A Research Framework for the Archaeology of Wales
Southwest Wales – Medieval
22/12/2003

The format of the review

To give as broad a context as possible for the discussion that follows I would like first to look at the disciplinary or discourse basis of work in our region, because clearly archaeology cannot and does not stand alone in our study of the medieval past. Most of the best work has always engaged scholars from other disciplines working primarily with other data than the material past. What we are not always careful to identify, however, is the extent to which our research agendas are constructed and motivated by those other disciplines and what effect this has on the nature and value of our contribution to debate. It is clear that the construction of a research agenda for medieval archaeology can only be realistic if it is debated with colleagues from other disciplines where agendas are shifting, in some cases quite dramatically. Any statement here will be contingent and provisional while it is being debated only by archaeologists.

Following this I would like to consider the effect of how the structures and missions of our institutions also determine the content and purposes of research. This is not to criticise those institutions, because in Wales I still believe that we have a structure which works and individuals within them who are dedicated to the understanding of our complex past. Rather it is to help the process of considering why we are where we are and what we might need to adjust or add to make our research objectives more achievable. This will help, for example, to explain perhaps why we have excavated so many elite structures and so few of the vernacular, as well as understand why so much has been excavated in the southern, richer and most Normanised, areas of the region and so little in the northern Welsh uplands. Anyone looking at the maps produced by the Resource Audit will spot that straightaway.

Finally I would like to summarise some of the things that could be achieved in the framework of current and future research. My main message here is one which I can remember writing into my first attempt at an exercise like this in 1974 when trying to identify the research priorities for rural settlement in England and Wales for the DMVRG as it then was. That message is an argument for more integrated contextualised research, which is truly inter-disciplinary in nature. This necessarily implies sustained long-term activities less restricted by institutional limitations: in other words more Llawhadens. To achieve this we will need to be more constructive in finding ways to work across institutional as well as disciplinary boundaries. To give us heart, it was thoughts like this which in England **did** lead in the end to some major programmes of research integrated with rescue, such as Raunds, Roadford, Wharram Percy and others which have contributed extensively to the great debate on village origins, manorialisation and feudalism.

The disciplines

Four main disciplines, alongside archaeology, have contributed ideas and data to our understanding of the Middle Ages. They are not discreet in the sense that scholars from all five areas have consistently borrowed not just ideas and data, but also methodology. One has only to think of the influence of morphology on our settlement studies, such as the work of Ken Murphy on small boroughs (Murphy 1997) or Jon Kissock's on the villages and field systems of Pembrokeshire (Kissock 1990 & 1997), to realise that we are constantly on the look-out for ways of better analysis. Hence it is essential, in framing our research priorities, not to be driven by our data alone, but also by the concepts and achievements of our close colleagues.

History

Much important Welsh national, regional and local history has been written and published in recent years. This includes key debates on the nature of pre-Norman Welsh society and institutions, on the pattern of conquest and political and cultural change between the eleventh and thirteenth centuries and on the processes of transformation in the later Middle Ages leading to modernity. However, an overarching synthesis for our region taking these debates into account is still to be delivered and it is sad that the notion of Dyfed has been abandoned as a structure of regional government in Wales. It alone had the resource and the will to promote its historical identities, a role which the Dyfed Archaeological Trust is now, almost alone and with almost no resource, left to undertake at a time when, paradoxically, the regions of Wales are beginning to re-emerge as a framework for our newly devolved government (e.g. economic regions of the NAW and tourist regions of the WTB). This research review is ironically, in itself, likely to be the most significant contribution in this area for many years.

Despite this, there is enough substance in the work of contemporary historians still to articulate the bigger historical questions in our own region, as well as on specific sites and localities. Nonetheless as a medieval archaeologist wanting to work on landscapes I still feel the need of a good economic and social history for Wales and our region in the Middle Ages, although we must recognise that the right kind of documents are scanty and exist in the necessary abundance for only a few places.

We do have the continuing projects of county history, most recently the two, soon to be three, volumes for Cardiganshire, although unfortunately for this exercise it is the volume on the Middle Ages which is still to come (Davies & Kirby 1994; Jenkins & Jones 1998). In Pembrokeshire the volume on the early modern period provides, in its rehearsal of antecedents, a very useful insight into the society and economy of the later middle ages (Howells 1987), but it is to be hoped that a volume (or volumes) to cover this complex subject in its own right might be on its way. The magisterial volumes by Sir John Lloyd on Carmarthenshire (Lloyd 1935) have been supplemented more recently by the studies edited by Heather James, although it was never intended to be a replacement county history (James 1991). Nevertheless its list of contributions does provide an important insight into the kind of multi-disciplinary agenda so essential to an understanding of both social and physical landscapes in Wales. None of these works, however, is organised topographically as they are in the VCH and we have to rely on the chance and scattered distribution of individual pieces of work to give us the primary research on specific places which in England reveals the patterns of territory, ownership and land use so important to medieval and later archaeologists as a foundation for contextualising sites and landscapes. This can be identified as a cultural reluctance from the later nineteenth century onwards to the production of local and topographical history: there is no Department of Welsh local history in a Welsh university. Since I have come to work in Wales, I have had had to undertake much more primary documentary research than has been the case in England or even other parts of western Europe I have also studied. It remains an important debate, therefore, the extent to which the fabric of Welsh history, the organisation of its data, readily enables cross-disciplinary research so essential to the conduct of historic archaeologies and to the creation of narratives about the Welsh landscape and culture. As I shall say later this is now becoming an even greater imperative as local 'community' is rapidly becoming a central plank of NAW, UK national and European policy as witnessed by such important projects as the SPARK villages brochures created by DAT or indeed Lampeter's recent study of Newcastle Emlyn castle for the town's development group. All of this comment on local history is not to deny, however, the scholarly support of individual Welsh medieval historians, such as Ralph Griffiths and Beverley Smith, in their willingness to work with archaeologists and use archaeological material both on specific localities (e.g. Griffiths 1991 on Dinefwr and Llandeilo) and on broader regional and national histories (e.g. Smith 1998 on Llywelyn ap Gruffudd).

Architectural History

The southwest region has a rich heritage of standing buildings and the most obvious observation, of course, is that, as yet, we do not have the Pevsner for Dyfed, although this will soon be remedied and will form an important gazetteer and some overview. For vernacular and minor polite architecture, however, Wales is privileged to have Peter Smith's masterly exposition based on individual studies and on stylistic and technological analysis (Smith 1988). The regional mapping here, especially those for the medieval and sub-medieval forms, is a powerful tool for our archaeological comprehension of architecture and its role in the cultural development of medieval Dyfed. For those of us working in Cardiganshire, his extended essay in volume 3 of the county history is also an important supplement, giving many more individual studies (Smith 1998). The high standards of scholarship and presentation found in these texts has continued in the work of others and we look forward to further synthetic contributions in due course. As with history, there is a need for much more engagement between archaeologists and architectural historians, not least because so little is known of the roots of vernacular style in the earlier middle ages. There is also a need to provide specific contemporary landscape contexts for the buildings that have already been so elegantly revealed to us.

For polite architectures we have the rich heritage of castles, palaces and tower-houses (in south Pembrokeshire), although we have very few structures of the lesser nobility and landowners. In particular we must be almost completely silent on the matter of undefended residences of Welsh princes and lords, a lacuna only archaeology can help with, as it has done in Gwynedd. There are certain possibilities as with Nicky Evans' recent findings at Llanerchaeron in Ceredigion. By contrast, many of the castles have been well studied by architectural historians and for a few this has been supplemented with detailed archaeological investigation, whether fabric survey or excavation as at Laugharne (Avent 1977, 1978, 1979, 1988, 1992, 1993), Carew (Gerrard 1990; Campbell 1990; Austin 1993, 1994, 1995), Dryslwyn (Webster 1981, 1982, 1983; Capel 1990; Capel & Jessop 1996), Dinefwr (Capel unpublished), Newcastle Emlyn (Parry 1987), Pembroke (Ludlow 1991), Great Castle Head, Dale (Crane 1999), Llansteffan (Avent 1991, Guilbert 1974; Gilbert & Schweiso 1972), Wiston (Murphy 1995), Cardigan (Murphy and O'Mahoney 1995), Newport (Browne & Percival 1992). This work, being conducted in the context of a new generation of castle scholarship, is already beginning to overturn some of the older, particularly the gradual evolutionary, assumptions about castle development to be found in some of the earlier architectural studies which still form the bed-rock of our study of these complex structures in the region (e.g. King and Perks 1962). An example of this is the developmental relationship between earthwork and stone castles, and we have to admit that this part of Wales has made little direct archaeological contribution to the discussion of how timber castles were created and abandoned in the processes of domination and resistance to be found in the twelfth century. An issue critical to this debate is the extent to which the motte and bailey castle can be seen culturally and socially as being in the same genre as the many ringworks to be found in the region. Here the basic matter of disentangling the medieval from the prehistoric and early medieval has not really been dealt with, despite relevant work at Great Castle Head (Crane 1999); Castell Henllys (Mytum 1999) and Llawhaden (Williams and Mytum 1998) and morphological interrogation by Dark and others (Dark 1994). This aspect is becoming a key issue as we begin to suggest in Britain and western Europe as a whole that the mid to later twelfth century rather than the tenth or eleventh is the period of greatest social change in the spaces to be found in architectures and countryside as well as in material culture (e.g. Gardiner 2000, Austin forthcoming). To what extent the motte and bailey and the ringwork is the end of the earthwork defensive tradition in Northern Europe rather than the beginning of the castle can be keenly debated and researched in our region which is so rich in surviving examples.

Ecclesiastical architecture, of course, shows also the force of such arguments for the twelfth century when effectively the narratives drawn from our extant churches can only begin at that time because of the total replacement of all predecessors. Indeed in terms of origins and antecedents, especially the transition from the pre-Norman llan and monastery, there is a huge gap in our knowledge, one which we may not be able to resolve until some way is found of funding full-scale excavation on at least one of our redundant parish churches, especially one which has documentary support for the pre-existence of a monastic component and preferably in circumstances in which other religious sites in the area can also be studied to give context. Related to this there are, of course, serious issues of conservation which are general to the United Kingdom as a whole and research is urgently needed into the reasons why we should conserve, what we should conserve and how these can be related to economic and social needs, particularly those of our rural communities. Indeed it is possible to argue in general that we do need in Wales some focussed and strategic research into the heritage issues of material culture, but that is another debate for another time!

To identify progress rather than lacunae, I would certainly highlight the value of modern archaeological techniques being applied to our standing buildings, especially those of photogrammetry and analytical fabric analysis. A remarkable example of this is the splendid study by Rick Turner and others on the Bishop’s Place at St David’s (Turner 2000). My only complaint here, as a teacher, is that so little of it is published when I am aware that so much has been done at places like Dinefwr and Strata Florida for example. I am not pointing an accusing finger, because our own study at Carew has yet to fully reach the light of day! This may be a case where a regional or even national review conference or seminar may bring our achievements more clearly into the light of day and assist the process of inter-disciplinary contact, especially if it is accompanied by the publication of the proceedings.

Historical Geography

There has been a significant contribution by historical geography through the morphological analysis of maps, the exploration and reconstruction of territory, the examination of status and social structure in the landscape and the mapping of documented activities such as render and obligation as well as the processes of agrarian and commercial life. We are still, for example, deeply in debt to the abiding authority of the William Rees maps, to which we constantly turn despite the fact that they lack basic reference to sources and that they make huge cartographic assumptions such as the dependence on nineteenth-century parochial boundaries (Rees 1951). The more recent historical atlas produced by the Board of Celtic Studies is actually of much less use to archaeologists although it makes some general spatial points (Carter 1981). We certainly need a more secure cartographic base for our historical understanding of Welsh topography in the southwest and I would hope that we can turn soon to the up-dating and refining of the Rees inheritance.

Perhaps the most significant contribution of historical geography has been the exploration of settlement forms through the use of morphology. Since the exposition of this methodology by Conzen ****, Uhlig **** and others, there has been much dependence on using it to identify the medieval cores of towns and villages, howbeit usually supported by very little independent corroboration. Most archaeological uses pay little attention to the severe limitations of both the method and the source material, usually eighteenth or nineteenth century maps (Austin 1985). The work of Soulsby on the towns of Wales, outside of the larger bastides, often bears little close scrutiny. Unfortunately such speculative identification of early cores tends to carry a lot of weight in impact assessment, settlement characterisation and resource classification. However, the testing of morphological models can sometimes be achieved as witnessed in the classic report on Newport Pembrokeshire where the morphological model generated by Nicky Evans from maps and medieval rental surveys could be tested by area excavations (Murphy 1994a). A significant component of the process of twelfth-century planning and colonisation was revealed in this work. Where, most significantly, the use of morphology has not been corroborated by field work is on the villages of south Pembrokeshire where the assumption abides that their plans, as much as those of the boroughs, bear the finger-print of twelfth-century planning and colonisation, linked to the documented Flemish settlement (Kissock 1990). This and the associated assumption about the co-axial field systems of the area in historical identities and must be subject to some focussed research (see Kissock 1993). Indeed the work of Margaret Davies as long ago as 1973 represents the last serious overview of medieval field systems in South Wales as a whole and again too much of this was based on morphological assumptions about date and origin (Davies 1973).

Even more controversial than this is the crucial set of ideas and metanarratives now focussed around the notion of the multiple estate which has become compounded, on the one hand with earlier empirical analyses of land use and render (e.g. Jones-Pierce 1959a & b) or anthropologically-derived concepts of transhumance from valley floor to mynydd, from hendre to hafod (Sayce 1956 & 1957) and on the other with the need to understand territory and community as a necessary corollary of understanding cultural landscape. This essentialist model defining discrete territories bound by lordship and kin and rooted in a vaguely ‘Celtic’ past was conceived by Glanville Jones in Wales and then extended into England and Scotland to identify complex systems of agrarian life and social order with a common origin in a pre-English, pre- and proto-history (Jones 1964, 1966 & 1972). This attempt to understand of a complex society in Wales, markedly different in nature and purpose from the Anglo-Norman village, parish, manor and household, goes back to ancient debates on law and institution in Wales (e.g. Seebohm ****) and is much critiqued (Davies; ****) Archaeology in our region, however, has done virtually nothing to test the power of the spatial and organisational hypotheses in this work, despites Jones’ frequent propositions about hill-fort, hendre, settlement and landscape. Whatever the difficulties of this work, it has generated nonetheless a compelling model of how the Welsh landscapes, such as those of the upper Cothi valley, might have worked. What else do we have? Archaeology of itself has generated pathetically little that is coherent about the larger spaces of the Welsh landscape in the middle ages and yet we are very prepared to deploy historical geographical assumptions in such work as historic landscape characterisation. The difficulties are enormous: there are few relic earthwork or crop-mark landscapes of this period (but see Muckle and Williams 1992) and the scattered remains of the hafodydd have permitted only very limited excavation (Butler 1963 & 1996; Austin 1988). Despite the difficulties, one aspect of the research agenda must be to extend the range of methodologies and the scope of work to enable us to meet the historical geographers in the spaces they inhabit.

Palynology and ecological history

Another major contribution is being made through environmental science and here the objective is an ecological history that is permeated by interpretations of anthropogenic factors at work in the system. A very large amount of pollen analysis has been undertaken, much of it recently of a very sophisticated form in the upper parts of peat profiles (e.g. Buckley 2001 & Cors Caron) where the medieval period lurks. We have, at Lampeter, recently mapped, for example, the pollen work done in the Teifi Valley and it is an impressive corpus of data, but we still need to produce a clear synthesis and overview. Without this work some of the false assumptions drawn from scanty documentation about, for example, woodland in the region will go unchallenged (Welsh Woodland). The need for synthetic research about environmental and ecological history is desperate, particularly when we can also see on the Audit maps a growing body of material extracted from excavations. Much of this, however, is haphazard and unrelated to critical historical questions of land use and habitat and seriously requires a research agenda for recovery and methodological advance.

Archaeology

In the preceding sections it should have become clear that much of the agenda has been set within the context of other disciplines, although explicitly archaeological methodologies of excavation and survey have been used. The principal focus has been on buildings and the spatial arrangement of settlements. Material culture, by contrast, has been relatively poorly served with much that is contained in the detailed substance of finds reports. Pottery has received some synthetic analysis, notably Papazian's pan-Wales review (Papazian & Campbell 1992) and Freeman's MPhil thesis (1995). However, there is a worrying trend for artefact texts to be separated from the main body of their reports (Brennan and Murphy 1993-4) and even not published at all, as at Carmarthen Greyfriars (James 1997). This will not help the process of research.

Dominant explanatory models

Inside both the data and the historical syntheses made from them there is a powerful diachronic at play which informs, but sometimes fragments the debate into periods when some of the most interesting issues from an archaeological perspective are the changes between and within periods. Within this diachronic narrative there are also specific modes of description, explanation and critique, which provide the various agenda items for research. It is worth rehearsing this diachronic, albeit schematically:

1. Pre-Norman and surviving Welsh society in Pura Wallia (10th and 11th centuries):

The essence of this is a pattern of explanation which suggests that south-west Wales until the Norman Conquest was largely unaffected by changes in the English state, buffered by the Vale of Glamorgan and having to deal only very rarely with Old English kings and earldormen - a pattern suddenly disrupted by the *anschluss* of the Montgomery brothers in 1095. The elements of this explanation appear to be:

Small kingdoms, occasional hegemonies and partible succession - engaged as explanations for the failure of the Welsh medieval 'state' to emerge: there is the suspicion that elite sites continue to be, in part, Iron Age/Dark Age in type as hinted at Carew (Austin ****), but this is far from being proved or being demonstrably consistent - the surprise of Dryslwyn **not** being a hill-fort is an important corrective to stop us jumping to generic conclusions - much more needs to be done here.

'Clan' landscapes: complex, but discrete territorial entities centred on kinship relationships as exemplified in the Welsh Law Codes: the multiple estate model is one way of reconstructing this: the predominant impression is one of long-term stability and continuity in the countryside. For all the suggestions, however, about hierarchical landscapes, usually drawn from 13th century and later documents such as Jones (1972) for the Cothi valley, no archaeological investigation has seriously taken place to explore the hypothesis

Intimate patterns of land use dominated by cattle and localised arable production for subsistence and tribute, little influenced by trade: it is often assumed, albeit not very explicitly that the 'hendre/hafod system goes back into this Old Welsh past if not back into a 'tribal' prehistory - these old summer pastures and limited transhumance (Sayce, Davies, Butler) based on short-distance relationships across the countryside for communities measuring their wealth in agrarian produce, particularly cattle, and their status in obligation and render. However, archaeologically none of the few excavations on rural sites have shown deep antecedents in a pre-12th century past, even hafodydd sites, although Ward has suggested earthwork sites that might fit into this category.

A Welsh monastic ('Celtic') church: the so-called 'clas'. Despite the formidable place of these institutions in the Welsh cultural psyche, even the most obvious sites, such as the main monastic centers (Llandeilo, Llanbadarn, St Davids), have remained untouched. The review by Niall Ludlow and the rigorous re-vamping of the Early Christian Monuments book by Nancy Edwards, both in an advanced stage of production, will undoubtedly move this whole debate forward and I have been sent a draft paper by Charles Thomas on Henfynyw in Cardiganshire which certainly challenges me!

That there is virtually no contribution yet to these issues from archaeology in this region, is partly because of the invisibility of both material culture and typologically identifiable sites. Here, almost certainly, we are going to need both methodological experiment and advance, as much as concentrated, albeit speculative focus on likely sites and landscapes.

2. Norman expansion into southwest Wales (end of 11th to later twelfth)

Southwest Wales as frontier between English and Welsh, between 'Germanic' and 'Celtic' is an abiding mode of explanation. The validity of frontier as model and of conquest and conflict as the primary motor of cultural and political change has been seriously questioned in recent years by historians (e.g. Davies ****).

However, the sudden visibility of material change in the archaeology (castles, towns, villages, stone churches, manor-houses, pottery, coinage, etc) is a potent force that leaves us still explaining change primarily in these terms. We need to

focus on re-evaluating our evidence in the light of revisionist histories, especially those that attempt to break away from confrontational 'nationalist' agendas constructed back in the nineteenth century (cf. O'Keefe **** for Ireland). One useful corrective is to consider the phenomenon as pan-European and to make cross-cultural comparisons. This helps to get us away from overemphasising the very specific histories of place and region. The main pattern of explanation, however, remains polarised around two oppositional grand narratives:
Conquest, domination and Normanisation - permanent in the south and temporary in the north with certain key phenomena:

Town plantation (Soulsby ****, Carter ****, Murphy ****), trade, sudden expansion of the range of commodities through import and production by colonial specialists (pottery etc)

Village plantation and introduction of open fields (Kissock ****, Howells ****, the Gower excavations just outside our area)

Castle building as the material trace of English royal and baronial authority: an assumption still exists that earthwork castles represent a fundamental cultural change: this is debateable: most stone building is twelfth century or later in origin - in other words not part of the first wave of 'conquest'

Church building following the Regularisation of Episcopal authority

Monasteries following monastic reform and the creation of copyhold estates

In the context of chronologies here it is worth noting two recent areas of thinking about chronology: Gardiner, as mentioned above, has argued that the building of stone-built structures, including integrated hall complexes, comes in the mid to later twelfth centuries and is a part of a watershed in the creation of domestic architecture, both polite and vernacular in England and elsewhere which in turn may reflect a fundamental change in social world-view. I have come to very similar conclusion myself independently. This goes along with an emergent opinion that village creation, as a consciously planned process is also a mainly twelfth or even thirteenth century process with the exception perhaps of the peripheral parts of Europe away from the great core of Northern France and southern and central England

Welsh resistance and adaptation in the northern upland - seen as involving palpable changes in elite culture (e.g. castles), but little in the rest of society - a view that Welsh rural culture continues largely unaffected, although archaeology has virtually nothing yet to contribute to this - it would appear that the continuing archaeological invisibility of rural settlement in the upland, whether valley floor or Mynydd, is the strongest evidence for a lack of change. Perhaps the most significant aspect of this is the almost complete absence of 'expansion' onto the mountain wastes as we have on Dartmoor, the Pennines and other European upland areas.

3. Post 1284

This date, as much as the 16th century Acts of Union, is totemic in Welsh history and with good cause whether we call it 'conquest' or 'imperialism'. It is, as with all specific dates, undetectable in the archaeological record, but there is little doubt that from the early thirteenth century onwards the material culture of the upland south-west starts to become more archaeologically visible, not least because of fundamental changes in trade and the processes of exchange and render. In the southern, more 'English', areas there are important advances in trade, production, and building of all sorts as the medieval culture begins the long process of diversification and pluralisation of commodity. The dominant modes of explanation here are:

Imposition of English or anglicised jurisdiction, governance and elites in Pura Wallia
Gradual formation of a consolidated class of Welsh and Anglo-Welsh gentry producing 'manorial' type houses and building complexes. Here, very significantly, the comparatively large amount of work on the castle, e.g. Laugharne and Carew, has contributed some very clear perspectives on how the nobility and gentry alike were acting to transform their architectures and designed landscapes, particularly in the fifteenth and sixteenth centuries, to the new realities of a centralising state and economy and their attendant ideologies. This can be put into line with newer interpretations of the changes to modernity (Johnson ****)

Slow transformation of the hafodydd into more permanent settlement, accelerating during the early modern period into cottage or 'squatter' settlements validated in the communities, contrary to English common law, by orally-transmitted Welsh traditional practice (ty-unnos). Excavation (Butler 1963; Austin 1988), survey (Ward 1991 &1995; Austin et al 1984-8) and assessment (e.g. the Mynydd y Ffynnon Project: Sambrook 1997a, b, c, 1999; Sambrook & Hankinson 1999, 2001) have all provided both intensive and extensive information for discussing these processes. At Bryn Cysegrfan I was also able to show the relationship of such building complexes to the development of an extensive rabbit warren in the later middle ages (Austin 1988). Nevertheless how these related to the lowland farms is difficult to assess without material from such locations.

This kind of specific explanation of change in terms of domination may mask the fact that south-west Wales is little understood or presented in terms of the dominant economic narrative to be found elsewhere in Europe, that of late thirteenth and fourteenth-century crisis and slow recovery during the extended ideological and dynastic struggles of the early Reformation and Renaissance state-building.

The archaeological phenomena of this are rare or absent, such as deserted villages or abandoned field systems, if only because they had been weakly developed in the twelfth and thirteenth centuries. However, even English South Pembrokeshire where the village is perceived to have arrived during the twelfth century clear signs of classic desertion are extremely rare. This suggests that our explanations may need to be examined, even to the extent of wondering whether village Pembrokeshire was a much later creation or was even largely non-existent. This contrasts clearly with the signs of late medieval decline and abandonment of tenements in the planned boroughs, such as Wiston or Newport, which suggests that the European processes were indeed operating. How climate also fits into this is unclear: whereas on Gower and along the Glamorgan south coast increasing storm episodes and the Younger Dune formation is in progress, this is not so apparent along the rocky promontory of the south-west, except spectacularly at Newport again.

One clear aspect of the trend into modernity is the failure of monasticism, as explored by Sian Rees at Haverfordwest Priory (Rees and the DAT at Carmarthen Grey friars (James 1997) and Augustinian Priory (James 1985). Ironically this failure is more often explained predominantly in terms of pan-European processes and linked closely to the English Reformation, but what we need more is the particular experiences of change investigated at local level which can explain why and how the elements of a Welsh practice of Catholicism were dispersed and transformed.

3. Observations on the contemporary context of research into the Middle Ages

There are debates in medieval archaeology as in all disciplines of the humanities and social science on theory and method. This is not the place to rehearse them, except to say that there exist now some radically different views of what our research agenda might be. This is aided and abetted by historians and historical geographers who have also moved their agendas on, some of the most striking of which relate to the latter end of the middle ages and the changes into modernity and capitalism. Long may such difference continue because it invigorates debate, but we must recognise that there is more than one valid approach and more than one potential agenda. They cannot always be harmonised.

The structure of information as shown on the Audit maps especially 'Medieval sites recorded in the SMR' is essentially based on categories which are largely functional and determined by generally accepted and common sense historical understandings of the nature of the past especially those derived from studies of institution and economy. We should be wary of believing, however, that such categories of themselves form a research agenda or even the framework of such an agenda.

Such an approach at the level of handling and improving the quality of primary data in SMRs, reports and is essential and should not be changed, partly because it has been developed through practice and is thus 'time-honoured', partly because it matches the 'specialist' interests of many individual researchers and partly because it also allows other more integrative approaches to be built from it with some security in the validity of the data and the meaning of the terminologies used. I would not advocate changing terminologies and structures of record. We must as medievalists, however, recognise, that no longer are the singular concepts of 'castle', 'monasteries', 'towns', 'vernacular buildings', 'domestic architecture', 'rural settlements', 'pottery', 'artefacts', and even 'environment' sufficient in themselves as justifiable research frameworks. Nor indeed, at the other end of the scale, are such catch-all concepts as 'landscape' or 'settlement morphology'. Modern research agendas in the understanding of the Middle Ages in Europe require us now to construct more complex and thickly textured accounts of human interaction and society. Ultimately we need to be putting our archaeological and material accounts alongside, and integrated with, for example, the work of Duby on private lives or Dyer on the ways of living. These see the Middle Ages as complex integrated matrices of human action, operating on a variety of scales from the individual to the regional. These people lived through the spaces of buildings, streets, mines, fields and woodlands without our typological distinctions between castle, field system or product. The best of our work does this, the worst of it is disintegrated fragments, the purpose and usefulness of which is uncertain to archaeologists, let alone historians. Good research must have the informed purpose to contribute to the wider discourses of the Middle Ages and to do so in a targeted and conscious way.

Recently I have argued that perhaps the best contributions archaeology can make to the study of the medieval past are specific, deeply understood accounts of places, localities, in the landscape where the connections between things and people can be intensively explored to support or challenge some of the fundamental often reductive assumptions made about the workings of specific types of building or economy or demographics or ideologies. For example, much that is written on population in the Middle Ages is predicated on multipliers derived from family reconstruction and analysis in the better-documented sixteenth and seventeenth centuries. Our perception of change in the medieval economy is very much driven by the idea of growth from the eleventh to early fourteenth centuries, collapse in the later fourteenth and early fifteenth and revival into the end of the middle ages and on into the eighteenth century when industrialisation provided the technological key to non-traditional demographic explosion. In Wales, we have little primary understanding of how this all worked. Everything we understand from the laws or social reconstruction puts emphasis on extended kins until the later middle ages and under these circumstances the multipliers of England do not apply. In the archaeology, however, there is not enough good excavated material for us to review this especially with such a strong bias towards upland and seasonal building complexes. However, we do know that, unlike many tracts of English and European upland and wold from the ninth and tenth centuries onwards, there is little or no substantial colonisation of the 'waste' until the post-medieval centuries in the south-west and other parts of upland Wales. We might suspect the force of social and cultural tradition, but in the end was the population of Wales rising or falling in this period? When we do know for certain that other aspects of European economy and institution were finding their ways into our landscapes? Documents will not contribute, but archaeology might. This is only an example of the potential contribution of archaeology through intensive interrogation and analysis of specific localities where we can begin not just to receive information about the readily visible (earthworks, standing architectures, morphologies), but also to interrogate the massive lacunae on the valley floors, for example, and strive, in a sustained way, to find methodologies which might allow the hard-to-find to be found. In this way we may begin then to see the whole of a community so that this can inform the understanding of other more fragmentary evidence whether archaeological or documentary gathered on the wider scale. In this I would point to projects such as Wharram Percy, West Heslerton, Roadford, Shapwick or Raunds where the distinction between research and rescue has been blurred and wider research objectives have been supported by chemistries of resource including both funding foundations and English Heritage alike.

Research unfortunately has been influenced, even driven, by management, curatorial and rescue agendas, albeit conducted to the highest methodological and intellectual standards: in Wales we have, largely through professional dedication, collectively made the best of the opportunities we have been given. What we know of the material culture of southwest Wales now is infinitely greater than what we knew 30 or 40 years ago, but we must also recognise that it is partial and opportunistic and perhaps has dealt with some of its most obvious and most visible aspects. So we have done castles well, boroughs reasonably well, uplands well, but valley floor farms and their environments very badly. The map constructed from the SMR which looks so impressive is full of chance encounters, place-names and passing references, most of which needs thorough and disciplined examination, before they can be used to drive fully researched narratives. Maybe we need to begin with discussing how to make the SMRs an even more powerful tool for research.

The extreme partiality of what we have been able to achieve is in some ways due to the structures of administration, institution and law that we have in the region and in Wales as a whole, but again it is also due to the initiatives and expertise of individuals and groups who have been attracted to work in the area. I don't want to dwell on these, but it is essential that we acknowledge throughout this process that these issues are fundamental to the full and proper implementation and furthering of the research agenda. I shall return to this briefly at the end

Specific projects, of course, move us beyond single types of site or material, because obviously we will find a range of evidence in our excavations, but often we either cannot or do not design for ourselves the capacity to contextualise the individual elements and we have to settle for truncated statements of data and analysis. We know this is about the pattern of resource we happen to have, but I do think it is also a bit because in Wales we have allowed ourselves to become over-compartmentalised by the constraints of statutory obligation, contract, institutional ambition and mission statement. There is no standing or recurrent conference or seminar series on medieval archaeology as a whole in Wales, although there are opportunities created by specialist groups. If we examine for a moment the work that has been done in recent years we can detect some primary objectives, which are not those fundamentally of historical research as such, but rather those of institutional function. This includes universities as much as others, not least because of the restrictive conditions of the Research Assessment cycle, which is not conducive to long-term, project creation. This is not to say that good research is not done, but rather to say that it is not the governing determinant of our actions. My list of such determining categories with some of the more prominent published pieces of research might be:

Monument management and development

Haverfordwest Priory;
Laugharne, Dryslwyn, Carew, Cardigan and Newcastle Emlyn castles;
Pembroke and Tenby town walls

Buildings conservation

Bishop's Palace, St Davids ; King's Court, Talley; Penallt, Kidwelly; Newport Castle

Rescue

Wiston, Pembroke and Newport urban areas;
Carmarthen's monastic complexes;
(Capel Teilo?) and Llanychlwydog churches,
Llanfair Clydogau rabbit farm and hafodydd, Marros Mountain field system

Curation and record strategies

(Too much of this is not published and hence not available to scholars - why is this?)

Churches survey (Ludlow 2000)
Medieval and later rural settlement (?)
Historic Landscape Characterisation (?)
Houses of the Welsh Countryside and Ceredigion houses (Smith 1988 & 1998)
Lawrence Butler's work for RCAHMW in the 1960s on upland house sites (e.g. Butler 1963)

Heritage in economic and social development (Community Archaeology)

SPARK villages brochures
Mynydd y Ffynnon
A number of specific community-based projects by DAT and others

University-based teaching and research programmes

Cellan and Caer Cadwgan project (Austin, Bell, Burnham, Young, UW Lampeter); Black Mountain (Anthony Ward, UoKent); Pembrokeshire and Carmarthenshire villages (Jonathan Kissock, UWC Newport); Carew (Austin and Drew)
MPhils & PhDs?

As far as excavation is concerned there is a huge emphasis on the great 'public' buildings of the region, driven often by economic considerations of heritage tourism. The use of curation management resources for survey have redressed some of this balance, but has left us with a broad disjunction between the deep intensive knowledge of elites through excavation and building survey and broad extensive knowledge of largely late medieval to early modern rural culture through survey with, fittingly, urban areas sitting somewhere in the middle with both excavation and survey, usually driven by morphological models. We have not yet had, in the state-funded sector, the mixed strategy of survey and excavation that has served Wales so well in the Llys and Maerdref project in Gwynedd.

4. A shopping list of what is needed:

A. Requirements of Method
The structure and approachability of our primary data

Continued research to refine our specific archaeological knowledge of sites and locations to improve the quality of the data-bases

A more conscious attempt to work with other disciplines

The provision of focussed inter-disciplinary and cross-sectoral programmes of research

B. The dominant lacunae in basic knowledge
In the upland north - what is happening on the valley floors - a considerable hindrance to our understanding of how communities worked as a whole.

Excavations and detailed survey of small towns in the 'Welsh' areas - very little to compare with the surveyed and excavated colonial Anglo-Norman towns

Detailed survey and dating of field systems - even the well-known Llan-non system has had little survey, has not been scheduled, and was very recently under threat.

Intensive understanding of historical ecologies, especially in the upland - this requires more synthetic research, but also a clear agenda created in co-operation with environmentalists - most work is still site-specific and dealing with the serendipitous results of rescue sites.

No excavations of medieval industrial complexes

Very few sustained programmes of landscape research in the region, although there have been important surface assessments, especially in the Upland Survey, of the range of survival and resource. There has been little targeted excavation and historical research to follow this up. We have gained little which can yet contribute much of significance to the narratives of medieval society and economy in Wales and Europe. If we were to refer to the citation indexes, we would find that there is precious little reference to Welsh research in European syntheses, except the Edwardian castles. I do know from my own experience that in work on the upland and rural cultures of Europe, Wales is hardly ever mentioned (but see Austin 1999).

This may lead us to the selection of places which have the right kind of information to enable us to pursue more integrative agendas - Map 1 may begin to give us clues: the extraordinary richness of the St David's peninsula is a clear case for evaluation; Lampeter has chosen the Upper Teifi valley around Strata Florida

C. Themes

- Cultural expression and identity: style, design and the inter-relationships of power and economy - especially within 'Welsh' culture
- Processes of cultural change

- 'The people without history' - we know extraordinarily little from archaeology of the rural 'peasantry' of the Middle Ages

Paper prepared by Professor David Austin (University of Wales Lampeter)

Note (January 2008): This version of the text was mounted on the Research Agenda website in 2003. In this draft, some references lacked dates of publication (indicated by asterisks) .