the Cold War—including traces left by peace camps, are now being widely taken seriously, with considerable efforts being devoted to recording the field evidence (e.g., de la Bedoyère, 2000; Dennison, 2002; Holyoak and Schofield, 2002; Tuck and Cocroft, 2005).

### **The Position Reached: Recent Developments and the Future**

Periodically, members of the profession produce considered statements of future directions and intentions, which necessarily involve reviewing progress to date. These well-intentioned syntheses are very difficult, if not impossible, to get right, as there are many potential users with a variety of different needs, ranging from purely academic to those who oversee the archaeology of a particular area (who may want just a definitive list of categories of sites, or even a seriated list of named ones in their area, that are considered worth excavating/investigating or preserving, to back up their daily need to justify work under their purview). In 1988, SPMA published *Priorities in Post-Medieval Archaeology*, a document with national scope. It was made clear in this document that there was no simple league table of desiderata that could be listed in order of general "need" or academic urge to investigate—the reality is a much more complex requirement to assign resources available to cater locally as needs arise, perhaps within a previously defined category, but in reality often presenting fresh, previously unspecified aspects that may sometimes be of unarguable significance to the development of the subject. The society is currently producing an updated, more thematic but much briefer statement of broad desiderata, as the current fashion is, but progress on

this in the era when almost every full-time archaeologist in Britain is greatly overworked by the day job alone, such an additional task is extremely difficult to orchestrate. A series of regional archaeological reviews are currently in the process of preparation across England. The first of these to appear was that for London (Nixon et al., 2002). It looks as if these documents are going to be extremely varied in their coverage of the latest period, depending on who has contributed to them. Some have had post-medieval archaeology or aspects of it inserted only during the final consultation period, while in others this was included from the start. It has been alarming to see the scale of some omissions, at least before the consultation stage. It still seems to hold true that post-medieval archaeology is given due consideration among senior practitioners largely through the enthusiasm of some individuals, rather than being seen more widely as a routine part of the subject as a whole, despite its prominence now in the working lives of most generalist practitioners in the field. These documents are undoubtedly together going to include some of the most advanced ideas about post-medieval archaeology, so a review publication drawing together all their relevant parts into one place would be most interesting.

Going beyond the limited archaeological community, the results of a major consultation exercise initiated by English Heritage in a project about attitudes to heritage were published at the millennium (Department for Culture, Media and Sport [DCMS], 2000). It is in many ways encouraging for post-medieval archaeology that this revealed the built environment of the recent centuries to be the most prized aspect of people's everyday appreciation of their past. On the other hand, it also showed up that for the majority of those in Black and Asian communities little if any connection was felt with this surrounding heritage, to the point for a significant number of taxpayers of a perception of exclusion from it. This highlights one of the most significant challenges for the future of the archaeology of the recent past, and one which is only beginning to be tackled.

A noticeable trend over the past 30 years has been the slow diminishing of the contribution that has been made by the "amateur" sector as a whole, while the "professionals" have steadily taken on

more of the subject matter. In the United Kingdom, the government currently places great emphasis on the voluntary sector and the provision of broad "access" for everyone to follow a wide range of activities. Archaeology has not been slow to draw several strands together, with the appointment of "community" archaeologists whose duties include involving the local populace (not least the children and metal detectorists) in practical undertakings to foster study of the past. The Portable Antiquities Scheme was launched in 1997 to create a publicly available archive of "archaeological objects" recovered from the ground by anyone other than those engaged in formal archaeological excavations (i.e., "members of the public")—in practice mainly metal detectorists. Ten years later, this government-sponsored program has some 50 employees across England and Wales, with hundreds of thousands of objects now recorded (see Egan [2005b] for the post-medieval period; for additional information, see the database under <http://www.finds.org.uk>).

Some of the recently devised but increasingly widely practiced branches of archaeology, within which the post-medieval period is prominent, do not necessarily produce results that routinely fit into the usual academic framework. The formal excavation of debris in a university archaeological department's van (Newland et al., 2006) suggests that the notion of imposing boundaries on the subject matter of the archaeology of the recent past may be unwise. In this case, the project was seen as a novel and striking means of teaching the fundamentals of stratigraphic excavation rather than providing any answers within the usual post-medieval agenda. Forensic archaeology is very largely preoccupied with recent crimes, whether individual murders or large-scale war atrocities (though its methods and highly focused problem-solving preoccupations within a given legal framework can be applied to evidence of any date). Several of the "community archaeology" projects have deliberately dealt with the buried remains of the very recent past, sometimes creating an archaeology of living memory (at least for some older participants). This enhances the involvement of local residents, in the right circumstances giving a uniquely keen, even poignant, association with what is being revealed. The jury is probably still out, but there are claims that such projects will at best only accidentally and very rarely answer the

wider, “big” archaeological questions, if they tackle these at all. As one of the archaeologists (Blair, 2006) involved with the “Archaeology of the Blitz” project at Hackney, north London, remarked, “little can be learned from the excavation that is not already known. But that is not the point. The role of Second World War archaeology . . . is to use excavation as a powerful educational tool, a nexus for interdisciplinary scholarship, and a source of social cohesion and a sense of shared community history.” The basic aim to involve local communities with their own archaeology can only be a good thing, and post-medieval archaeology inevitably enjoys a near monopoly in this particular branch of local studies. It is ironic that (with honorable exceptions) it has taken an external stimulus from on high to draw the attention of those engaged full-time in the subject back to the contribution that amateurs can make and—not least for post-medieval archaeology—have made in significant measure.

The recent period of dramatic expansion in the subjects tackled by post-medieval archaeologists has seen the definition of new areas and the wide acceptance by the profession of some themes that were previously pursued by just a few enthusiastic individuals. This appears to have come about largely through the parallel thinking of many individuals, rather than from some debate or direction from universities, though the government has played a significant part in some of this, as noted above. It has become difficult to think of a theme that has left standing remains or traces in the soil that is not now within ambit of post-medieval practitioners. Sporting infrastructure has recently come into the recognized fold—ironically in the very week the old Wembley Stadium (built of reinforced concrete in the early 1920s) was demolished without the kind of detailed record that is being advocated. There are some new organizations, the Archaeology of Zoos Network (see O’Regan, 2002) and the Society for the Study of Childhood in the Past, which were founded largely by archaeologists, are multiperiod and international in scope. It will be interesting to see which of these many emerging themes develop a sustained theoretical base, with widely recognized specific aims and priorities, and which ones may prove simply to be isolated, one-off projects without any specific wider framework either to guide the ways the evidence is tackled or

even to encourage what has been recorded to be set alongside more traditional archaeological data. A few outright “wacky” projects may get carried out, but overall the expansion is taking post-medieval studies into valid new areas to tackle fresh material of real interest.

While most of the developments discussed above give cause for some satisfaction, there is still abundant scope for building further on these achievements. The latest period is certainly coming to be routinely considered for field investigation and research, at least by the younger generation. County and regional summaries of archaeology published very recently still tended to end with 1500 or 1540 c.e., occasionally with a summary of the area’s “industrial archaeology” in a last chapter. A recent straw in the wind is a county archaeology for Sussex, which does indeed go on to ca. 2000 (Rudling, 2003). It would be good to see the next generation routinely taking its local archaeologies in a single synthesis to the turn of the millennium.

One measure of success in any subject is surely the sustaining of a regular academic journal. The initial concern (noted above, in the introductory section) that the subject would be unable to continue generating a critical mass of scholarly papers to keep the proposed journal *Post-Medieval Archaeology* going can now be seen in the long term to have been unfounded. The change in 2003 (Volume 37) to a twice-yearly format to cope with a significant increase in copy must be a clear indicator of solid achievement. Another encouraging indication of expansion is the appearance in Ireland of a new sister organization in 2001, the Irish Society for Post-Medieval Archaeology, which holds an annual conference (some of the papers given at these will be published together as monographs) and has its own newsletter.

Looking back at the “editorial” founding statement at the start of the first issue of *Post-Medieval Archaeology*, which noted the roles of faith, hope, and charity in the launching of the new journal (Anonymous, 1967), the faith and hope of the founders have surely been vindicated. SPMA is now a registered charity that is obliged to provide a range of services for its members and others within the subject—a duty the organization’s leaders take very seriously. While the debate about the precise duration of the post-medieval period (touched on in the

same editorial) continues without any sign of consensus, the Canadian paper intended for the first volume was in the event published the next year in the second one (Sutermeister, 1968). If only all the delays to the dissemination of worthwhile information in our subject were so brief—the field has truly grown to become a vast one.

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