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Exploring Anglo-Saxon Landscapes

The English landscape, in all its unique variety, has been moulded by every stage of human habitation. Yet it is to the least obvious and least regarded of those stages that it owes some of its most distinctive features, both in broad-brush regional diversity and in local detail.

This book explores that lost world of the Anglo-Saxon built environment and its relationship to the wider natural and man-made environment. It analyses the division of England into culture zones, their fluctuation through time, and the regional distinctiveness of settlements within them. Its sources are mainly archaeological, above all the rich harvest of developer-funded excavations since the 1980s, but its various strands lead outwards from the material to the economic, social, cultural, and conceptual. Its unifying theme is how people behaved within *constructed space*: the rural space of farms, the ritual space of holy landscapes, the administrative and defensive space of forts and installations, the hierarchical space of great halls and proto-manorial sites, the commercial space of towns. It argues that those various constructed spaces were often both sophisticated and elegant, outcomes of the same distinctive aesthetic culture that has long been celebrated for its small-scale artistic products.

Notwithstanding the many and excellent books that have both ‘Anglo-Saxon’ and ‘settlement’ in their titles, mid- to late Anglo-Saxon settlements—strictly defined—were little studied until recently. Compared with any other period from the late Iron Age onwards, hard evidence for the layout of houses, farmsteads, villages, and complex settlements has been remarkably fragmentary, so that accounts of historical, environmental, agrarian, and linguistic phenomena have tended to patch over the hole in the middle—the settlements themselves—with projections from much later evidence. Nonetheless, the historiographical tradition of exploring the social and topographical contexts of settlements is centuries old and sets the agenda for important parts of this book.

HISTORY, GEOGRAPHY, AND PLACE NAMES

Curiosity about English settlement forms, and their relationship to environment and social structure, is nothing new. Famously, the topographer William Harrison was already expressing it in Elizabeth I’s reign:

It is so, that our soyle being devided into champaigne ground and woodlande, the houses of the first lye uniformly buylded in every towne together, with streetes and lanes; whereas in the woodlande countries . . . they stande scattered abroad, eache one dwelling in the midst of his owne occupyng.¹

Harrison appreciated the broadly regional nature of this dichotomy and its implied contrast between what might be called the ‘structured’ and the ‘individualistic’ zones of England. Its origins and causes, and the extent to which the more integrated and organised forms of settlement reflect stronger top-down direction and control than the dispersed and unstructured ones, have intrigued many writers since then. The present book confronts that persistent problem from a new angle.

Another long-abiding theme, once thought to go to the heart of early English identity, is concerned less with the form of individual settlements than with the bonds of territorial organization and obligation linking them together. It goes back to what early Victorian scholars (following German ones of the previous generation) defined as the ‘mark theory’: the idea that early Germanic folk groups lived in territories with grazing and other resources held in common.² Shorn of its ethnic overtones, the basic reality of this mode of organization—what I shall call the *regio*—in most parts of early medieval Britain was recognised afresh, with varied emphases and terminologies, during the second half of the twentieth century, notably by the historical geographer Glanville Jones and the historian Steven Bassett.³ Still helpful is James Campbell’s definition from 1979:

The essence of the argument is that the system of lordship and local government over much, possibly all, of early England resembled, and at least in wide areas, was connected with that of early Wales. The main unit in such a system was an area of varying but substantial size (say, not less than a hundred square miles) centred on a royal vill. To this vill the settlements within its area owed dues and services of some complexity, including those intended to provide for the ruler and his court on regular tours, and to maintain his men, horses and dogs on other occasions. The area centred on the royal vill would often or always have common grazing. The subordinate settlements could vary in the nature of their obligations.⁴

Many of the settlements discussed in this book—at least in the period before 950—will have been located in large, internally diverse territorial entities like this. Characteristic of such territories are the forms of control and exploitation from above that Geoffrey Barrow and Rosamond Faith have termed ‘extensive lordship’:⁵ conservative rather than entrepreneurial, receiving complexes of services and renders rather than rents, and based on established connections between broad ecological zones rather than the dynamic exploitation of confined ones. It was probably only gradually, with the expansion of ecclesiastical bookland

¹Printed in R. Holinshed, *The First Volume of the Chronicles of Englande, Scotlande and Irelande* (1577), vol. 1, p. 90.

²Campbell 2008, 34–35.

³Jones, G.R.J. 1976; Bassett 1989, 17–23; Charles-Edwards 1989; Blair 1991, 12–30; Grant 2000, 63–64; Blair 2005, 154, 252; Williamson 2013, 25–30, 34, 82–106. Note that historians now reject as anachronistic the term ‘multiple estate’ favoured by Jones and his followers.

⁴Campbell 1986, 95–96.

⁵Faith 1997, 1–14; below, p. 104.

from the late seventh century and small-scale secular proprietorship from the mid-tenth, that more intensive structures of lordship and exploitation developed.⁶

Did these successive territorial and land-management frameworks have much impact on the form of the settlements within them? The ‘*regio* model’ runs some risk of seeming stereotyped, changeless and all-embracing: life cannot have been so uniform, and there must have been significant diversity. In particular, the archaeologically rich zones of the East Midlands, Lincolnshire, and Norfolk are essentially undocumented, and we should not assume that the people whose buildings and possessions we find there lived under these or any other forms of socioeconomic constraint. Also, as we shall see, the notion of a stable ‘royal vill’ at the centre of each territory is archaeologically very problematic. Nonetheless, the ‘*regio* model’ is an essential context for themes that will be discussed here at length, notably the ‘great hall complexes’ of the seventh century and the constellations of linked functional settlements with *-tūn* names formed in the eighth.

Touching ultimately on the same range of issues, though from a different starting point, are the approaches that a series of scholars since the 1970s have applied to place names in their landscape contexts. It was above all Margaret Gelling who recognised the potential of these names, hitherto dismissed as ‘trivial’, to help us understand how ordinary Anglo-Saxons viewed the world around them. As Gelling indignantly wrote:

They have much to contribute to linguistic and geographical studies, and they offer marvellous opportunities for imaginative contact with the Anglo-Saxons. They are never ‘trivial’; the wolves of Woolley, the geese of Goosey and the swine of Swinford were matters of life and death to the Anglo-Saxons, and the choice of word to describe the settlement-site is as serious as any statement which our forefathers have bequeathed to us.⁷

In this justly stirring passage, we should pause on ‘the choice of word to describe the settlement-site’. We can be confident that place names describing hills, rivers, and valleys mean what they seem to say; but does a *-hām* or a *-tūn* necessarily describe, or even refer to, the village that now bears that name? The presumption that it does, in the absence of other evidence, is one of the few problematic strands in the work of Gelling and some of her followers. Still, Gelling not only showed that the Anglo-Saxons had a fine-tuned sense of the landscape, its vegetation and resources, but also traced the gradual emergence—against this background of names describing the natural world—of new layers more concerned with proprietorship and control.⁸ Cautiously used, some of these names can tell us something about the physical form of settlements.

Gelling’s methodology (and her outstanding qualities as a teacher) inspired a new generation. Two names stand out: Della Hooke, whose meticulous and insightful studies of the West Midlands, drawing on charter-boundary evidence, have recovered whole swathes of the Anglo-Saxon landscape in remarkable detail;⁹ and Ann Cole, whose flair for grasping the geographical forms underlying place-name elements led to an inspiring collaboration

⁶Below, pp. 312–14; cf. Faith 1997; Blair 2005, 251–61, 368–71.

⁷Gelling 1984, 6.

⁸Gelling 1978, 1988.

⁹Especially Hooke 1981a, 1985.

with Gelling herself, as well as a study of the landscape as perceived by Anglo-Saxon travelers.¹⁰ Recently, a series of workshops organised by younger scholars, resulting in a remarkable collection of essays called *Sense of Place in Anglo-Saxon England* (2012),¹¹ moved place-name studies beyond landscape and lordship to the dynamics of *doing things*: the multifarious agrarian, craft, and commercial activities in the landscape indicated by elements such as *-wīc*, or by ‘functional *-tūn*’ compounds such as Drayton, Charlton, or Eaton. These new understandings have done much to inspire the present chapter 6.

While some historical geographers have been attracted to the potential of place names, others have mined the evidence of historic maps for settlement forms and their development through time. In the 1970s it was Christopher Taylor who set an agenda for landscape history in his broad-brush but highly stimulating surveys, which did take account of such exiguous archaeological data as were then available.¹² The technique of using the layouts of still-surviving villages to construct hypotheses about settlement change and reorganization during the tenth to thirteenth centuries was taken much further by Brian Roberts,¹³ though his focus on northern England meant that he was almost completely unaided by direct archaeological evidence, or by written evidence before Domesday Book.

No general research on the historic landscape of England could avoid engaging with the dichotomy that William Harrison set out more than four centuries ago. His conception of ‘champaigne ground’, where ‘the houses . . . lye uniformly buylded in every towne together’, was given a new lease of life in two magisterial publications by Brian Roberts and Stuart Wrathmell, *An Atlas of Rural Settlement in England* and *Region and Place: A Study of English Rural Settlement*.¹⁴ Their outstanding achievement was to present, in various forms and in exquisite detail, the distributions of dispersed and nucleated settlements as shown on mid-nineteenth-century maps (Fig. 1). Combined with a wide range of other data from earlier sources, these distributions were used to divide England into ‘settlement provinces’, ‘sub-provinces’, and ‘local regions’, with a special focus on the ‘Central Province’. This zone, snaking across England in a sinuous belt of uneven width from the Dorset coast to the Northumberland coast, gave new form and definition to an old concept: Harrison’s ‘champaigne ground’, the midland region of developed open field systems, or what the landscape historian Oliver Rackham called ‘planned’ as against ‘ancient’ countryside.¹⁵

No scheme that divides England up into regions will ever be accepted universally. Precisely because it offered such splendid ammunition for further debate, the Roberts and Wrathmell system immediately came in for criticisms. Some were specific (are the province boundaries drawn in the right places?), others more fundamental (can nineteenth-century data really be projected so far back; is this the right way to conceive English regionality at all?). The most interesting responses are the ones that, without seeking to undermine the scheme, argue that it is only part of the story. Stephen Rippon, for instance, questions the demographic and economic centrality of the Central Province, emphasises dynamism and

¹⁰Gelling and Cole 2000; Cole 2013.

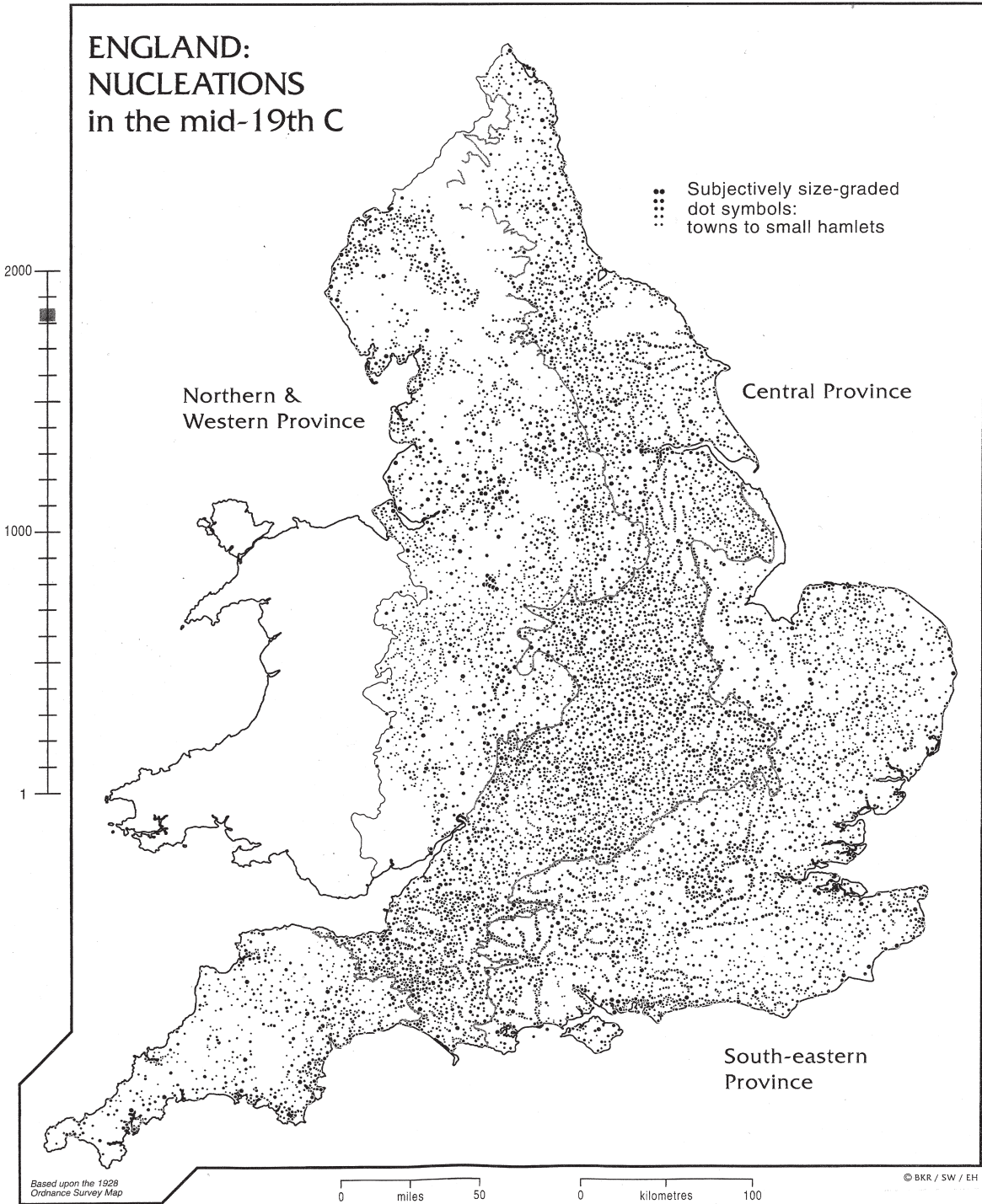
¹¹Jones and Semple 2012.

¹²Taylor C. C. 1977; Taylor C. C. 1983.

¹³Roberts B. K. 1977, 1987, 2008.

¹⁴Roberts and Wrathmell 2000, 2002.

¹⁵Rackham 1976, 16–18.



1 Nucleated village settlement and the 'central province': the evidence of early nineteenth-century maps, displaying the contrast observed much earlier, in the 1570s, by William Harrison. This map of 2002 by Roberts and Wrathmell has become a classic: the starting-point for debates on regionality in settlement and farming.

agency in regions outside it, and sees what he terms the ‘long eighth century’ as the most fundamentally transformative phase.¹⁶ It will become clear that my own analysis has much in common with these, although I prefer to shift the critical stages a few decades back into a ‘long seventh century’. Rippon also proposes ‘landscape change through emulation’, and asks whether ‘ideas of agricultural practice spread in the same way as ecclesiastical reform’¹⁷—a remarkably prescient observation, as we shall see. Alan Lambourne’s wide-ranging and often subtle investigation of ‘patterning within the historic landscape’ also emphasises human choice and agency, proposing a dynamic between environment and culture in which ‘local communities respond[ed] by making calculations as to the best strategies to adopt, given their knowledge, resources and traditions.’¹⁸

Predictably, the nucleated villages and the open-field strips and furlongs of the Midlands have had a special attraction for scholars: when, why, and by whom was something so conspicuously orderly brought into being? Their regularity, with its strong implication of top-downwards control, has encouraged the idea of a midland ‘village moment’, often placed in the tenth century: lords replanned previously scattered farmsteads and enclosed fields along integrated and systematic lines, with the result that the farmers lived side by side in compact villages and farmed their land in intermixed strips under a communally organized system that determined the sequence of crop rotations and fallow courses.

Here the most substantial work—notably in the East Midlands—is David Hall’s. His long career of research into the historical and physical details of open field systems has now culminated in a substantial monograph.¹⁹ While carefully avoiding a ‘one-model-fits-all’ approach, and stressing that villages and fields could develop at different times and through several stages to assume a regular appearance, Hall still concludes that the Central and East Midlands underwent drastic change, involving the submergence of early to mid-Anglo-Saxon settlements and their farming landscapes under the new open fields. He envisages ‘the addition of arable lands on a large scale to a core of older arable’, usually by the ploughing up of commons:

The absence of physical evidence of early ring fences may hint at a complete replanning of fields, as well as expansion. . . . Dispersed Saxon-period sites underlying some Northamptonshire fields . . . were possibly associated with greens, as seen in East Anglia and elsewhere, . . . yet none has been observed in the field patterns, and there must indeed have been a ‘great planning’ of the fields, rather than an infilling. In contrast, fields of much of the East and West were based more on pre-existing Roman sites, Saxon clearances, and small intakes that often remained unchanged into the Middle Ages in terms of general location and structure. It is clear that the irregular field types of Eastern and Western Regions are quite different to those of the Central Region. . . . The model, then, for the Central Region is for a large-scale ploughing of commons at various dates from about the eighth century until the Conquest or shortly after. The implications of such an operation are that it would tend to create a planned and expanded

¹⁶Rippon 2008.

¹⁷Rippon 2008, 255–56.

¹⁸Lambourne 2010, i.

¹⁹Hall, D. 2014. Kerridge 1992 remains valuable.

field system; . . . it would need holdings to be uniformly dispersed over the new fields; and it would require an overall communal system of farming.²⁰

Hall's two central hypotheses—a large-scale expansion from an older arable core across surrounding commons and a high degree of deliberate planning—seem to me to be both valid and convincing. However, as we shall see, a previously unsuspected factor now throws a different light on the nature of planning and on the chronology of planned settlement formation.

Within a few weeks of Hall's *The Open Fields of England*, the same publisher issued a book that could hardly be more different. Debbie Banham and Rosamond Faith's *Anglo-Saxon Farms and Farming* sets out to show that 'there was much more to Anglo-Saxon farming than open fields,' and that 'the Anglo-Saxons valued animals more than plants.'²¹ For present purposes the most important strand in their approach is its emphasis on change over time. Drawing on the fast-accumulating body of environmental data, they propose a post-Roman 'abatement' in which population and therefore cereal production dropped, followed by a process of 'cerealisation' through the seventh to tenth centuries when bread-flour production increased, the arable component in husbandry grew progressively more important, and the land was made to work harder.²² They point out that the number of places where open-field furlongs can be *proved* to have existed before the Norman Conquest is small, and that this mode 'may not even have been the most common way of rearranging arable fields'. More widespread 'must have been some version of the more flexible 'infield-outfield' arrangement. . . . The land (usually) nearest the settlement is cultivated every year, and manured regularly, while the rest is mainly used as pasture, but parts of it . . . are taken into cultivation as required, and returned to pasture when fertility declines.'²³

This perception, though so different in emphasis from Hall's, is not irreconcilable with it. Hall sets out to explain a particular kind of farming landscape, in its fully developed form and in the complex detail that we can only grasp from much later sources. Faith and Banham show that that stage—where it occurred at all—was the culmination of a long-drawn-out process through the mid- to late Anglo-Saxon period and beyond. That is far from incompatible with Hall's idea of an early arable core extended outwards by the ploughing up of commons, though it implies a chronology somewhat later than his. But in evoking a world not dominated by arable farming, Banham and Faith offer different and very convincing priorities for the location of Anglo-Saxon settlements, at least before the tenth century:

The emphasis we both place on the importance of livestock could turn some entrenched beliefs on their head. One concerns the locations of early settlements. Conventional wisdom for a long time had it that incoming Angles and Saxons (for those who believe in incoming Angles and Saxons) looked primarily for land which was easy to cultivate—hence, for instance, the frequency of early settlement on the light river gravels. . . . But can we be sure that finding easily worked arable land was in fact their main concern? . . . [M]any different kinds of land,

²⁰Hall, D. 2014, 193–95.

²¹Banham and Faith 2014, 7, 75.

²²Banham and Faith 2014, 39, 75–76, 294; some of this draws on Hamerow 2012, 147–49.

²³Banham and Faith 2014, 68–73.

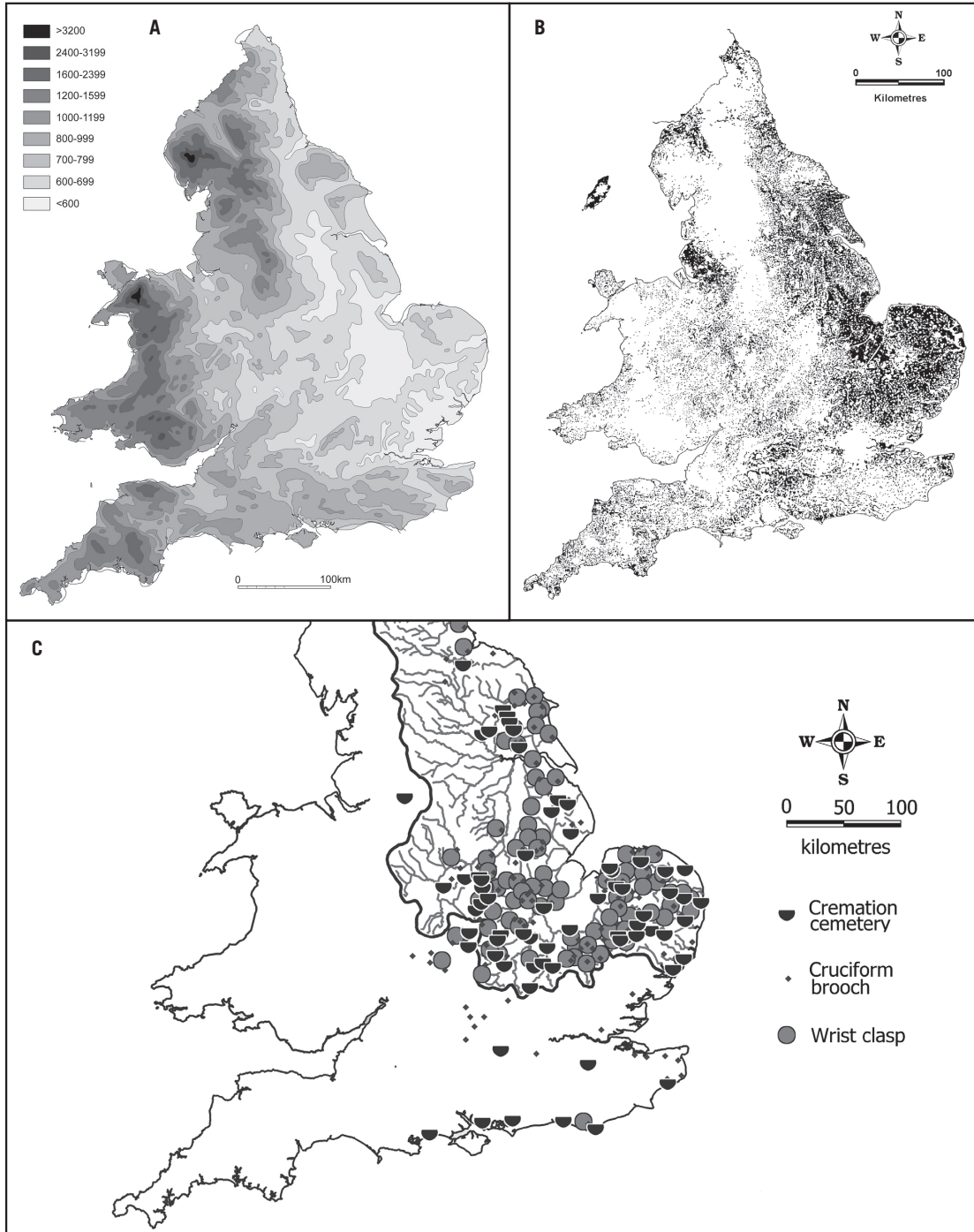
much of it what we would describe as ‘marginal’, could be, and was, cultivated in the past. If we take animal husbandry seriously, we can look at arable farming from a different perspective. Is it not just as likely that what primarily attracted settlers to river valleys were their stretches of riverside grassland where their cattle could graze and which could provide winter fodder?²⁴

But the most fundamental, iconoclastic, and thought-provoking critique of approaches to settlement forms, agriculture, and regional diversity has come from Tom Williamson, in a series of publications culminating in his *Environment, Society, and Landscape in Medieval England* (2013). His main concern is to reassert the primacy of ecology and environment as determinants of settlement and farming patterns, in reaction to work that stigmatises such approaches as ‘environmental determinism’ and prioritises human agency. Thus, for Williamson it is geology, climate, drainage, and water supply that determine social as well as agrarian diversity: watersheds defined boundaries between communities; security against harvest failure promoted the social freedom of eastern England; and the need for a rapid, intensive pooling of resources to plough the midland clay soils explains open field systems and their cooperative basis. Above all, Williamson questions the long-term reality of the contrast—apparent to William Harrison, and fundamental to the work of Roberts and Wrathmell, Hall, Rackham, and many others—between on the one hand the ‘planned’ or ‘champion’ landscapes of the Midlands, with their nucleated villages and open fields, and on the other the surrounding zones of ‘ancient’ countryside, with scattered settlement, enclosed fields, and abundant wood-pasture.

In terms of the Annales historians’ threefold division of time, Williamson’s concern is with the *longue durée*, my own with the *conjunctures*. These are complementary approaches, and in general I see no incompatibility between my emphasis on cultural phenomena and his on the ecological frame. There are places—notably in the matter of village planning—where I believe he goes too far in denying human artifice, but the areas of agreement are larger and more striking. Despite being arrived at via different data and methodologies, our perceptions that a North Sea culture zone, defined by the drainage basins of the Wash and Humber, was older and fundamentally more important than the ‘Central Province’ are essentially congruent (compare the present Fig. 2; Figs. 5 and 6). Williamson’s view of that eastern zone based on ecology, and my own based on buildings and material culture, agree in concluding that individual prosperity there was broad-based, deep-rooted, and not a result of Viking settlement. And his relatively late chronology for the cultivation of heavy clays and the expansion of open field systems—eleventh to twelfth century rather than ninth to tenth—supports both the conclusions of Faith and Banham about changing agricultural needs and my own about changing settlement forms. Given that our projects were pursued without reference to each other, this is as reassuring as it is remarkable.

Notwithstanding the richness and excellence of these historical, geographical, and onomastic approaches to the rural landscape, they have—until very recently—suffered from an almost inevitable deficiency. While they tell us much of interest and importance about the spatial relationships between settlements; their landscape context; their economic, agrarian, and tenurial basis; their relationship to geology and communications, they have very little

²⁴Banham and Faith 2014 142–43.



2 Environment and culture. These three maps, designed by the landscape historian Tom Williamson, illustrate the fundamental distinctiveness—at the heart of the present book—between east-central to eastern England and the rest. **A:** average annual rainfall, illustrating how lower levels of spring and summer precipitation make it easier to grow grain crops in more easterly areas. **B:** the distribution of arable land use in Britain, circa 1940. **C:** cremation cemeteries and selected Anglian objects in relation to the watershed boundary of Williamson's 'North Sea Province'.

to say about what settlements actually looked like. Numerous indeed are the books and articles that have in their titles such phrases as ‘early medieval settlement’ or ‘Anglo-Scandinavian rural settlement’, but they scarcely discuss any actual settlements of the period on the basis of firsthand evidence; that even applies to the excellent and path-changing volume of essays, published in 1988, entitled *Anglo-Saxon Settlements*.²⁵ The striking contrast with works on urban history and economy, which have been using firsthand excavated data for decades, emphasises how heavily the research effort in Anglo-Saxon archaeology during the second half of the twentieth century was concentrated in towns.

Of course, none of this is meant as criticism. Anglo-Saxonists of my generation—myself as much as anyone—have struggled to squeeze every drop out of the indirect evidence, because direct evidence for rural settlement after 600 has not been available. Happily, that is now changing.

THE ARCHAEOLOGY OF ANGLO-SAXON SETTLEMENTS BEFORE THE 1990s

Excavation of Anglo-Saxon settlement remains began in the 1920s, when E. T. Leeds, of the Ashmolean Museum in Oxford, realised that fifth- to seventh-century sites in the upper Thames valley were being destroyed by gravel digging.²⁶ Transformative though this observation was, Leeds introduced some misinterpretations that proved hard to dislodge (even if his perception that Anglo-Saxons lived ‘in holes in the ground’ may have had a rich and unexpected afterlife in the fiction of another Oxford Anglo-Saxonist, J.R.R. Tolkien). Perhaps more insidiously, this rather specific category of site—early, riverine, unstructured, dominated by ‘squalid’ sunken-featured buildings—set a pattern for ideas about the ‘typical’ Anglo-Saxon settlement that would endure for several decades.

After World War II, reconstruction and development posed increasingly evident threats to the below-ground Anglo-Saxon heritage. It was at that point, in response to the obviously greater urban challenges, that recognition and exploration of Anglo-Saxon towns began its rapid acceleration at the expense of interest in the countryside. From the 1960s, a much larger and wider group of people gained digging competence, and groundwork was laid for the development of pottery and artefact typologies and of a more informed intellectual framework. The emphasis on towns was heightened in the 1960s by the spectacular discovery of a major eighth- to ninth-century trading emporium at Southampton (*Hamwic*), and still more by the work of Martin Biddle and Birthe Kjølbbye-Biddle at Winchester. As well as teaching excavation standards to a whole generation, they established a research framework for understanding early medieval towns that proved hugely influential; for rural settlements, nothing remotely comparable in intellectual coherence was available then or for long afterwards.

The 1960s and ’70s were the age of rescue excavation, itinerant diggers, and volunteers. Like much else at the time, it was idealistic (saving irreplaceable data from greedy developers

²⁵Hooke 1988.

²⁶Hamerow 2012, 7–9.

became a moral duty), countercultural (the atmosphere and lifestyle on some excavations had resemblances to the Glastonbury Festival), and to an extent state supported.²⁷ There was finance for large research excavations such as rarely happen today; by contrast, some heroic rescue archaeologists had to salvage what they could, with minimal resources and in the face of massive destruction, in circumstances that would now be considered disgraceful.

Enquiry gathered pace and data accumulated, but by the early 1970s not a single Anglo-Saxon settlement had been properly published to modern standards. (One project that would eventually make a fundamental contribution was the classic study of a deserted medieval village, at Wharram Percy on the Yorkshire Wolds, but that set out from a late medieval perspective, and it would take several decades for the Anglo-Saxon origins of the village to be understood.²⁸) Then came some landmarks: the Yeavinger and Cheddar reports in the 1970s, Goltho in 1987, Facombe in 1990.²⁹ But the basis for synthesis and overview remained very exiguous throughout the 1980s, and the very fact that a few major projects had been so successful in enlarging knowledge engendered tunnel vision: ‘palaces’ and ‘burhs’ filled almost the whole frame.

Other scholars, however, were now using a methodology that was extensive rather than intensive—the collection and plotting of pottery scattered in the plough soil—to explore broader horizons. Field walking was, and is, especially productive in East Anglia, where the local Ipswich pottery and its successors leaves intact and distinctive sherds that catch the eye. More recently, the practice of ‘test pitting’—the excavation of small boxes at points on a predetermined grid—has sought to extend this kind of evidence to areas not under the plough, notably village centres.³⁰ Both field walking and test pitting yield broad-brush data for movements of settlement, which in some cases (notably the work of Peter Wade-Martins and Andrew Rogerson in Norfolk, and the Whittlewood Project in Northamptonshire and Buckinghamshire),³¹ are large enough in scale to be really useful.

Meanwhile, an entirely different resource was assuming an importance that few archaeologists could have predicted thirty years ago. The amateur hobby of metal-detecting used to be regarded by professionals as a scourge, and some aspects remain problematic, but there is now a growing body of responsible detectorists who report their finds. The objects are divorced from their stratigraphical contexts and often poorly located, but they have been logged in staggering quantities and are available on searchable databases: the Early Medieval Corpus (EMC) for coins,³² and the Portable Antiquities Scheme (PAS) for other metal objects.³³ There are some obvious dangers of distorted retrieval: metal-detected finds concentrate heavily in eastern England, but so do the huge, flat expanses of intensively farmed arable land beloved by detectorists. Nonetheless, the sheer quantity of these items is now

²⁷The flavour of excavation at that time is vividly conveyed by Rahtz 2001.

²⁸Wrathmell 2012, and below, pp. 152–53.

²⁹Hope-Taylor 1977; Rahtz 1979; Beresford 1987; Fairbrother 1990.

³⁰The ‘English CORS’ project is the subject of regular interim reports, of which the most recent is Lewis, C. 2016, giving references to earlier work, notably Lewis, C. 2010.

³¹Wade-Martins 1975; Rogerson 1996; Rogerson 2003; Jones and Page 2006.

³²<http://www.fitzmuseum.cam.ac.uk/dept/coins/emc/>.

³³<http://finds.org.uk/>.

opening avenues that were entirely closed off—indeed invisible—until recently.³⁴ That applies especially to the coinage, where the work of Michael Metcalf, Mark Blackburn, John Naylor, and Rory Naismith has transformed our knowledge, but also to the small personal items that have been studied by, among others, David Hinton and Gabor Thomas.³⁵ I am convinced that broad patterns of presence and absence delineated by these data are real, and show us important regional changes in material culture across the mid- to late Anglo-Saxon period.

Yet at a fine-grained level, pottery scatters and distributions of artefacts are not the same thing as settlements. They give a general idea of where people lived, but they are likely to be complementary to house sites rather than identical with them—especially when pottery was spread with household waste on intensively manured arable land. There is a sense in which conceiving settlements as dot-distributions of found objects risks forgetting what they are in real life: houses, gardens, roads, paddocks, and farms, embodying social culture and practice in their design and organization, and constituting the domestic world of their inhabitants. To understand Anglo-Saxon settlements properly, there can be no substitute for full-scale excavation.

GATHERING KNOWLEDGE: ACADEMIC RESEARCH, CONTRACT ARCHAEOLOGY, AND THE PRESENT PROJECT

It was from the late 1990s that substantial reassessments of rural settlement archaeology started to appear, and when they did, it was from two rather different directions. For the early period, Helena Hamerow is the current representative of the Oxford tradition going back to E. T. Leeds, but she has enriched it with a much broader Continental background. Following a study of communities in northwestern Europe published a decade previously, her *Rural Settlements and Society in Anglo-Saxon England* appeared in 2012.³⁶ Both this book and its author have contributed powerfully to the gestation of my own project. The centre of gravity of Hamerow's work is in the fifth to seventh centuries, and I have tended to leave aside the areas that I could not handle as well as she does, which is one reason why my own survey starts in 600. Readers of the two books will see some different emphases, but only one or two significant disagreements.

For the period after 850, the landmark was Andrew Reynolds's *Later Anglo-Saxon England: Life and Landscape* (1999), augmented in 2003 by an important paper on boundaries.³⁷ The author started from an interest in expressions of governance and authority such as court sites, the punishment of criminals, civil defence, and private fortification, and this emphasis was inescapably heightened by the fact that most coherently excavated sites available at that stage were of an aristocratic, military, and urban character. Over the next decade,

³⁴By far the largest and most thorough exploration of the metal-detected material is the VASLE project, the results of which are available electronically as Richards, Naylor, and Holas-Clark 2009. Their methodological discussion has informed my own work. A more recent—and excellent—exploration of the problems and possibilities of detected finds is Daubney 2016.

³⁵Hinton 2005; Thomas, G. 2001.

³⁶Hamerow 2002; Hamerow 2012.

³⁷Reynolds 1999; Reynolds 2003.

a series of important papers by Mark Gardiner extended knowledge of aristocratic and vernacular house types and building practices,³⁸ while Gabor Thomas shifted focus to the genuinely ‘ordinary’ settlements that were starting to emerge in the archaeological record.³⁹ Nonetheless, the definition of status and function remained an intractable problem, and between the work of Hamerow on the one hand and Reynolds and Gardiner on the other, there also remained the curiously invisible decades of the ninth century.

In the early twenty-first century, some major research excavations were (and are) still in progress. By targeting specific problems and questions, they are crucial for sustaining intellectual enquiry and renewal in a way that is beyond the reach of commercially funded rescue excavation. The most creative contributions are Dominic Powlesland’s vast endeavour in the Vale of Pickering, Gabor Thomas’s excavations at Bishopstone and Lyminge, and the Sedgford project in northwest Norfolk.⁴⁰ The continuing dialogue between synthesis and research-driven excavation is essential for the continued health of the subject.

But it was inevitably from a different source—and for many academic archaeologists, a less appealing one—that the great bulk of new information would now come. In the Thatcher era, archaeology changed along with everything else: the world of rescue excavation was revolutionised. Rather than amateur, altruistic, and carefree, it became commercial, competitive, and market orientated. No longer funded by the state, it became a cost of development: ‘the developer pays’ was a mantra of the 1980s. So instead of excavations being planned to suit a research agenda, the vagaries of development came to lead academic archaeology.

Was that a good thing or a bad?⁴¹ At the time, many of us worried about the fragmentation of expertise, the decline of coherent research strategies, and digging in an intellectual vacuum. All these have proved to be real problems, which (above all in specialist areas such as pottery studies) have encouraged the accumulation of countless trees with too little thought to the forest. But there is another side. First, the number of individual excavations per year has grown hugely. Second, although it remains true that the largest areas excavated are the open landscapes destroyed by mineral extraction, housing estates, roads, and reservoirs, many of the smaller interventions have been in areas previously almost untouched by the trowel, notably (for present purposes) the historic cores of villages and small towns. In this way, places and landscapes that have hitherto been archaeological blanks are starting to become visible.

The quantity of raw primary evidence recovered by contract archaeology and metal-detecting during the past three decades is vast, and daunting in its very abundance. But the practical problems go beyond embarrassment of riches: much of the excavated evidence is not even available in print. Planning consents require the production of a basic report for the ‘client’ (normally a developer), a copy of which is then supposed to be placed in a public repository, but the route to publication in a journal or monograph is much longer and highly selective. This ‘grey literature’ is available (at least in theory, though obtaining it can be laborious in practice), but accumulation of data has run far ahead of analysis. Exploitation of

³⁸See especially the synthesis in Gardiner 2011.

³⁹Thomas, G. 2012b.

⁴⁰Powlesland 1999, 2000, 2014; Thomas, G. 2010; Thomas, G. forthcoming; SHARP 2014.

⁴¹The problems and opportunities are eloquently explored by Bradley 2006.

these untapped riches was a major part of the case that I made in applying to the Leverhulme Trust for the fellowship that made this project possible, and one of my earliest endeavours on starting it.⁴²

The first step was to choose my sample of reports from the Archaeology Investigations Project (hereafter AIP) database, supported by English Heritage and hosted by Bournemouth University.⁴³ From this I selected the 'early medieval' summary listings and then weeded out nonrelevant items and sites already known through publications and ongoing projects. That left approaching a thousand entries of potential relevance, though well over half of these were unlikely to provide structural evidence and were set aside. Of the 364 remaining reports that looked worth pursuing further, some 20 percent were available online as PDFs held by the Archaeology Data Service (Oasis),⁴⁴ but for the rest I was reliant on the goodwill of the commercial digging bodies that had originally created them. Fortunately, most of these were extremely helpful, and by the end I had obtained (or at least obtained access to) some 82 percent of the reports that I needed. As this illustrates, current arrangements for archiving and disseminating unpublished data are far from perfect, but for the purposes of my project it was not an unsatisfactory result.

At this point, archaeological readers will be wondering why I did not make more use of the Historic Environment Records (HERs) maintained by local authorities. I did contact most of them and found their custodians extremely helpful in providing digitally selected data, but for purposes of academic research the HERs themselves proved to be problematic and deeply compromised. They are essentially planning tools, to enable local government officers to identify archaeological constraints on sites proposed for development. They vary widely in quality, and even in the well-maintained ones the records range from very good to very bad, so that any automatic production of distribution maps from them would be fundamentally flawed. A county-level project might be able to clean the data up, but for my one-man, national-level project that proved completely impracticable, and in many cases, it seemed to be beyond available resources to isolate the reliable, relatively modern excavations.

As the stacks of 'grey literature' reports built up and I started to trawl through them, I could really feel that I was travelling into the unknown. Digging units struggle even to meet planning requirements and have little time to contextualise their sites; it is astonishing how many unsuspected and startling implications jumped out after the simple exercise of superimposing the trenches on the first edition Ordnance Survey map. As I progressed, I realised that the new material was making a difference not just in quantity but in the fundamental range of questions that Anglo-Saxon settlement archaeology could answer: at last we were getting a quantifiable and representative sample.

But the most memorable explorations were on the open road, as I travelled the length and breadth of England, visited sites and landscapes, and interviewed local specialists. In some ways, the interviews were the most revealing exercise of all. Archaeologists in commercial

⁴²Simultaneously, Duncan Wright was using some of the same material for his parallel survey of archaeology in currently occupied settlements: Wright 2015, 5–16.

⁴³<http://csweb.bournemouth.ac.uk/aip/aipintro.htm>.

⁴⁴<http://archaeologydataservice.ac.uk/>.

digging units and local planning departments are overworked people with little time to think outside their own areas, but they are keenly interested in how their material fits into a wider picture, and they are pleased when academics take an interest. Everyone was enormously helpful, and their tolerance when I insisted on pinning them down opened up many unanswered and sometimes unasked questions.

I started this project with some scepticism about the developer-funding regime. I am now convinced that, although current practice and standards are far from perfect, the gains greatly outweigh the losses. In the world as we have it, there is no other way in which such a huge quantity of data could have been recovered from such a wide range of contexts. For the first time, they make it possible to ask and answer major questions about regional diversity, change over time, and the relationships of settlements with each other and with the landscapes around them.

Of course, not all the evidence is so inaccessible; much of it is published in journals and monographs, and some of it is very well known. An important aspect has been the reassessment of familiar sites from first principles, based on scrutiny of the detailed stratigraphical data in the reports (and occasionally in primary site records). In many cases I have proposed reinterpretations, sometimes drastic ones, and I appreciate the good humour and flexibility of excavators who have found themselves thus challenged. Given the current worrying tendency in reports to emphasise the summary and conclusions at the expense of stratigraphy (and in particular to publish plans but not sections), it deserves emphasis that I have scarcely ever accepted excavators' interpretations unreservedly; a basic principle of an excavation report must be that it provides an adequate basis for reinterrogating the site and reaching different conclusions.

In combination, the published and unpublished data are a more than adequate basis for a comprehensive new assessment. When I started, I hoped that by this point I would have a complete database, classified chronologically (as 'mid' or 'late' Anglo-Saxon) and according to site type, from which complete distribution maps could be generated. In the event, that has proved a step too far; on my own, I could not clarify and resolve all the inadequately or inconsistently reported cases in the eastern zones that have yielded large numbers of sites. Since my principle throughout has been to work with incomplete data in preference to unreliable data, I have preferred to use the cleaned-up results of the AIP listings, combined with similarly cleaned-up search results from the EMC and PAS, for my distribution maps (Figs. 5–7).⁴⁵ For regions of England outside the eastern zone, with their much sparser data, significant sites are shown individually on maps covering the date ranges 600–850 and 850–1050 (Figs. 49 and 120). Throughout the book, I make individual reference to reliably reported sites in support of my various arguments about regional variation and change over time. In one way, this is less than I hoped to achieve, but on the other hand the richness and quality of the data has far exceeded my expectations. If I have not understood every site, I hope that I have understood enough sites to view mid- to late Anglo-Saxon settlement—and its absences—in a new way.

⁴⁵In processing these various data sets, I am especially grateful for the help of Ehren Milner (AIP), Daniel Pett, John Naylor and Helen Geake (PAS), and Martin Allen (EMC).

ARCHAEOLOGY, HISTORY, ETHNOGRAPHY, AND REALITY

This book does not follow any explicit philosophical, theoretical, or disciplinary tradition. Rather, it tries at all stages to integrate the approaches of excavation, landscape archaeology, history, and place-name studies, which are often seen as discrete disciplines, and also sometimes takes perspectives from other societies in other times and places. A few methodological words may therefore be helpful.

My position on two potentially controversial points is easily stated. First, although trained as a historian (and unlike some historians), I accept the status of archaeology as a mature and intellectually coherent discipline with its own valid methodologies. Second (and unlike some archaeologists), I reject the relativist position that all our perceptions of the past are our own subjective projections; events really happened in Anglo-Saxon England, and although most of them are irrevocably lost, we can recover some of them and form hypotheses about them that are either right or wrong.

More, however, needs to be said about the interface between archaeology, history, and onomastics in early medieval studies. Prehistoric archaeologists have no written sources and must develop their interpretations autonomously; industrial archaeologists have them in abundance and seek to amplify areas where they happen to be deficient. Early medievalists are in a midway position: texts exist, and at best can be quite abundant or expressive, but they are extremely patchy and leave huge areas of life untouched. Within the scope of this book, for instance, there are usable written sources for Northumbria in the first half of the eighth century, and Wessex in the second half of the tenth, whereas Mercia has only tiny flashes of documentation, and the rich, undoubtedly complex social structure and economy of East Anglia and Lincolnshire before 950 are effectively prehistoric.

Very understandably, the last two generations of Anglo-Saxon archaeologists have rebelled against servitude to an inadequate historical narrative, and have opted to view the period through prehistorians' eyes. Up to a point, that is valid and creative as well as healthy. But sometimes, excavated evidence really does have written sources to illuminate it, and in those cases, they need to be given full weight and interpreted in accordance with historians' criteria. Those criteria have not always been respected when documents cross the archaeologist's path; some debates (notably on the ecclesiastical or secular identity of high-status sites in the eighth century, and on the rise of defended aristocratic residences in the late tenth to eleventh) have been compromised by rigid and uncritical appeal to specific texts, and to ready-made historical and anthropological models that do little either for archaeology's intellectual autonomy or for understanding of the period.

The scholar of distant periods who appeals to analogies from modern ethnography risks falling into the same trap. Recent preindustrial societies may resemble the Anglo-Saxons at a very general level and in some specific respects, but they were not the Anglo-Saxons. Such perspectives cannot be precise analogies, nor consistent keys to crack the code. What they can tell us is that certain modes of behaviour, interaction, or belief are humanly possible (and sometimes widespread), and that baffling sets of fragmentary data can occasionally be paralleled in better-understood contexts where they add up to something that makes sense.

In places, I apply this approach to problems of social custom and ritual, and to help our understanding of preindustrial economic activity, which are familiar procedures. More distinctively, I apply it to one special obstacle in exploring Anglo-Saxon settlement archaeology. As we will see many times, this was a culture whose sophisticated artisanship and careful structuring of the built environment sat remarkably lightly in the landscape. The archaeological traces are consistently flimsy, and although excavation now provides far fuller data than ever before, the data will always be less complete than for the Roman or high medieval periods, or even for parts of prehistory. Awareness of how people built transient structures in other times and places may be helpful here.

One thing of which we can be confident is that Anglo-Saxon England had much in common with the timber-building rural societies of northern, central, and eastern Europe before 1900. Those societies give us a handle on the potential range of forms in timber architecture, on the kinds of relationship we might expect between excavated footings and lost superstructures, and on the extent to which buildings, decorations, and other expressions of material culture may be entirely invisible. Here I tend to use parallels with Finland and Karelia, because I know that material at first hand. Parallels of a similar kind might have been drawn with (for instance) Romania, Hungary, or Greece, but there is something to be said for a comparison between northern societies of the east and the west: there is a sense in which Finland and eastern England are the opposite and parallel peripheries of a greater Scandinavian culture-province.

The potential contribution of place names to the themes of this book remains huge, especially given the creatively interdisciplinary nature of recent and current work. Here they are chiefly used in some specific contexts, to do with the identities, associations, and hierarchies of specialised places during the eighth and ninth centuries. In the relationship between onomastics and archaeology, it is especially crucial for specialists to respect each other's fields of expertise. Authoritative statements on the development and basic 'meaning' of place names are the province of those with appropriate linguistic and philological training.⁴⁶ On the other hand, onomastic studies must be responsive not only to geographers' insights into landscape (which is now normal), but also to archaeologists' inevitably evolving views on the nature of settlement (which is less so): the definition of place names must acknowledge the problems of defining places.

As preserved for us, Anglo-Saxon settlement names mainly refer to villages and towns, but it will be argued here that those entities were transformed during the centuries after the names were formed; even names referring both then and now to single farms tell us, in themselves, nothing precise about their physical form. So if a *wīc*, a *tūn*, or a *burh* did not look like Norwich, Wilton, or Aylesbury as we know them, what did they look like? Place names rarely map onto excavated sites in any clear-cut and unambiguous way, so that the dialogue between onomastic and archaeological perspectives on the character of places must proceed cautiously and step by step. Inevitably, some categories of 'settlement' discussed in these pages (for instance *burh-tūnas*) have the disembodied character of recurrent place-

⁴⁶Gelling 1988.

name descriptors that still lack archaeological correlates, and that may have referred to structures or installations that were not primarily habitations.

The physical landscape, the written sources, the excavated data, the ethnographic parallels, and the place names are all imperfect chains of evidence, only too ready to snap at the weakest link. When multiple strands of evidence are interwoven to form a structure robust enough to hold up when individual strands break, and to constrain speculations that stray too far outside it, we may make progress. Prehistorians have gone a long way with such methods. The societies explored in this book are not prehistoric, but farming communities on the edge of the Christian Roman world whose political organisation, customs, and beliefs are sketched out—at least in broad terms—by written sources, who left thousands of place names, and whose activities remain widely visible in the landscape. If scholars of the Neolithic can handle fragmentary data convincingly and creatively, so can early medievalists.

THE SCOPE AND THEMES OF THE PRESENT STUDY

The chronological range of this book starts around the year 600, when the English were coping with the impact of rapid sociopolitical change, conversion to Christianity, and accelerating links with neighbouring peoples and their economies. It ends around the year 1100, when they were coping with the aftereffects of the Norman Conquest, though the specifically Norman impact on the English landscape is not part of its story. This restriction is not to deny the formative importance of the two preceding (and undocumented) Anglo-Saxon centuries, but they are best explored through ‘pure archaeology’ and have been the subject of excellent recent works.

Like the initial contacts and accommodations between Britons and Anglo-Saxons, the distinct cultures of western and northern Britain fall largely outside my range. This is a book about English England, and principally about those parts of it that produce physical evidence for settlement, territorial organization, and the exercise of power. However, the West Midlands and the northwest are fully part of the story, and have constituted a more abiding interface between British and Anglo-Saxon modes of material culture than is usually appreciated. Alongside an emphasis (inescapable because of the sheer weight of evidence) on eastern England as regards settlement forms, readers will observe a preoccupation with Mercia at the height of its territorial power, especially its administrative and economic organisation. If to some extent this reflects my own interests, it may also be a useful counterweight to the many surveys of early England that have overemphasised Wessex because of its later supremacy and its good documentation.

Before 950, England was almost completely rural. Outside a group of highly specialised centres that can be counted on the fingers of one hand, the economic, military, political, cultural, and religious functions that would later make towns special were distributed between a diverse range of places that, whatever else they had, did not house dense concentrations of people. If we are to speak of ‘towns’ we should do so in the light of contemporary understandings, not of our own. This is important, because until recently the archaeological research effort has been concentrated overwhelmingly in the places conceived as Anglo-

Saxon ‘towns’ or (unhelpfully) ‘burhs’, which were viewed through the backwards-facing telescope of what they would eventually become. To preserve balance, I have taken a light touch with the huge accumulations of data from (especially) London, Southampton, Ipswich, Lincoln, York, and Winchester, though I have also tried to show how these exceptional places have their typical aspects, and evolved within spatial patterns that are very widespread. This approach will, I hope, highlight the places that really are exceptional, and also the magnitude of the tenth-century step-change.

This book has a deliberately ambiguous title. On the one hand, it shows how scholars can build a new and far richer picture of the Anglo-Saxon human landscape from the data now at their disposal. On the other hand, its focus is on how the Anglo-Saxons built their own living environment: on their houses, villages, towns, fortifications, churches, and central places. Fields and farming, both arable and pastoral, have been very effectively explored over the past decade, notably in the excellent works of Banham and Faith, Hall, Rippon, and Williamson. So have the rich and fast-accumulating data for environment, ecology, and farming practice provided by pollen cores, grain samples, and animal bones, about which little will be said here. Buildings, settlements, and specialised central places obviously cannot be discussed in isolation from their economic and landscape contexts, and will not be so discussed here, but they deserve an extended study in which they take centre stage.

Roman villas and Norman castles have a much greater impact on most people’s imagination than anything built in England between 400 and 1050, yet small-scale works of art from the period—the Sutton Hoo and Staffordshire treasures, the Lindisfarne Gospels, the Alfred Jewel—are probably better known than individual items from either the Roman or the later medieval periods. Did the Anglo-Saxons’ impact on the inhabited environment have nothing in common with the technical brilliance of their small precious objects? This book tries to answer that question.

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