

## **A Research Framework for the Archaeology of Wales**

### **The Medieval**

What follows is an initial and somewhat raw statement drawn together by Andrew Davidson, Bob Sylvester, Jon Kissock and myself, David Austin. I have hurriedly pulled together, in the last twenty-four hours, the four separate pieces provided by each of us to give the meeting some sense of what emerged from the regional papers. In one or two places I have exercised heavy editorial control and thus this paper had best be regarded not as the considered final product, but as work in progress. Because of this history the structure is somewhat topic based and, in my view, somewhat traditional, but I think we all agree that the future is for us to view all these elements as inter-dependent and best furthered by greater research synthesis and by more focussed landscape-based studies. We might also agree that some extensive, research-funded programmes of excavation are desperately required on rural and minor urban sites and landscapes.

### **Rural settlement and housing**

There is a strong tradition of research into the rural settlement and housing of medieval Wales. This has focussed largely on surviving buildings of a later period, and on debates about the nature of folk tradition, whether it is indigenous to Wales or cascades down through the social structures and outwards from England and Europe. At present the balance of evidence seems to favour the latter. Excavation and field work, as demonstrated by the recent DRS project has tended to follow the agendas and terminologies of that debate, rather than find new ground for archaeology.

In terms of settlement, two primary dichotomies dominate the discourse: the polarised distinctions between dispersed and nucleated and between upland and lowland. In these terms we know far more morphologically about the nucleated since we believe, although based on little evidence, that these are still apparent in 18<sup>th</sup> and 19<sup>th</sup> century mapping and have their origins in the Anglo-Norman conquest of Wales. By contrast the dispersed forms are elusive and deeply problematic, although probably the dominate settlement type of most of Wales in the Middle Ages. We also know far more about the abundant sites present on our uplands than our valley floors apart, perhaps from the deserted villages of Glamorgan.

What is desperately needed for Wales is a synthesized account of the settlement forms and material presence of medieval rural society with its rich regional variation. This needs to be contextualised within the framework of adjacent political entities and with an explicit landscape approach. On the other hand we also need one or more projects which will provide deeper studies of the medieval and later rural landscape and its significance. This should include extensive excavation, especially on valley floor sites as well as on a range of non-domestic sites.

## **Land Use**

Outside of the main conurbations Wales has a relatively well-preserved landscape, much of it upland in character. Certain themes immediately become apparent as the focus for future research, namely settlement systems and the division of the landscape into the units in which it was managed. Current perceptions of the uplands have little chronological depth, yet data are becoming available from nationwide programmes such as the Uplands Initiative and these must be utilised and developed. Generally, systems of land allotment, and their chronological and spatial variation urgently need reconsideration, not only through the traditional functional explanations, but also the social constructions behind the land-use systems. In south Wales, for instance, there has been hardly any discussion of field systems since the 1970s.

## **Medieval Industry**

There is a need for assessment of sites by industry. Metal mines have been assessed throughout Wales, but little work has been undertaken on limestone industries or granite quarries. Millstone quarries also merit assessment. The identification of an area of 17th century coal mining on Anglesey is a good example of how an industry specific assessment can be successful. Analysis of building stone to identify their source would provide a clearer understanding of trading patterns and resource utilisation, perhaps in conjunction with the new Building Stones Group.

Water mills and wind mills were widely used throughout the Middle Ages, and were one of the earliest forms of use of mechanised power. Historical sources contain abundant evidence that, linked with archaeological research, could aid our understanding of tenurial obligations (e.g. bond townships owing suit to royal mills), transport routes and technological development. Of particular interest would be information concerning use of horizontal water wheels, and the location of former post mills.

Landscape studies examining the relationships between industry, settlement and agriculture would allow a clearer understanding of the development of industry, and provide essential frameworks around which research work could be targeted.

There is considerable research potential in the study of the distribution and marketing of medieval goods, particularly ceramics.

## **Coastal Studies**

The coast environment offers both threats, because of the erosive forces arising from the dynamic forces operating on the coast edge, and opportunities from specific coastal environments, including buried peat deposits, waterlogged deposits and sand dunes.

Coastal archaeology is defined as archaeology within the coastal zone. It therefore does not have to have a specific maritime function, though it often does, and the archaeological resource to be found there is invariably specific to the coast. An assessment of medieval ports and harbours is of particular importance. Though much historical research has been undertaken it has not been matched by a similar number of archaeological studies, and considerable work both on identifying and recording medieval harbours and placing them in a wider landscape so that account is taken of related settlements and land-based transport routes needs to be undertaken. Wider cultural maritime landscapes, both nationally and internationally, also need to be identified to aid interpretation and management of archaeological remains.

The value of shipwrecks as a source of archaeological evidence is well known. The Magor Pill wreck has added considerably to many facets of research in medieval times, and the Newport wreck has similar potential. An assessment of the resource, or even identifying potential sites (for example former harbours that are now silted up) would help improve our understanding.

### **Religious Sites**

Much has been done in past years on major monastic sites, but we are only just beginning to understand their landscapes and much needs to be done of how they both changed and were changed by the pre-existing social and economic order. There is also the feeling that the Cadw-funded Welsh Historic Churches dataset should provide a base for more detailed studies of individual minor churches and their landscapes; and allied to this is the concern about the lack of church excavations. Then the transition from the early medieval llan to the medieval church deserves more attention, and there is also a need to look at the other site types such as chapels, many of which closed before the Reformation. The absence of work on friaries has been flagged in some regions and monastic cemeteries merit attention in the future.

### **Urban settlement**

It is generally agreed that there is a shortage of focused excavation within urban settlements generally, and particular concerns in some regions that the nature of Welsh as opposed to Anglo-Norman urbanisation is little understood. Even where work has taken place on a more regular basis, published syntheses are rare. Nor should we forget sites lower in the urban hierarchy such as large villages, but advances in our understanding of these smaller nucleations will come only if enough work is promoted within their historic cores. All of the regions reveal a lack of recent detailed town surveys, and studies utilising plan morphology analysis must be encouraged. Specific urban aspects could usefully be addressed: road systems, bridges, market places, manufacturing and industry, and the interaction of towns and their rural hinterlands all come to mind. Finally there are some regions where little work has been done in recent years on town walls and gates, and by extension the extra-mural suburbs outside them. This could usefully be rectified.

## Castles, fortified and other high status sites

Much work has been done on documentary and architectural studies of castles in Wales based on a strong tradition of scholarship focussed on the military and colonial aspects of the Welsh narrative.

There have been a number of key excavations in recent years, largely on Guardianship and well-protected sites, most still to be published. These have been aimed both at the extension of knowledge about Wales' dominant medieval structure and at the development of their representation within the heritage and tourism industries. The Castles of Wales are a primary heritage icon. This has led to the paradox that we have focused a great deal of the research resource available in the public sector on the one form of structure about which we already know the most.

Only in Gwynedd has the Llys and Maerdref project been able systematically to identify locations and architectural forms for the middle ages, but is yet to establish the elusive links with antecedent, pre-12<sup>th</sup> century power and authority.

There are lacunae:

In terms of areas: limited work in Gwent, especially on earthwork sites; in Gwynedd insufficient and inconclusive work on the pre-12<sup>th</sup> century high status sites and mottes; in Clwyd, Powys and Dyfed almost no understanding of the centres of Welsh authority and their translation into later medieval forms;

In terms of theme: few studies on the landscape context of castles including gardens; not enough regional contextualisation of castles; little or no excavation work on moated sites and tower houses.

## Theory

Finally as a codicil, my colleagues asked me to make a few points about the role of theory in research. This is a wide subject and a contentious one. With so little time today I would wish only to make a brief statement. In my view all archaeology is political. If it does not admit to this, it can only fail to engage in the principal concerns of our contemporary society. If we fail to engage, then we become irrelevant and dispensable. In Wales the principal political agenda is identity and it is revisionist. For a long time the identity was one of resistance against a mighty neighbour and there was a temptation for this to be singular and monolithic, asserting its uniqueness and its non-Germanic characteristics. The other identities were intrusive and colonial. Like monkey-puzzle trees they were regarded as non-native plants. Now we find ourselves in different circumstances. Wales seeks to be a nation. To achieve this, however, with maturity we must engage in revealing the diversity of our traditions, the regionality of our culture, the constant influence not just of our immediate neighbours, but of the wider world and, perhaps most importantly, the nature of our conflicts and contentions beyond those of the simple myths

of colonialism. Wales is diverse with multiple identities and the climate is that of multi-culturalism. Most of that diversity has its narrative roots in the Middle Ages, both early and late, a period which provides also some of the most important of the material symbols representing us and our political trajectory. We need agendas informed by such considerations, not those mired in the narrow parochialisms of a fading politics. To achieve this I fundamentally believe that the alienation of the academy in Wales, and indeed the rest of the world, from the state and developer funded field practitioners and researchers must end and end very soon.

### **Towards a strategy**

*Settlement studies* - In upland Wales (Pura Wallia), there have been extensive review studies and fieldwork on rural settlement, especially above the 250-metre contour. In England there have also been both extensive and intensive overviews of settlement. By comparison our understanding in Wales, particularly in the upland areas of dispersed settlement, is very poor. There is especially a lack of deeper, more intensive studies of discrete areas. This needs to include excavation and related field and environmental work, on medieval, secular settlement sites of different status together with their landscapes, both on the valley floor and the mountain. We have massive lacunae in our knowledge of dating, morphology, ownership, land use and change.'

*Landuse* - In Wales as a whole, there are considerable issues in relation to the identification of land use in the middle ages, including assumptions about function and date made in retroactive analysis from modern mapping and from documentary studies of economic history. Many assumptions are being made particularly in landscape characterisation which are largely untested. This is particularly true of enclosed field systems which are too often assumed to be late. This needs also to be integrated with studies of the cognate settlements of such landscapes, especially dispersed farms, hamlets and smaller nucleations, including those with churches.

*Industry* - There is a lack of coherent knowledge of the production and products of medieval industry in Wales. In particular there need to be more industry-by-industry studies and landscape contextualisation of the production locations. This work should be integrated more closely with detailed artefact studies to establish the technological bases of production and to establish an understanding of the milieu of markets and consumption as well as the social context of their use and meaning.