Medieval Settlement: Some suggestions for the future
by Stephen Rippon

It is impossible to predict where any academic discipline will go in the future, and frameworks to guide future research must be the work of learned committees not single individuals. The MSRG has already published its Research Policy (MSRG Annual Report 11, 1996), and what follows, therefore, is not an attempt at a definitive research framework but rather some reflections on current practice and suggestions for future directions.

An interdisciplinary subject
During the past fifty years the study of medieval settlements, and increasingly their associated landscapes, has been notably multidisciplinary. This is reflected most obviously in the pioneering collaboration of Maurice Beresford and John Hurst, in the diverse membership of the MSRG, and has contributed to the success of past and current projects such as those at Wharram Percy, Shapwick and Whittlewood. There has already been a blurring of the distinction between medieval settlement studies and the wider world of landscape archaeology, and while this is to be welcomed there remains too much research where there is insufficient dialogue between archaeologists and historians: we must continue to push for greater interdisciplinarity with archaeologists and historians actually working alongside each other for the duration of their research, and discussing the problems, potential and interpretations of their data, as is so successfully occurring in the Whittlewood Project (the archaeologist and historian even sharing an office, fieldwalking and digging test pits together! (Jones and
despite the growing interest of medieval archaeologists. The modern study of vernacular architecture, for example, began shortly after the second World War, and despite the growing interest of medieval archaeologists in rural settlement at the same time (the Vernacular Architecture Group was established in 1952), dialogue between the two sets of scholars was limited (Pearson 1998, 166), despite the early collaboration of Fox and Raglan (1951–4) in Monmouthshire. Some archaeologists/landscape historians have recognised the potential of studying standing buildings, such as in the much debated ‘great rebuilding’ of c. 1570-1640 (e.g. Hoskins 1953, cf Machin 1977; Johnson 1993), but progress has been slow (e.g. Currie 1984; Machin 1994; Mercer 1994). This is becoming an ever more important issue as increasing numbers of dendrochronological dates are showing that the earliest surviving domestic houses are 13th century, exactly contemporary with many of our excavated examples (e.g. Pearson 1994). These buildings are a remarkable untapped resource for archaeologists as the evidence can be so much better preserved: in Devon, for example, a late medieval cruck-built dwelling at Northwood Farm, Christow, originally of one storey but later enlarged and improved, produced virtually no evidence below ground before the 18th century (Brown and Laithwaite 1993). To date, much of the research into vernacular architecture has been typological and thematic in character (Pearson 1998, 174): what is needed are more regional surveys of the whole building stock, integrated with archaeological and historical research that enable these buildings to be placed in their landscape and tenurial context (e.g. Shapwick, Somerset: Aston and Gerrard 1999).

**Palaeo-environmental evidence**

There is a particular need to collect more palaeo-environmental material: such is the nature and survival of documentary evidence that it is archaeology and in particular palaeo-environmental material which has the greatest potential for shedding light on a wide range of topics including pre-Conquest landscapes (and see below), and the lower strata of society in all periods. Whilst excavations on urban and high-status sites have produced an abundance of palaeo-economic evidence such as faunal assemblages, there is, at present, a dearth of such material from lower-status rural sites (Dark 2000). The absence of deep stratigraphy and past refuse disposal practices mean that this will always be a difficult issue to resolve without large-scale open area excavation, though it is essential to obtain such material, and synthesise the data we have (e.g. Albarella 1996), if we are to address the presence imbalance towards the monastic, seigniorial and urban economy evident in both palaeo-economic (e.g. Albarella and Davis 1996) and historical research (e.g. Campbell 2000; Campbell et al. 1993).

There is also a need to make greater progress in obtaining long palaeo-environmental sequences that reflect landscape use, and change, over time (though there will always be problems correlating radiocarbon dated palaeo-environmental events with the more refined historical record). The lack of such data for the medieval period is well known (Dark 2000) and one sometimes senses an air of resignation that suitable deposits simply do not exist, as lowland peat bogs have lost their medieval and later deposits through desiccation and peat cutting, or tell us relatively little about the medieval landscape, as the higher upland blanket bogs lay beyond the areas of medieval settlement and so at best only give a very broad regional picture (e.g. Exmoor: Frances and Slater 1990; Moor et al. 1984). There is a danger, however, of making sweeping generalisations about the date and extent of upper peats and in the Somerset Levels, for example, there are now four sequences and several palaeochannel complexes that extend into the medieval period (e.g. Aalbersberg 1999; Aalbersberg et al. forthcoming; Beckett and Hibbert 1979; Somerset County Council 1992; Brown et al. 2003). These show considerable continuity in landscape exploitation across the Roman–medieval transition and even an increase in arable cultivation on the adjacent drylands between the 5th and 6th centuries, perhaps to compensate for the inundation of formerly reclaimed land on the Levels; a further increase in the intensity of landscape exploitation, including increased alluviation, around the 10th century could be related to the reorganisation of dryland landscapes which saw the creation of nucleated villages and open fields in places such as Shapwick (Aston and Gerrard 1999; Rippon 1997).

Other suitable deposits do, however, exist from within the medieval settled landscape if one knows where to look for them. The landscape history that they tell can be significantly at variance to the ‘traditional’ picture derived from upland sequences. The Greater Exmoor region, for example, has revealed a series of small valley and spring mires containing well-preserved sequences covering the last two to three millennia (Fyfe et al. in press). In contrast to the high upland sequences, which showed woodland regeneration in the early post-Roman period, these lowland/upland fringe sequences produced a remarkably consistent picture of complete continuity in landscape exploitation between the Roman period and the 8th–10th centuries when there was a pronounced increase in arable cultivation probably associated with the introduction of a regionally distinctive pattern of agricultural management known as convertible husbandry. Such small mires are, however, not easy to locate and the key to the success of this project was, once again, interdisciplinarity: in this case an archaeologist working with geographers who knew how to locate these inconspicuous mires.

**Studying a multi-faceted landscape: historic landscape analysis**

The somewhat diverse concept of landscape archaeology has existed for some 30 years, and medieval settlement studies have made use of the ‘historic landscape’ – the present pattern of fields, woodland, roads and settlements etc. – throughout that period (e.g. Aston 1985; Muir 1981). The physical fabric of the present countryside has long been recognised as ‘the richest historical record that we possess’: ‘the surface of England is a palimpsest, a
The characterisation of a historic landscape should, therefore, encourage a strongly holistic and multi-faceted approach and must be taken beyond simply classifying field boundary patterns: landscape character is the product of the interplay between a series of themes or components such as human occupation (settlements, religious sites etc), landuse (agricultural field systems, common pastures, woodland etc), other resource exploitation (eg mineral extraction, fuel production), communication (eg roads, rivers, the coast), and less tangibly, though of considerable importance, patterns of landownership and tenure (estates etc). The study of roads, for example, cannot be divorced from the study of landuse as one of their primary functions was to allow the movement of livestock between settlements, different sources of pasture and ultimately the market (Fox 1996b). The study of industry can similarly not be separated from the production of fuel and the communication system used to transport raw material and finished products to the consumer (eg Fould 2001).

Analysis of the historic landscape (and note that we have moved significantly beyond what many see as 'historic landscape characterisation'), is not just a means for looking at the whole landscape on a large scale. It is also an excellent means through which a wide range of sources and techniques for studying past settlements and landscapes can be integrated. One example must suffice: there has been an enormous amount written about social and agrarian history based upon the extensive archives of the estates of places such as Glastonbury Abbey and Winchester Cathedral but what did these estates actually look like, and how did they change over time? The fabric of the historic landscape, along with relic features now only preserved as earthworks and cropmarks, provide the ideal framework through which the wealth of topographical detail contained within these archives, such as place-, road-, field- and furlong-names, can be located through a comparison with later sources such as the Tithe Surveys (eg Aston and Gerrard 1999). In this way medieval landscapes can be reconstructed and brought to life at different points in time, and the reasons why the landscape changed explored. This highly interdisciplinary study of the historic landscape is far broader than the 'historic landscape characterisation' currently being undertaken to inform the planning process and countryside managers, and it may be that a broader term such as 'historic landscape analysis' is more appropriate (Rippon in press 2). The emergence of specialised settlements is another area that requires greater research. The MSRG has already highlighted the significance of seasonal settlement (Fox 1996a), and Fox (2001), for example, has shown the relatively recent origin for the fishing villages that form such a distinctive part of the landscape in South West England (and see Gardner 1996; MSRG Annual Report 2001, 5-14). Dyer (1995) has demonstrated the distinctive archaeological evidence for sheepeateres, occasionally occupied by shepherds in the Cotswolds, while Winchester (2000) has shown the importance of specialised demesne cattle farms ('vaccaries') in the uplands of northern England. There are in fact a wide range of other specialised settlements located in what have traditionally been regarded as 'marginal' areas that were actually rich in non-arable based resources, ranging from upland transhumance huts, shielings, and hafod (eg Hooke 1997, 85-90; Johnson and Roe 1994, 80-3; RCARMS 1997, 72-3; Ward 1997; Winchester 2000, 79-81) to marshland dairy 'wicks' (eg Rippon 2000, 2037). Care must be taken, however, to distinguish different phases of activity within these landscapes which

HLC is an approach through which every parcel of landscape is attributed to a particular 'type', each with a set of defined attributes and historical processes that led to their creation (eg Herrington 1998; Rippon 1996; www.hants.gov.uk/landscape.index; www.lancashire.gov.uk/environment/archaeology). Cadw, English Heritage, and Historic Scotland are all currently supporting a series of historic landscape characterisations (or 'historic landuse assessment' in Scotland), which are designed to be part of the planning system and countryside management: as such they are forward-looking and predominantly concerned with the character of today's landscape (Fairclough and Rippon 2002). There has been, however, relatively little dialogue between these HLC practitioners and the wider academic community, which is unfortunate as the technique has enormous research potential for examining on a large scale, firstly, how present landscape character came about, and secondly what past landscapes looked like. In Scotland, for example, HLC is demonstrating the somewhat unexpected extent to which landscapes were reshaped in the later 18th and 19th centuries (Dixon and Hingley 2002). In other areas, and given good documentary material, even medieval landscapes can be reconstructed. In a number of wetland areas, for example, the integration of historic landscape analysis with a comparison of 190 century and medieval field-names has shown that discrete areas with a distinctive morphology can be related to different periods, processes of landscape creation (i.e. types of reclamation), and types of landuse: high medieval fields created for arable or common meadow have a very different appearance to late medieval fields created for sheep pasture (eg Rippon 2002a, b).

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even in upland areas could include separate phases of seasonal grazing and cultivation (e.g. Silvester 2000). It is also important that these specialised settlements, and the communities that they supported, should not be divorced from the wider agrarian landscape. Woodland, for example, which in the past would have been intensively coppiced for fuel, could have also been managed in part as wood-pasture making a link with the wider agrarian economy. Woodland management is a much neglected subject, and it is not clear whether it gave rise to specialist settlements: documentary evidence from the post-medieval period suggests that charcoal burning was a seasonal occupation (Armstrong 1978; Kelley 1996), but did those engaged in such activities live ‘on-site’ or in nearby agricultural settlements?

Overall, therefore, the future study of medieval settlements and their associated landscapes needs to build upon its existing success brought about through interdisciplinary research and dialogue. There is a need to go beyond the traditional collaboration between archaeologists, historians and historical geographers to embrace more successfully the study of palaeoenvironmental material, place-names, vernacular architecture and the emerging practice of historic landscape characterisation. This breadth of source material is on the one hand a great strength, but on the other presents a problem: as the volume of literature, and number of conferences increases it will become ever more difficult for scholars to keep abreast of developments in these other fields of study. Greater interdisciplinary dialogue (though seminars, workshops etc), and scholars actively engaging in each other’s work is essential.

The diversity of settlement patterns
The need to study specialised settlements has been discussed above, but at a more regional scale there remains a need to widen the scope of settlement studies beyond the ‘village zone’. The extent of regional variation in settlement patterns, and their associated field systems, has been recognised since at least the 16th century when topographical writers drew the distinction between ‘Champion’ landscapes of nucleated villages and open fields in the Midlands, and the ‘Woodland’ landscapes of dispersed settlement and enclosed fields to the south east and west (Rackham 1986; Roberts and Wrathmell 2003). Although the village-zone actually covered less than half the country, there remains a marked bias within current medieval settlement studies towards this central ‘village zone’. The most extensive investigations of medieval settlement, at Wharram Perrey, Thrustington, Raunds, Milton Keynes, and Shapwick, all relate to landscapes dominated by villages and there remains a desperate need for equivalent projects in areas characterised by more dispersed settlement (and see Klapste 1999).

The Whittlewood Project, alongside Board’s (2001) work in the nearby Rockingham Forest, is making a start; in examining areas with some relatively dispersed settlement within the Midlands zone (and see Taylor 1995), but there remains a lack of comparable projects in the south east, west and north of Britain. The neglect of more dispersed settlement patterns can be seen most clearly in Gerrard’s (2003, 100-1) analysis of the investigations of medieval sites recorded in Medieval Archaeology that shows that farmsteads have been consistently under-represented. Understanding these landscapes characterised by more dispersed settlement patterns is, however, a complex matter. The work of Fox (1989), for example, has shown that what are now highly dispersed settlement patterns in western Devon are in fact the product of a late/post-medieval contraction of what had been more substantial hamlets. The origins of landscape characterised by these 12th and 13th century hamlets is, however, unclear; it certainly represents a clear break from the prehistoric and Romano-British tradition of isolated enclosed settlements (rounds, hillslope enclosures etc), but it is not clear whether these were replaced by a medieval pattern of isolated farmsteads, that later grew into more substantial hamlets through population increase, or whether the medieval pattern that replaced the landscape of enclosures was always one of hamlets and that its change to farmsteads is a relatively recent aberration. Even within the landscapes characterised by more dispersed settlements there is considerable, yet ill-understood, regional variation something that historic landscape characterisation and Roberts and Wrathmell’s (2000; and see Roberts and Wrathmell 2003) Atlas of Rural Settlement in England, will help in mapping. In East Anglia, for example, a series of fieldwalking surveys have shown how medieval settlement initially had a ‘Middle Saxon’ focus around the parish church and then shifted to nearby commons and greens; these communities were associated with a system of open field agriculture that was distinct from that in the Midlands. In the South West, a far more dispersed settlement pattern was associated with a regionally distinct pattern of field systems used for the ‘convertible husbandry’ system of rotation-based cultivation. While considerable attention has been focused on the extensive landscape reorganisation that led to the creation of nucleated villages and open fields in the Midlands (e.g. Lewis et al. 1996; Williamson 2003) there is now a need to examine the origins and development of these other regionally distinctive landscapes that were also emerging at the end of the 1st millennium AD.

The scale of landscape and settlement research: settlements, estates, tenants and their landholdings
The traditional units of landscape and settlement research have been the county and the parish. Such units are convenient in that they have clear boundaries and correspond to the organisation of both archaeological (e.g. Site and Monument records) and historical sources and data-sets (e.g. Records Offices, Victoria County Histories, English Place-Name Society volumes, County Records Society series etc). But are these really the most meaningful units within which to study past landscapes and settlement patterns?

The smallest unit of medieval settlement was the tenement. Documentary sources make it clear that there were richer and poorer tenants within any rural community and the study of settlements and their adjacent landscapes needs to include the spatial disposition of peasant landholdings, and indeed the lord’s demesne (e.g. Hall 1988, fig. 5.7; 1995, figs 2, 8, 9).
Without good documentary sources this will be difficult, but some impression of at least the late medieval/early post-medieval pattern (after the reorganisation and engrossment of landholdings following the 10th century population decline, eg Gardiner 1998) might be gained from studying the patterns of landownership in the Tithe Surveys. Several studies in the South West, for example, reveal discrete parts of parishes with a highly scattered pattern of landholding which along with the historic landscape character – blocks of long narrow fields – suggest the former existence of open fields; this is in sharp contrast to other parts of the same parishes where discrete blocks of more rectilinear fields were all held in severality (Aston 1988, fig. 5.6; Gillard 2002, 135-75; Pattison 1999, fig. 26; Rippon in press 2). The disposition of specific parcels of land may have changed, but the basic distinction between zones of scattered landholdings and zones held in severality is likely to have been stable.

Where earlier evidence exists these tenements can be sometimes be tracked back to at least the 15th century (Rippon, in press 1), while in Cornwall, recent work to refine the original historic landscape characterisation (Herring 1998) is revealing that these small irregular open fields existed on a far larger scale than was previously assumed.

There is certainly a logic to using townships/tithings, parishes and manors to study medieval settlement as they were the next tier in the hierarchy of territorial units within which the landscape was actually exploited. For detailed research, particularly of well-documented areas or where the opportunities for fieldwork are considerable, parish-scale areas may be as much as the resources available to many projects can cope with! Whilst some parishes can be regarded as characteristic of those in a locality, no parish however can really be regarded as typical, and as such a larger study area may contain a more representative sample of settlement types within a region. This certainly appears to be the case in Whittlewood, where eleven ancient parishes were selected based upon the quality of the archaeological/documentary material, and the sample they provided of settlement types in this region. It is interesting to note that the county boundary was ignored as it appears to have had no impact upon landscape character, which is, in fact not an uncommon phenomenon: although counties often been used as the unit for landscape study at the scale of synthesis and overview (eg Costen 1992; Williamson 1993), most shires were created simply as units of local government which both ignored existing landscape character and had little impact upon subsequent landscape change.

An alternative approach to identifying the study areas for medieval settlement research is to focus on the distinctive regions, sometimes called parishes, that we know actually existed in the past. Some were defined predominantly by their physical characteristics (e.g. downland, fenland etc Thirsk 2000), while others were more culturally constructed (e.g. the Black Country). There certainly are parts of Britain where communities did wholly occupy just a single physical zone, such as the extensive series of parishes in the costal siltlands of Fenland, and these areas certainly provide very coherent units for the study of medieval settlement (e.g. Hall 1996; Silvester 1988).

There are many cases, however, of human communities occupying territories that straddled several different physical topographies, such as the strip parishes of the Lincolnshire Wolds and Wessex Downland and their adjacent lowlands (Everson et al 1991; Hooke 1988b). In such circumstances it is not the chalk downs or clay vales that should be the basis of research, but the territories within which human communities exploited such a range of environments – which are often marked by watersheds.

A final scale at which medieval settlement and landscape can be profitably researched is that of the estate. Following the work of Jones (1979), 'multiple estates' in Wales have seen much study (eg Hooke 1997), and while it may not be appropriate to apply this model of territory organisation outside that area, similar large early medieval estates do appear to have existed elsewhere and these can provide the ideal study area for medieval settlement/landscape research (eg the eight later parishes within the early medieval territory of the Rodings, in Essex: Bassett 1997). For the post-Conquest period, the newly-created lordships and honors would similarly provide interesting units for detailed study (e.g. Honor of Dudley in the west Midlands: Hunn 1997).

Finally on the question of the scale of research, it is essential that British scholars continue to engage with colleagues throughout the British Isles (a great strength of the MSR and other organisations such as the Society for Landscape Studies) but also in mainland Europe. Conferences such as Ruralia are an essential means for scholars discussing what were often pan-European processes such as settlement nucleation and the all too often neglected dispersed settlement patterns (e.g. Klapste 1999).

**Portable antiquities, PG16 and the ‘grey literature’**

Recent decades have seen the implementation of several pieces of government policy that will have a profound impact upon all archaeology, but particularly ubiquitous and artefact-rich medieval settlements. The increasing popularity of metal detecting over the past twenty years has led to many archaeological sites being damaged, but the responsible reporting of finds by many practitioners has also led to an upsurge in information particularly for the earlier medieval period. In some counties, such as Norfolk and Suffolk, good relations have been established between museums and detectorists for several decades, and this has led to a dramatic increase in the number of known early medieval sites (e.g. Newman 1992), and socio-economic issues such as the extent of coinage circulating in rural areas (Dyer 1997). The Portable Antiquities Scheme is now extending this good practice across the country. In Lincolnshire, for example, a previously unknown class of settlement, the so-called Middle Saxon ‘productive sites’, have been revealed largely through detector finds (Ulmschneider 2000). The increasing collaboration between archaeologists and detectorists has the potential to shed important new light on medieval settlement as a whole, and in particular in periods/regions with a poor ceramic sequence (e.g. Cheshire: Philpott 1999).

Another government initiative, Planning Policy Guidance Note 16 (PPG16) is also leading to a dramatic increase in
archaeological information and has the potential to achieve even more: it is essential that the study of historic period settlement becomes firmly embedded into English Heritage's regional research frameworks. While PPG 16 may be criticised for preventing proper excavation due to the assumption of 'preservation in situ', there is now the potential for a huge amount of small-scale work to be carried out in and around medieval settlements that previously would not have happened. Organisations such as the MRSG need to be pro-active in educating those involved in the development-control process that fieldwork within existing settlements does have enormous research potential: the individual pieces of work may not amount to much, but cumulatively they have a considerable potential for advancing our understanding of the origins and development of individual medieval settlement and settlement patterns as a whole. There is a need to ensure that larger projects see proper publication, while for smaller-scale work it is essential that summary notes are published in county journals, and that the recent initiative to place Sites and Monuments Records online continues (e.g. Essex and Somerset).

**Defining the period of study: the problem of the early medieval period**

All archaeologists have to define the chronological and spatial limits of their research, but is the 'medieval period' really an appropriate one for the study of settlement? There is in fact a marked division between those scholars of early medieval ('Anglo-Saxon' or 'Viking Age') settlement archaeology, and the post-Conquest period, yet in order to understand the latter we have to understand the former. Indeed, there has been much debate over the degree of continuity between Roman Britain and medieval England, but how many medievalists have the time to keep abreast of scholarship on the later Roman Britain? At the other end of the chronological scale how significant is the medieval-post-medieval divide? The desertion/shrinkage of settlements, and enclosure by agreement of open fields, was a long process which took different forms in different regions, lasting from the later 14th through to the 17th century: might one argue that it was the agricultural and industrial revolutions that mark a more meaningful break in landscape history in many areas than c. 1500?

The major period for which we still lack any clear understanding of settlement is the early medieval period and the patterns that preceded the creation of the historic landscape of today. A number of major large-scale excavations have revealed 'early' and 'middle' Saxon settlements, though we have little understanding of the wider settlement patterns that they formed part of or the field systems that they were associated with. In parts of East Anglia and the East Midlands the continuous ceramic sequence has allowed fieldwalking to reveal a remarkably dense pattern of settlement, but the lack of excavation and palaeo-environmental analysis means these settlements, and their associated field systems and economy, are ill-understood. Similarly, where excavations are carried out on a sufficiently large scale on Romano-British settlements, evidence for post-Roman or 'Anglo-Saxon' occupation is sometimes revealed, yet fieldwalking elsewhere often shows a discontinuity between settlements of these periods: is there any regional patterning evident, and when exactly were the Romano-British settlements abandoned? The lack of a good ceramic sequence means that scientific dating must increasingly be used to date the stratigraphically latest deposits on 'Romano-British' sites, and the earliest phases of medieval settlements.

**A crisis of confidence?: the need for large-scale excavation**

The discussion so far has focussed upon research at a landscape scale, which has become increasingly popular since the 1970s. There is, however, a growing crisis not just in medieval settlement studies or indeed medieval archaeology, but in archaeology as a whole; we are apparently losing the confidence to embark upon large-scale, open area research excavations. There are many reasons for this, including the inevitable long delays in publication, the absence of suitable funding sources for long-term research, and the current short-termism created by the Research Assessment Exercise (RAE) which for younger scholars in particular makes planning a ten-year programme of excavation simply impossible (as increasingly do the rising costs of excavation in Britain due to accommodation, catering, machinimg, health and safety, and employment regulations). This issue must be addressed at a high level as some research issues can only be addressed through large-scale, long-term, open area excavation, preceded by appropriate large-scale survey work including not just the traditional techniques of aerial photography, fieldwalking and geophysics, but the emerging techniques of soil chemistry and GPS/GIS (e.g. Aston and Herrard 1999; Chapman and Fenwick 2002).

There are many issues that we have made little progress on addressing at a site level, notably with regard to the social structure of settlements. As described above, documentary sources suggest that within a village community there were richer and poorer peasants but how does this manifest itself in their buildings and artefacts? What was the function of different rooms within peasant houses, and areas within the toft and thecroft: where were crops processed and stored? Where were the landless labourers and servitors accommodated? In order to answer these questions we need to add greater social depth to our settlements through linking documentary evidence to the physical remains from excavations, the detailed recording of artefacts, and the application of the techniques of spatial analysis that are increasingly being developed in the study of standing buildings (e.g. Austin and Thomas 1990; Gardiner 2000 - and see Gilchrist 1995; Grenville 1997; Johnson 1996). To achieve this excavations must be on a large-scale: whole tofts not just peasant houses, and ideally whole settlements, need to be excavated - as colleagues in the prehistoric and Roman period have been achieving for decades.

Such research must also be carried out over the long term so that ideas can be discussed and allowed to mature. The history of both the Wharram Percy and the Shapwick projects shows how ideas, and therefore strategy, must be allowed to evolve over time. Whilst the 'Project Design' culture we now have should improve the management of excavations and particular
post-exavation and publication programmes, they are in danger of confining scholars in an intellectual straightjacket. There is one final reason why there needs to be a return to long-term research projects: excavation, and the way that it brings scholars together, should be seen as part of the research (and training) process rather than simply as a technique of collecting data. The roll call of those who served at Wharram Percy, and the careers across many disciplines that it helped promote, is testimony to itself to the value of long-term research projects.

Conclusions

This has been a generally positive view of the future of medieval settlement studies while identifying areas of concern and potential development. It has stressed that we must continue to build upon the interdisciplinary strengths of past and existing work, notably the collaboration of archaeologists, historians and historical geographers. There is a need to increase this interdisciplinarity, through constructing links with those who study place-names and vernacular architecture, and a more positive attitude needs to be taken towards the potential for gaining palaeoenvironmental sequences. Other boundaries that need to be broken down exist in both time and topic – is it not 'historic period settlements and their landscapes that we should be (or already are?) actually studying? Such research can be carried out at a wide variety of scales that will obviously depend upon the nature of the questions being asked, the source material that survives, and the resources available to study it. The parish and the county will remain convenient units although thought should be given to whether future research projects should be structured around regions that had a greater significance in the past, in terms of the human communities who exploited the landscape. In terms of space we must also remember the need to look beyond the narrow confines of a parish, region or our sometimes rather inward-looking island. Many of these are challenges faced by other areas of medieval archaeology, notably the study of towns and monasteries, and we must strive to adopt an increasingly integrated approach. The full potential of analysing the historic landscape is yet to be realised, and ‘historic landscape analysis’ has the potential for integrating a wide range of different data, as well as encouraging us to look at the whole landscape. There is still a bias towards the study of nucleated villages and regular open fields, and there is a need for much more work on landscapes characterised by more dispersed settlement patterns and more varied systems of managing landuse. We must consider the whole range of resources that were being exploited, including woodland and minerals, and the specialised sites that this resulted in.

Finally, the current planning environment within which archaeology is working has enormous potential for providing new information from deserted, shrunken and existing settlements, though there is a desperate need for a return to long-term, interdisciplinary research projects within which open area excavation plays a significant part. The current Research Assessment Exercise culture within which universities have to work, and attitudes of the funding bodies, is in danger of discouraging scholars from embarking upon long-term research, including excavation, despite the obvious benefits that they have had for our disciplines. We must all hope that this crisis of confidence is resolved.

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