

## Notes on the South-East Research Framework public seminar on the medieval period (03/11/07)

Chair: Joe Flatman

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**Notes:** Jake Weekes

### **Some recent developments in the medieval period in South-East England**

Jake Weekes (JW)

In order to produce a resource assessment for the medieval historic environment in the South-East, JW would first consider the approach taken by Peter Brandon and Brian Short, who published in 1990 what remains the most recent regional synthesis for the period. Any new assessment of the resource will have to be relevant not only to the large amount of new data that have been added since 1990 (as well as assessing the effectiveness of current mechanisms for regional comparison of that data), but will also need to take theoretical developments into account. In fact, a primary purpose of reassessing the resource is in order to see the degree to which the data can afford the new types of questions we might now wish to ask. These questions can perhaps be characterised as increasingly ‘anthropological’ rather than being historically formulated. JW would also take this opportunity to raise some further questions, specifically in relation to cultural identity and Anglo-Norman material culture in the region.

Clearly developer funding conditions have meant that enormous amounts of new data have been produced since 1990, although, as has often been noted, not consciously in line with any specific research questions. Apparently in response to this, a perceived dichotomy between ‘developer funded’ and ‘research’ archaeology has developed, and seems often to be promoted by archaeologists themselves (who also frequently identify themselves with either ‘fieldwork’ or ‘research’ camps). An example of such reification can be seen in a recent review of medieval archaeology by Christopher Gerrard, who states that:

‘Developers pay to record the archaeological deposits which might be damaged by their schemes and diminishing government funding has been channelled into other areas such as post-excavation costs...’ (Gerrard 2003, *Medieval Archaeology: Understanding traditions and contemporary approaches*: P184).

This would actually seem a most alienating statement. Surely it is important that all archaeologists (and indeed any developers who might fund their work) are able to recognise that they are contributing to a research discipline that seeks to understand past societies via material remains, that data collection is never without a theoretical framework (whether consciously recognised or not), and that analysis and interpretation are as much part of the work as data collection? JW hoped that one of the key benefits of the research framework would be to reduce the culturally perceived gulf between ‘research’ and ‘data collection’, and provide an effective

means by which more detailed research questions can be communicated and inform methodology in the future (whatever the funding source).

Nonetheless, it is worth noting with Gerrard (2003: Chapter 6) and others a main criticism of the developer funded mechanism of data collection to date, which is a tendency to place strategic emphasis only on those areas specifically under a direct threat from the impact of either ground works or building alterations/destruction. This has resulted in a large number of unconnected small-scale excavations with many of the results being practically lost in a burgeoning corpus of 'grey' literature. This limitation (from a research perspective) can be demonstrated with all the accepted general classes medieval archaeology, from castles, towns, rural settlement, ecclesiastical settings, buildings generally, and all facets of the highly complex archaeological record of the material culture of the region at this period.

Connected with the above, there is the genuine lack of a coherent regional form and fabric type series for ceramics (already highlighted by prehistoric and Roman period specialists earlier in the seminar series). This makes regional comparisons very difficult or impossible at present, as a number of different classificatory systems exist (and are growing) for the same material. Similar problems for regional comparison extend to all other artefact types, not to mention further specialist areas such as environmental analyses and archaeological science. The competitive process of much, if not all, current archaeology (and this could include academic competition for funding and jobs within the context of the Research Assessment Exercise) has not lent itself to effective mechanisms for sharing and progressive analysis of data on a regional scale. Some recent large-scale excavations, however, as well as environmental analyses, have shown the level of detail as well as contextualisation we might be missing through (often under-funded) 'keyhole surgery' in urban settings in particular. (By way of example, Mark Houliston reported on findings from the Whitefriars excavations at Canterbury and Enid Allison on environmental analyses of material from Townwall Street, Dover, later in the session.)

Developments of theoretical frameworks (and therefore of new types of research question we would wish to ask of the regions' historic environment resource) have also been the focus of much archaeological activity since the Brandon and Short volume was published, and much of this theory driven activity has been, and remains, in promoting a more developed anthropological perspective on archaeological evidence.

Brandon and Short's 1990 analysis starts from a large-scale, 'systems' based approach to the region which, while it certainly has some value as a starting point for an argument about generalities of regional society and economy, would be considered too deterministic by many archaeologists today (and probably would have been by many more historians in 1990). The book approaches the region from a fundamentally economic perspective, in terms of broad 'mechanisms', 'structures' and 'dimensions' of intra-regional 'dominance' and 'dependence', the pattern itself being shaped by the particular geological structure of the region as a determinant of land-use and tenure (1990: Figure 1.3). Essentially, this model is one of a 'long durée' exploitation by the dominant coastal fringe of the dependant Wealden and Greensand interior (a pattern which, it is argued, can be traced from the prehistoric period well into the post-medieval).

Within this generalising scheme, Brandon and Short's work is also noticeably historically driven at all levels with archaeological material tending to flesh out the historical. This can produce very interesting results for particular historical events. So, for example, it is possible to map the progress of the Conqueror's armies throughout the region based on the severely diminished values by the time of Domesday of land through which they had 'passed' (1990: Figure 2.1).

Yet it is interesting that there is next to nothing in Brandon and Short's discussion of the earlier Middle Ages about the development of any collective Anglo-Norman identity/language/culture in the longer term. In fact, only one short paragraph seems to hint at a complex subject that is surely of much interest from a post-colonial perspective:

'Normanization should not be thought of as abruptly destroying English culture. St. Augustine's Abbey at Canterbury under its abbot Aethelwig was one of the centres of resistance to the Normans in Kent and it remained a centre of active Anglo-Saxondom down to 1089 while continuing to produce manuscripts in an Anglo-Saxon tradition well into the twelfth century...' (1990, 40).

It is perhaps even more surprising to find nothing on this subject in Gerrard's recent review (2003) of theoretical developments in medieval archaeology, and, again, a brief review of the journal *Medieval Archaeology* for the last 20 years seems to show little specific interest in this subject as an area of study. As many years of debate about 'Romanization' (or more accurately, these days, 'creolization') and material culture in the field of Romano-British studies would seem to suggest that there might be ways of re-thinking Anglo-Norman hegemony and structuration through its material culture (at the very least as a comparative case study?).

That the Normans and their successors (i.e. the ruling class of medieval England) were very soon calling themselves and each other 'English' rather than 'French' (and certainly not 'Norman' with all its Norwegian overtones), is attested by literary sources. Many of the Anglo-Saxon thegns had apparently left the country by about 1080 to enter imperial service at Byzantium. Higham writes that they

'...dedicated their own chapel...appropriately enough to SS Nicholas and Augustine of Canterbury. The establishment of these English thegns, who refused to be reconciled to William's kingship, in the Near East provides a fitting end to our discussion of the death of Anglo-Saxon England.' (1997).

But how did the majority of the 'English' population who remained define themselves in the years between the battles of Hastings and, say, Crécy (and indeed beyond)? To what extent was there a material expression of a new cultural identity? In fact, might we be more consciously tracing the development of a 'creole' material culture in the South-East at this time, with its linguistic equivalent in Middle English? Evidence of truly 'Anglo-Norman' cultural forms might of course be equally sought in Normandy. For instance, consider Impey's (*Medieval Archaeology* 1999) suggestion that seigniorial residences in Normandy of between *ca.* 1125 and 1225 were not only part of the same tradition as English examples of the same date, but that this model actually originated in an Anglo-Saxon tradition. Impey also considers the Norman impact of domestic housing in England in the same article.

Perhaps the less enfranchised members of society in the post-conquest South-East England retained more localised distinctions of group identity, and perhaps even some pre-conquest attitudes. This could be particularly interesting from a regional perspective (if some of us have to struggle somewhat to take a regional rather than a county or even more localised view today, what was the situation then?); yet events promoting a broader, even 'national' identity, such as government edicts relating to war with other governments (for the practice of archery, for example), Plagues, Poll Taxes, and Peasants Revolts must have had an impact on old group values and associations.

Understandably, such events were a watershed for the structure of medieval society, and Brandon and Short find interesting historical evidence of particular biographies of social mobility from the heart of the region as we move into the later medieval period, for which archaeological evidence, in the form of houses as well as other material culture, is also available. Such is the case of the 14<sup>th</sup> century Robert de Etchingham:

'the occupier of Glottenham [who] had higher standards of living than earlier occupiers...attested by the archaeological evidence of cinder-paving in the form of a court-yard in front of the buildings and outside the rear doorway of the hall. He buried rubbish tidily in pits and used high quality polychrome jugs from Saintonge. Yet the construction of the house with a minimum of masonry suggests that Robert was not a man of unlimited financial means...' (1990, 120).

Then there is the case study of one Bartholomew Bolne, who for Brandon and Short typifies the new middle class social climbers at this time, enriched by land enclosure following the epidemics and social upheaval of the 14<sup>th</sup> century, and socially advanced through education and public service (1990:120–121). Bolne was steward of Battle Abbey for 50 years in the 15<sup>th</sup> century; Brandon and Short's material illustration of him is a reproduction of Bartholomew Bolne Brass, from the church at West Firlie in Sussex (1990: Figure 3.5). It is perhaps of particular interest then that Bolne and his wife Eleanor seem to be depicted on this brass in the formal posture and clothing of a Knight (in armour) with his Lady: this seems an excellent example of combined public munificence and ideological symbolism invested in a local church. The earlier *Sir William* de Etchingham's rebuilding of the Etchingham parish church in the 1360s, complete with a series of carefully arranged stained glass windows representing royal and lordly coats of arms (1990: 119) is an even more lavish example of associating a family with the ongoing power of the lords, both temporal and spiritual.

Such examples of the detail of the history of social change in the higher Middle Ages actually seem to militate against the generalising model of exploitation of the interior of the region by the coastal inhabitants, as initially postulated by Brandon and Short, and point rather to agency on the part of what seem to be inhabitants of the interior. From an anthropological perspective, these buildings (like R.de Etchingham's manor house), decorative art (like W. de Etchingham's church windows and the Bolne Brass) and more portable material culture (like R. de Etchingham's polychrome jugs from Saintonge) potentially constitute a material yet nonetheless legible 'text' for archaeologists interested in understanding the structures beneath the surface of medieval society. Mathew Johnson, for example, has discussed the use of space in the medieval hall houses of the South-East as a context for the construction and maintenance of social ideologies, the building layouts directly relating to the perceived social standing of different members of the household based on class and

gender (within his acclaimed more general Introduction to Archaeological Theory: 1999). Again, Roberta Gilchrist, for example, has investigated such subjects in relation to medieval castles as well as monasteries and nunneries, through formal spatial analyses (again see Gerrard 2003: Chapter 6).

On the subject of castles, and ‘defence’ generally (a SERF Resource Assessment theme), it is worth bearing in mind that the earliest medieval castles can be viewed as more *offensive* (protecting and consolidating an invasion). Later defensive structures associated with private and ecclesiastical houses can also be seen as affording protection from within the region as much as without, either during times of civil war, or more generally guarding against the unwanted attentions of the less well off (whether acting in concert or in a more *ad hoc* manner). But all of these buildings also signify other things about their owners, too. The moated sites belonging to Robert de Etchingham and others surely promoted an image of social status as much as the internal layouts of the halls themselves. On a grander scale, Bodiam Castle in East Sussex has clearly been shown to lack efficacy as a defensive structure *per se*, and can be viewed more as a prototype of the post-medieval stately home, complete with formal gardens and a viewing platform from which to survey them. Johnson (1999, Chapter 10) sees Bodiam as a case study; the historical source (in this case a ‘licence to crenulate’ in military language) must be qualified by an understanding of the actual building and landscape context.

Moving on to artefactual evidence, the need for regional type series for various artefacts has already been noted. This is because, as Gerrard puts it:

‘...the choice of a region as a unit of study bridges the gulf between isolated microstudies at the local level and national surveys which may gloss over conflicting trends’ (2003, 187–188)

And the PPG16 (and PPG15) system in particular seems to be fundamentally characterised by microstudies and a lack of either regional scope or any collective mechanisms whereby that scope might be realised. However, it is important to emphasise once again that many would now perhaps hope to go beyond cataloguing and analysis of large-scale distribution maps of artefact types (in order to understand procurement and systems of trade and economy, for example). It is at least equally important to reconstruct the public and private use of these objects in the construction and maintenance of social and cultural identities (e.g. via structuralist and phenomenological approaches).

By way of example, we might return here to the apparent discrepancy between Robert de Etchingham’s use of expensive pottery (and presumably equally exotic and expensive contents, such as wine) and his apparently retaining a timber rather than a masonry house, as noted by Brandon and Short. They put this down to financial limitations, but might there be more to de Etchingham’s continued use of a more traditional building material than sheer lack of funds? And what were the symbolic aspects of the expensive crockery and its contents (and perhaps even of the way in which it was discarded)? We might certainly reflect that it was probably not de Etchingham himself who physically ‘buried rubbish tidily in pits’, but representatives of social classes not as often visible in historical sources as they might be in the archaeological record. What can we learn about their ways of life in comparison with the upper and (apparently increasing) ‘upper middle’ classes of the period?

The fact that the Middle Ages were a time of upheaval, perhaps especially for the working population of the region, is especially evidenced by archaeological sources. That the poorest strata were in an especially vulnerable position seems to be indicated most clearly by surface visible deserted medieval villages, but there is likely to be a great deal more information below the surface that will help to reconstruct the everyday lives of the relatively poor throughout the period, as well as their part in large-scale socio-economic change. For example, the site of a small farmhouse was recently excavated by the Canterbury Archaeological Trust at Bogshole Lane, near Herne Bay (Richard Helm, *pers. comm.*). The building had apparently been abandoned in the mid-14<sup>th</sup> century (as already noted, a critical time for demographic and social change, not least as a result of the Black Death); the complete ground plan of the building within a gravel courtyard and plenty of ceramic evidence, and, considering the little we know about the lifestyle of the poorer strata of society at this time, should be considered as being of regional significance.

As well as profound change in the rural sphere, the concurrent development of towns (where many unable to sustain a rural existence were probably driven to congregate) is of course a related subject. Moreover, the development of a mercantile class based in the urban centres (a starting point for social mobility?) is a socio-economic aspect of the period that should not be overlooked. The Martin's work at Winchelsea has elucidated some of the economic activities of such merchants in the form of buildings evidence. Once again, however, a research framework for the future should attempt to deconstruct other social and experiential aspects of urban centres, including the phenomenological, 'structural' and ideological, rather than merely treating conurbations as economically driven nodes of settlement and commerce.

As well as the 'lower' and 'middle' classes, the lives and experiences of minority and/or particularly marginalized groups like Jews or lepers are also an area for more emphasis in further study. Such subjects should not themselves be marginalised as the province of specialist interest, however. As Christopher Daniel has argued in his book on Death and Burial in Medieval England: '(C)omparisons between main-stream Christian societies and these liminal groups throw both into sharper contrast' (1997, 205). Both Jewish and leper communities are also more likely to be better represented in the archaeological than the historical record. The archaeological funerary record in particular is an important source of information for such groups (St James and St Mary Magdelene Leper Hospital in Chichester being of national significance), but is also an area of study in itself, in relation to the social implications of funerary rites.

It is interesting to note the apparent persistence of superstition in funerals of an overtly Christian era, for example, such as the placing of pebbles in the mouth of the deceased (e.g. at the cemetery of St Nicholas Shambles in London, see Schofield and Vince 1994, on Medieval Towns: Figure 6.5): how widespread were such practices and what were their social implications? Beyond this, there are whole schema of religious rituals and beliefs to consider, of course, including pilgrimage, for example. Portable Antiquities Scheme finds might in the future provide important new data in the latter area (and for route ways more generally) through 'stray' finds, but better understanding of the whole vista of religious practice in the public and private spheres, as well as the interaction of religious groups with wider society, is surely

vital in any attempt to reconstruct the experience of Medieval life, and especially in the South-East with an international focus on Becket's shrine.

Migrants into South-East England during this period, such as the glassmakers of the Weald, are further examples of less mainstream social groups that warrant more detailed study in comparison with 'mainstream' society, and this also brings us to the significance of new approaches to medieval industry in the region. A better understanding of crafts and industry is a key area for further research. However, this research should deal not merely with the development of industrial practices (and associated trade mechanisms) in themselves, but also with reconstructing the social context, and experiences of the people involved in such activities. As has been put forward for the study of medieval castles and churches, there is a need to place industrial centres in a wider landscape, social and experiential setting, and in particular to archaeologically investigate the hinterlands and support networks (especially in terms of labour) of industrial centres.

For a historical period like the medieval, clearly multidisciplinary approaches, using historical and cartographical sources as well as archaeological, are the best possible route for further research (preferably with historians and archaeologists working together). The period has also often been viewed as formative of certain aspects of modern life, but it is important to avoid perceptions and assumptions of straightforward (culturally determined) 'identification' with the medieval inhabitants of the South-East, to the detriment of realising those aspects that might be alien to us and particular to that culture. If we avoid over-privileging of historical sources and reliance on deterministic models, a more relativist anthropological approach to the evidence could add much value to our diachronic understanding of a complex medieval society in the region, especially with the new archaeological data that are increasingly available. It is vital, however, that collection and storage of these new data be carried out in response to the research questions we would wish to ask, and also that this dialectic continues to be reassessed in the future.

### **Possible research directions on monastic sites**

Judith Roebuck (JR)

JR had conducted an initial resource assessment of sheer numbers of early medieval and medieval monastic establishments in the region (not including hospitals and templars), and would advocate a multidisciplinary approach to monasticism: it is important to take a holistic view of both the evidence and what we hope to reconstruct of monastic life.

JR presented a total of 33 known monastic settlements in Kent:

- Benedictine 6
- Benedictine nuns 4
- Augustinian 6
- Augustinian nuns 1
- Cistercian 1
- Premonstratensian 4

- Priors/ Friaries: 9
- Alien priories 2

8 known monastic settlements in Surrey:

- Benedictine 1
- Augustinian 4
- Cistercian 1
- Carthusian 1
- Priors/ Friaries 1

And 27 known monastic settlements in Sussex:

- Benedictine 3
- Benedictine nuns 2
- Augustinian 7
- Cistercian 1
- Cluniac 1
- Premonstratensian 3
- Priors/ friaries 10

There is much variety in our understanding of these sites in terms of their layout, the relationships between them and their relationships with the lay community in terms of land holding and everyday life. The sites include upstanding remains as well as buried archaeology, as well as historic and cartographic evidence. There has tended to be a focus on the churches in the past excavations at the expense of learning about monastery complexes and their hinterlands. An exception for further study might be St. Augustine's in Canterbury as a good deal of the excavations there looked at domestic features beyond the monastery, even though this wasn't considered a research focus at the time

By the 9<sup>th</sup> century the monasteries were becoming quite influential centres of agriculture, learning etc, and it is important to note continuity from the Saxon to medieval period in this study. Sites in the region known to have both Saxon and medieval monastic remains include:

- Canterbury Christchurch
- Canterbury St Augustine's
- Chertsey
- Rochester
- Minster in Thanet
- Folkstone

There is much more scope for using the wide range of documents available to complement the archaeological evidence; this has only really been carried out to any extent at Canterbury, and not a lot else has been done in the region as a whole in terms of this type of synthesis (it has to be admitted that a relative lack of HER information has not helped in this regard).



JR suggested that archaeologists need to move away from the ways in which they have traditionally approached the study of monasteries. Rather than an emphasis on describing the site plan, there is much potential for formal spatial analyses in terms of hierarchies, public and private access etc, such as those carried out by Gilchrist, and already mentioned by JW. Also, one of the aspects archaeologists had not been good at dealing with are the spiritual values involved. We cannot ignore the religious aspect of these establishments. Moreover there are synergies between Roman sites, Saxon 'tribal land' and the location of monastic sites in the future to consider. The involvement of monastic enterprise in industry and trade, and study of burial populations (comparisons between monastic and other communities) are areas for prioritising further study, as is the post-dissolution use of sites.

Finally, it is important to reconsider how these sites and the information associated with them are presented to the public; there is a real need to key the visitor experience into aspects of medieval monastic culture that research can reconstruct through multidisciplinary approaches.

### **Excavations at Canterbury Whitefriars 1999-2003**

Mark Houlston (MH)

Giving a detailed account of the early medieval and medieval phases seen in the Canterbury Whitefriars excavations, MH highlighted some of the important things that this type of large-scale open area excavation can and can't tell us. The main benefit from detailed excavation and recording methods over such a large area is that much more can be understood in terms of context. However, concentration of features over such a long period can make earlier phases much more difficult to piece together. Of particular interest for the later medieval period was the detailed evidence the Whitefriars excavations had provided for the area around Gravel Walk in the early post-conquest period (including medieval Gravel Walk itself), and for the development of the friary from 1324 until the Dissolution.

It is worth noting that in the period 1050–1225 there would still have been significant ruins from the Roman period standing. The fact that Gravel Walk seems to have been respected is an important discovery. The excavations also revealed other road surfaces of lost lanes that had gone out of use when the friars took the land. An early building that survived right through to post-medieval period was discovered in this phase, and also a small building at other end of social structure (probably belonging to shop keepers, and very utilitarian). Pits tended to dominate, with over 1000 excavated, and primary fills often containing bone, coprolites etc, and therefore important evidence of diet. The latest period prior to the Friary (1225–1325/50) is characterised by some evidence of depopulation, a decline perhaps associated with plague. There is also evidence of waste over an early road surface. Nonetheless, medieval Gravel Walk may have been a market at this time. There was also evidence of one or more complete properties, and in one or two areas evidence of slightly higher status buildings, but buildings were mostly small scale and of timber.

The excavations could then trace the piecemeal development of the friary (land acquisition and building) from 1324–1537 in detail. Actually, pictorial evidence

shows that some substantial sections of the friary were still extant as late as World War Two. However, the combined efforts of the Luftwaffe and 1960s developers had destroyed all upstanding sections of the buildings. The Whitefriars excavations showed that much evidence remained beneath the surface, however, including:

- The east end of the chancel, built in two phases, using different building materials
- A tomb added on the north side of the chancel, and within the nave about a dozen burials, which will be compared with burials from the lay cemetery to the north of the nave in terms of disease, nutrition, etc. (and also compared with evidence from other sites, for example the 1300 burials from nearby St. Gregory's in the city)
- Sunken areas, occupation levels etc, a later warming room with a fireplace
- An early latrine and reredorter backed onto the dormitory, producing good quality environmental evidence predating the dissolution, and also containing 60–70 complete pots
- Later a new latrine at the end of the infirmary
- A later structure cut into the infirmary, which meant re-facing the flint foundation of previous building
- Further to the west, the kitchen: not a lot of waste, but building went through eight major rebuilds, culminating in a masonry structure with what appeared to be a chimney in each corner
- Final buildings that appeared to be storage rooms and a small passageway
- The boundary wall (which was interestingly one of the latest structures was there being no evidence of earlier wall)
- A large pit beyond the dormitory block (7m deep), was dated very late in medieval period may have been linked with gravel extraction, and was backfilled with waste from the friary, again producing excellent environmental evidence
- Further interesting structures to the north, near the altar end of the church. These had dwarf timber walls (from the 14<sup>th</sup> century). The building survived until the dissolution, with a large cess tank associated, the latter within 5m of the altar of the church
- Isolated kitchen buildings
- A new building at the end of the medieval period on a different alignment, which might be a reflection of there being not much competition for land
- Destruction deposits and layers, many of which were clearly enough *in situ* to warrant careful recording with an electronic distance meter (EDM), and which included painted masonry, clearly representing the Dissolution
- The other side of gravel walk, which was generally characterised by complicated evidence of timber buildings associated with groups of pits throughout the sequence.

The detailed findings from environmental analyses were not yet complete and could not therefore be included in this paper. Enid Allison's paper (following) would give

some idea of the depth of analysis afforded by such remains, however, via the case study of the Town Wall Street, Dover.

### **Townwall Street Dover**

Enid Allison (EA)

EA reported some of the most significant results of environmental analyses from the site at Townwall Street, Dover (excavated in 1996 in advance of a new filling station). The site covered a large area in part of Old Dover about which little was previously known from a historical or archaeological perspective. There was no waterlogging, but the site nonetheless produced a lot of good evidence. In the post-conquest period the site was situated on an ancient shingle spit/ridge, and was subject to intensive occupation by the middle of the 12<sup>th</sup> century. Excavations revealed the ephemeral remains of 41 buildings either side of a road (which later became Clarence Street) as well as a path running between buildings.

Much of the material was derived from sealed deposits resulting from many episodes of re-flooring the buildings with chalk floors, and over 200 laminated floor deposits and thin layers of occupation material were examined, dating to between 1175 and 1300. There was much (and some surprising) evidence of work and life in this seaside community, including fishing materials, pottery, textiles and the remains of two newborn babies. There were few pits for an urban excavation (about 12) indicating that much refuse was probably dumped in the sea, but there were also dumps of refuse outside the buildings (apparently quickly buried).

Pollen preservation was poor, but analysis of limited quantities of pollen and more abundant charred plant remains suggested that the occupiers used straw on the floors and that the roofs were thatched.

In addition, over 83,000 fish bones and dermal structures (the majority from plots F and G where preservation was better) gave a clear indication of the primary activity of the site. A number of fish species were represented, but the vast majority (about 80%) were herrings, as compared with fish of the Cod family (about 13%). There were no exclusively fresh water fish. The overall assemblage was similar to that from Fullers Hill, Great Yarmouth, and there is also documentary evidence to support such proportions of fish species in archaeological contexts. The species also indicated various fishing techniques being deployed for much of the year, from net based herring fishing in autumn in North Sea, to off shore fishing for cod and whiting using long lines with hooks. An absence of hake indicated that the fish were not being sourced from waters off South-West England. There were also some rarities, including species generally thought to be appearing in British waters due to global warming, which tallies with other evidence that the climate was warmer generally in the earlier medieval period.

Complete fish were present providing few clues to any specialised processing activity. An exception to this was the presence of many scales from herrings perhaps resulting from the production of pickled herrings (roll mops). There were also bird remains, and most interestingly butchery marks on the remains of sea birds from a consistent

range of *taxa*. Other animals represented included cattle and sheep (most butchered at quite an advanced age), and pigs (mostly aged about 2yrs or less), as well as some cats and dogs. Some porpoise vertebrae suggest the possibility of illicit consumption by locals, as this was considered a “royal fish”.

Eggs of worms from the urinary tracts of rats (including examples from some of the floors), and the residues and technology for malting and the brewing of strong beer probably flavoured with, among other things, gorse and heather (used as fuel in the malting process) added further to the reconstruction of life in this apparently poor community: a particular community which, however, had some very specialised and interesting traits, and that nonetheless raised research questions that could be pursued more widely.

### **Some Thoughts on the Medieval South-East**

Joe Flatman (JF)

JF moved discussion back to the more general level, first weighing up the relative current contribution of developer and research led (agreeing with JW that all archaeology is, and should be recognised as research archaeology). Nonetheless, because developer led archaeology has not been driven by archaeological research questions, the picture it gives of the medieval (and other) archaeology of the South-East has been skewed. There is perhaps an emphasis on urban and sub-urban sites, and definitely in certain parts of the region.

Again, it would seem that most Aggregates Levy Sustainability Fund money has gone into prehistoric sites because of the nature of the geology of the SE, and the pressures for aggregates for industry, driving extraction in the region which in turn results in discovery of sites of major importance: primarily prehistoric. In this, the fact that many medieval sites are already well-known almost ‘disadvantages’ them in comparison to other period sites. For example, in Surrey the site of Waverley Abbey is technically in a minerals ‘zone’ (identified as being an area having minerals suitable for extraction), but because it is so important and well protected it will obviously never come under actual threat from mineral extraction, and thus is (thankfully) not eligible for ALSF-type funding. Some of the only ALSF medieval work in the SE has therefore come in the marine-zone work (see JF’s contribution to the SERF Maritime seminar). From the strictly research led perspective there has been little that has focussed on the medieval South-East, for reasons that include:

- Lack of dedicated university archaeology departments in the South-East – e.g. in Surrey there is no dedicated archaeology department, only the RHUL Departments of Geography and Classics
- Lack of external research funding support for ‘local’ projects – even for universities based in/adjacent to the South-East (e.g. UCL, Southampton, King Alfred’s, Kent, etc.) it can be hard to get funding to do research – the South-East appears to be seen by funding councils both as too ‘parochial’ and also too well provided already by developer funding, etc. It is far easier for South-East- based academic archaeologists to get funding to work either abroad or in more

economically depressed regions of the UK. Those projects that have been successful getting large funding (e.g. UCL Anglo-Saxon Civil Defence Project) are [i] collaborative and cross-disciplinary (this project involves UCL Inst. of Archaeology, York Dept of History and Nottingham Centre for English Studies), [ii] regional, not site or county specific, and [iii] primarily desk-based

- Lack of joined-up work by universities, local government and local societies? There is no lack of stakeholders in South-East England, but few of these usefully co-operate on fieldwork, for a variety of reasons
- Relative lack of local government or other organised regional museums – e.g. in Surrey there is no central museum ‘service’ nor a formal ‘county museum’, similar in both East and West Sussex and Kent. This has always meant that resources, experience and archives are scattered. This is in turn a consequence of the nature of the South-East of England, especially its inland, outer-London suburbs, where a high percentage of London commuters and a low percentage of external visitors (especially holiday makers) means there is little impetus to extend local museum services
- A wealth of prehistoric materials in the South-East in comparison to other regions, and correspondent relative ‘poverty’ of medieval materials. Several high-profile prehistoric sites in the South-East (i.e. Dover BA boat; North Park Farm, Surrey; Boxgrove) have understandably drawn attention towards these subjects. Similarly, particular research foci of individuals and groups have long led to a focus away from the medieval. For example, in Surrey far more research attention has been paid to the Roman and post-medieval periods than to the medieval, except in the case of PPG16 type work in towns and suburbs, and ALSF-related work on prehistoric sites. Medieval remains get the ‘short straw’ of attention in this. One of the only exceptions to this is the ongoing work exploring the medieval landscape of Romney Marsh
- Relative density of woodland in the South-East, and lack of a general appreciation of this. The popular perception of South-East England, even among people living here, is that it is densely populated with extensive suburbs and virtually no real ‘countryside’ remains – the Weald is virtually invisible to most people, the downlands only slightly less so. This perception does not help foster a sense of the need for period studies in which locales like the Weald are essential.

The bulk of ‘new’ data is tied up in ‘grey’ literature, and a sustained campaign will be required to see this integrated into some sort of publication that really sums up recent developments in our understanding of the medieval South-East. It is important too to make the medieval more prominent in voluntary fieldwork agendas, and to foster communications between societies and other stakeholders (see above).

Much of the problem of missing data from certain areas is tied in with modern landscape use. Large sections are not as developed, so we are not seeing the way these areas were used in the past. The Weald is a particularly good example of an area that is still largely unknown in terms of its medieval past, but it is drained by nine ‘major’ rivers – is there a context here for a river based landscape characterisation survey for

the medieval period? Another important aspect to consider is coastal change, which was massive both during and since the medieval period. Reconstructing coastal landscapes and life should be a research priority, in tandem with understanding the related development of urban centres and (in turn) their relationships with their rural hinterlands. It might be suggested that transport-related settlement linked to ‘light industry’ is a core ‘theme’ in this overall landscape. In addition, perhaps we should be doing more to reconstruct such aspects of medieval life as:

- A ‘sense of place’ in this traveller landscape? Perhaps what ‘defines’ the medieval South-East (as with the modern South-East) is that it is a landscape where a lot of people pass through a lot of the time; the archaeological ‘footprint’ of such existence in a pre-industrial/proto-industrial world is likely to be smaller than that of the settled farmlands of the central England lowland belt
- Realities of life on a ‘cosmopolitan’ coast but a ‘suburban’ hinterland?

Fine-grained area analysis with environmental work will help to reconstruct these landscapes and experiences of the medieval South-East. It may simply be that the medieval period has a lighter archaeological ‘footprint’ in the region than expected, particularly in places like the Weald. It is not that people were not there, but rather that they were influencing the landscape in ways more subtle than in the more heavily farmed central England belt, for example. We should consider the extensive environmental archaeological and other fieldwork that has been necessary to even *begin* to glimpse the nature of medieval Romney Marsh, and then extrapolate similar work under the Weald forest canopy; one can begin to get a sense of what is needed here! Similarly, we should then fit landscapes such as Romney Marsh into an understanding – again, through fine-grained environmental and other analyses – of inter-linked urban spaces, especially along the coast.

In the case of Romney Marsh for example it is thus a question of how the marsh’s development/management relates to both the ecclesiastical authority of the local major monastic landowners and also the lay authority of the semi-autonomous Cinque ports. Are there also links here to earlier (at least Roman if not even earlier) management strategies? And how is the coast, especially such ‘wetlands’, being approached as a socially meaningful space (given the evidence for such use in the prehistoric period, for example the possible ‘ritual’ aspects of the Dover Bronze Age boat deposit, prehistoric ritual deposition of metal objects in watery places [as discussed by Bradley in his book *Passage of Arms*, 1998, 2<sup>nd</sup> edition]). Consider also the medieval preoccupation/association of freshwater marshlands with female monastic communities (as discussed by Gilchrist in her books *Gender and Material Culture: the Archaeology of Religious Women* [1994], and *Contemplation and Action: the Other Monasticism* [1995]). Again, an interesting comparison might be made to, say, the research of Paolo Squatriti (1998) in his book *Water and Society in Early medieval Italy, AD 400-1000* and more recently in the edited volume *Water Management, Communities and Environment: The Low Countries in Comparative Perspective c.1000 - c.1800* (2006).