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LANDSCAPES OF DEFENSE

At the Nexus of Archaeology and History in the Early Middle Ages

It is axiomatic that for some two generations, the study of the military history of pre-Crusade Europe has been one of the major academic casualties of World War Two[1]. This conscious neglect is obvious despite the uncontroversial fact that preparation for war, war itself, and its aftermath consumed an enormous part of the surplus human and material resources in most regions of the erstwhile Roman imperial West throughout the early Middle Ages, particularly in the construction and maintenance of fortifications and related infrastructural elements, including roads and bridges. However, following Western Europe's recovery from World War Two, diligent efforts by archaeologists, especially in Britain and Germany, with important contributions also from France and Italy, have resulted in a massive increase in knowledge about medieval fortifications, as demonstrated quite clearly by the volume of essays under consideration here. As a result of this vast and increasing body of research, the construction of fortifications has come to play a role in some historians' efforts to grasp the magnitude of the costs of war, broadly understood, and the sophisticated administrative organization required to undertake such projects on a large scale successfully[2].

1 This essay is the review of: John Baker, Stuart Brookes, Andrew Reynolds (ed.), Landscapes of Defence in Early Medieval Europe, Turnhout (Brepols) 2013, XVIII–383 p., 65 ill. (Studies in the Early Middle Ages, 28), ISBN 978-2-503-52956-1, EUR 100,00. The lack of proper attention to military history is made clear by Hans-Werner Goetz, Social and Military Institutions, in: Rosamond McKitterick (ed.), The New Cambridge Medieval History, vol. 2: c. 700–c. 900, Cambridge 1995, p. 451–480, esp. p. 479–480, where the subject of military organization receives very little attention. Ead., Charlemagne. The Formation of a European Identity, Cambridge 2008, in her otherwise excellent book neglects both military organization and warfare in her effort to craft a picture of Carolingian identity despite the fact that Charlemagne and his government were overwhelming focused on military matters. This neglect of military affairs is particularly evident in German scholarship. See, for example, the discussion by Hans-Hennig Kortüm, Der Krieg im Mittelalter als Gegenstand der Historischen Kulturwissenschaften: eine Annäherung, in: id. (ed.), Krieg im Mittelalter, Berlin 2001, p. 13–43.

2 See, for example, Bernard S. Bachrach, The Cost of Castle-Building: The Case of the Tower at Langeais, 992–994, in: Kathrin L. Reyerson, Faye Powe (ed.), The Medieval Castle. Romance and Reality, Dubuque, IA 1984 (Medieval Studies at Minnesota, 1), p. 46–62; and reprinted in Bernard S. Bachrach, Warfare and Military Organization in Pre-Crusade Europe, London

Of growing importance to our understanding the role of fortifications in early medieval history have been the efforts of scholars working in the new tradition of landscape archaeology, largely dominated at present by British specialists, to synthesize the findings from a broad array of fields in order to decipher patterns of human agency that are embedded in the landscape over relatively wide geographical spaces. Archaeologists working within this new investigative paradigm are in the process of helping us to understand the undertaking of what would appear to have been long-term defensive military strategy in Anglo-Saxon England[3]. In large part, this work in landscape archaeology has been developed on the basis of advances that have been made in the study of individual fortifications along with roads and road systems, as well as ancillary sites such as watch towers and fire beacons[4]. In addition, the intensive study of place names that are connected with military installations of all types as well as with roads, which would seem to have had an important if not a primarily military purpose, has been used to explain how defensive systems were developed[5]. Specialists within the new investigative paradigm argue, on the basis of their findings, that the landscape can be read in a manner similar to the ways in which historians read written texts[6]. One central and ineluctable result of this research is to highlight the role of government officials and military strategists in constructing and carrying out large scale plans, such at the burghal hidage system, which was expanded substantially by King Alfred of Wessex († 899)[7].

As a result of the work of archaeologists, including those who focus on the »landscape«, specialists in early medieval history now are required, whether or not they are yet aware of this imperative, to integrate many of these aspects of military construction into their view of early medieval government, demography, and economy. It is necessary for historians to recognize that neither natural terrain nor man made military topography can be ignored in regard to our understanding of the role of government in sophisticated military planning. In large part due to the work of archaeologists and especially landscape archaeologists, the death knell has been sounded for those historians who see the governments of Rome's successor states in the West and their administrative institutions as primitive or pre-state manifestations of a long-supposed Dark Age. We are now long past the point where any scholar familiar with the exceptionally large and growing corpus of archaeological studies dealing

2002 (Variorum Collected Studies Series, CS720), with the same pagination; and ID., The Fortification of Gaul and the Economy of the Third and Fourth Centuries, in: Journal of Late Antiquity 3.1 (2010), p. 38–64. For a study centered on the chronological period under consideration here, see ID., David S. BACHRACH, The Costs of Fortress Construction in Tenth-Century Germany: The Case of Hildagsburg, in: Viator 45.3 (2014), p. 25–58.

3 For example, John BAKER, Stuart BROOKES, Anglo-Saxon Civil Defence: Theory and Historical Context, in: ID., Beyond the Burghal Hidage. Anglo-Saxon Civil Defence in the Viking Age, Leiden 2013, p. 1–41, provide something of an introduction to the value of discussing »civil defense« and the place of »civil defense« in »state-formation theory«.

4 See Stuart BROOKES, Mapping Anglo-Saxon Civil Defence, in: BAKER, BROOKES, REYNOLDS (ed.), Landscapes of Defence (as in n. 1), p. 48.

5 Ibid., p. 49.

6 This is the gravamen of the entire collection of essays discussed in this review.

7 See the very useful collection of essays, David HILL, Alexander R. RUMBLE (ed.), The Defence of Wessex. The Burghal Hidage and Anglo-Saxon Fortifications, Manchester 1996.

with military installations can still opine about the destruction of ancient civilization and its replacement with a barbarian warrior culture modeled on epic fantasies such as Beowulf or putatively more accurate historical works such as Tacitus' »Germania«[8].

This process of integrating archaeology and history, however, will not be easy. In some parts of Europe, particularly in the British Isles and in Germany, archaeologists have been hard at work dealing with these the artifacts that demonstrated highly organized and very expensive governmental control of fortification strategy[9]. It is unfortunate, therefore, that, in general, disciplinary boundaries among academics in both countries are vigorously guarded and overall there is far too little cooperation between archaeologists and historians. In 1963, Walter Schlesinger, chair of medieval history at the University of Marburg, sought to help to bridge the divide between the fields with his investigation of the fortress at Merseburg as a model and inaugural study for the long-term project on *Deutsche Königspfalzen*. Here, Schlesinger estab-

8 In this regard, see the discussion by the leading specialist in Ottonian history, Hagen Keller, Grundlagen ottonischer Königsherrschaft, in: Karl Schmid (ed.), Reich und Kirche vor dem Investiturstreit. Beiträge beim wissenschaftlichen Kolloquium aus Anlaß des 80. Geburtstags von Gerd Tellenbach, Sigmaringen 1985, p. 17–37, who argues that the material resources of the Ottonian kings were peripheral to their real basis of power, and that their lack of institutional continuity with the Carolingian empire is most obvious with regard to military matters. Keller made these claims despite the well-known efforts by the Ottonians, based upon Carolingian precedent and institutions, to maintain systems of fortifications along their eastern frontiers. In this regard, see Walter Schlesinger, Burgen und Burgbezirke. Beobachtungen im mitteldeutschen Osten, in: Werner Emmerich (ed.), Von Land und Kultur. Beiträge zur Geschichte des mitteldeutschen Ostens. In gemeinsamer Arbeit mit Wolfgang Ebert et al., zum 70. Geburtstag Rudolf Kötzschkes, Leipzig 1937, p. 77–105; Dietrich Claude, Der Königshof Frohse, in: Blätter für deutsche Landesgeschichte 110 (1974), p. 29–42; and Berthold Schmidt, Das Westsaalegebiet im Verband des fränkischen Staates und die Ostexpansion des 9./10. Jahrhunderts, in: Zeitschrift für Archäologie 18 (1984), p. 23–32. Now also see David S. Bachrach, Restructuring the Eastern Frontier: Henry I of Germany 924–936, in: Journal of Military History 78.1 (2014), p. 9–35. With respect to efforts to build Beowulfian models into discussion of early medieval governmental operations, see Steven C. Fanning, Tacitus, »Beowulf« and the »Comitatus«, in: The Haskins Society Journal 9 (1997), p. 17–38, which deserves special attention for showing how generations of scholars wedded to a primitivist approach to the early Middle Ages have misused both Tacitus' »Germania« and the notion of the *comitatus* as a part of an effort to sustain the »warrior culture« myth.

9 The archaeological literature on early medieval fortifications is too vast to summarize here. However, readers can gain a valuable introduction to the foci and methodologies of archaeologists specializing in this topic in Joachim Henning, Alexander T. Ruttkay (ed.), Frühmittelalterlicher Burgenbau in Mittel- und Osteuropa, Bonn 1998; and Joachim Henning (ed.), Europa im 10. Jahrhundert. Archäologie einer Aufbruchszeit, Mainz 2002. Also see the important work of Matthias Hardt, Linien und Säume. Zonen und Räume an der Ostgrenze des Reiches im frühen und hohen Mittelalter, in: Walter Pohl, Helmut Reimitz (ed.), Grenze und Differenz im frühen Mittelalter, Vienna 2000, p. 39–56; Matthias Hardt, Hesse, Elbe, Saale and the Frontiers of the Carolingian Empire, in: Walter Pohl, Ian Wood, Helmut Reimitz (ed.), The Transformation of Frontiers from Late Antiquity to the Carolingians, Leiden, Boston, Cologne 2001, p. 219–232; and Matthias Hardt, The »Limes Saxoniae« as part of the Eastern Borderlands of the Frankish and Ottonian-Salian Empire, in: Florin Curta (ed.), Borders, Barriers, and Ethnogenesis. Frontiers in Late Antiquity and the Middle Ages, Turnhout 2005 (Studies in the Early Middle Ages, 12), p. 35–50, who has demonstrated with great skill the tactical and strategic efforts of both the Saxons and the Franks to utilize systems of fortifications for territorial defense.

lished an important paradigm for synthesizing both historical and archaeological sources to develop a comprehensive model of the physical structure, purpose, and use of royal palaces, many of which were also fortresses[10].

Despite these pioneering efforts, in both Germany and elsewhere it remains a commonplace that archaeologists often complain that historians are too firmly wedded to their texts, which, in any event, they correctly note are more or less biased, and, therefore, cannot be relied upon to provide accurate information. Historians, in turn, complain that archaeologists are not trained in the methods needed to explicate the written evidence, which, in fact, requires taking into account the textual problems inherent in these sources, including the *parti pris* of authors for which a sound control both of Latin and philology is required[11]. By contrast, many archaeologists would appear to believe that the artifacts speak for themselves and often seem willfully ignorant that their own interpretations of the artifacts likely are biased. Several decades ago, Phillip Grierson highlighted this point when he wrote: »it is said that the spade cannot lie, but it owes this merit in part to the fact that it cannot speak[12].«

»Landscapes of Defence in Early Medieval Europe«, edited by John Baker, a Research Fellow at the Institute for Name-Studies in the University of Nottingham, Stuart Brookes, Research Associate in the Institute of Archaeology at University College London, and Andrew Reynolds, Professor of Medieval Archaeology at the Institute of Archaeology, University College London, have brought together thirteen essays by archaeologists and historians that illustrate many aspects of the *status questionis* that exists at the nexus of early medieval archaeology and history, particularly military history. This volume is one of two studies produced with the help of grant to Baker and Brookes from the Leverhulme Trust in 2005, which was intended to deepen understanding of Anglo-Saxon warfare and military organization by treating in exceptional detail the military landscape of some parts of Britain. These scholars published an initial volume, »Beyond the Burghal Hidage«, in 2013, which has much to recommend it[13].

Of the thirteen articles published in »Landscapes of Defence«, eight treat Anglo-Saxon England. There is one that treats the Low Countries, one that deals with the German-Slavic frontier, one on Scandinavia, and two on Christian Spain. For all intents and purposes, the heartland of western Europe, or to put it another way, the Frankish *regna* and empire are ignored despite the vast numbers of studies that have been made of fortifications and defensive systems constructed both within the borders of the hexagon and to the east between the Rhine and the Weser. In addition,

10 Walter Schlesinger, Merseburg – Versuch eines Modells künftiger Pfalzbearbeitungen, in: Deutsche Königspfalzen. Beiträge zu ihrer historischen und archäologischen Erforschung, vol. 1, Göttingen 1963, p. 158–206.

11 See, for example, the discussion of this issue by Bernard S. Bachrach, Writing Latin History for a Lay Audience c. 1000: Dudo of Saint Quentin at the Norman Court, in: The Haskins Society Journal 20 (2008), p. 58–77; and id., Early Medieval Fortifications in the »West« of France: A Revised Technical Vocabulary, in: Technology and Culture 16.4 (1975), p. 531–569; and reprinted with the same pagination in: Bachrach, Warfare and Military Organization (as in n. 2).

12 Phillip Grierson, Commerce in the Dark Ages: A Critique of the Evidence, in: Transactions of the Royal Historical Society, Fifth Series, 9 (1959), p. 123–140, here p. 129.

13 For an overall positive review of Baker, Brookes, Beyond the Burghal Hidage (as in n. 3), see Bernard S. Bachrach, Journal of Military History 78 (2014), p. 1103–1106.

there is no study that looks south of the Alps toward Italy where the investigation of fortifications and systems of defense is a very well developed tradition as evidenced, for example, by the work of Ricardo Frankowich[14]. On the whole, the volume is Anglo-Saxon centered and recalls the bromide, Channel fogged in, continent isolated.

Collectively, the thrust of this work can be divided into four broad categories. First, it is argued that patterns of fortification can now be more easily detected in the early medieval landscape with the help of a wide variety of modern devices such as ground penetrating radar. When these patterns are understood in light of the natural topography of a particular area they serve as evidence for policy or strategic decisions by governments that possessed knowledge of current political situations and initiated plans to shape future behavior. Secondly, because the military landscape can be read in this way it provides *prima facie* evidence for governmental sophistication that merits these polities to be classified as »states« according to traditional anthropological definitions of a state. Thirdly, periods of military crisis provided governments with an opportunity to expand the scope of their authority and to impose both administrative oversight and economic burdens, *e. g.* taxes and corvée obligations of various types, in a more intensive manner than was possible when external threats are less exigent. These situations also enabled governments to inculcate ideological support for their actions by successfully orchestrating an effective defensive posture in times of peril. Finally, large fortifications served important governmental roles in addition to their military functions. In particular, governments utilized these strongholds as centers of administrative activities including courts, mints, and tax collection, as well as refuges when enemies invaded the region.

Most of the essays in this useful and provocative volume focus on military landscapes, various ways of identifying in detail the complexity of such landscapes, and the fortifications that comprised such landscapes. However, four essays do not fall into this category and tend to blur the overall focus on military landscape. The lead essay by Reynolds, »Archaeological Correlates for Anglo-Saxon Military Activity in Comparative« (p. 1–38), which would appear to have been intended as an introduction to the volume as a whole, focuses on the notion that Anglo-Saxon England was dominated by a warrior culture. His additional goal to »concentrate on social explanations for military activity« (p. 2), takes this essay even further from the military aspects of landscape archaeology. Reynold's bias in regard to the study of military matters from a military perspective is illuminated by his personal view that these are »one of the most undesirable features of human behavior« (p. 2).

Reynold's primary focus is not on landscape archaeology but on »burial archaeology«, which he believes provides evidence for the notion that so-called »weapons graves« are evidence that Anglo-Saxon England was dominated by a warrior elite. It is certainly clear that the decision to bury some men with swords entailed the permanent disposal of items that had substantial intrinsic value both because of the raw metal content of high quality iron, and the high-cost labor that was required to transform this iron into a weapon. Contrary to the model set out by Reynolds, however,

14 For a very useful introduction, see Ricardo Francovich, The Beginnings of Hilltop Villages in Early Medieval Tuscany, in: Jennifer R. Davis, Michael McCormick (ed.), The Long Morning of Medieval Europe. New Directions in Early Medieval Studies, Burlington 2008, p. 55–82.

such an observation does not lead ineluctably to the conclusion either that the political elite of early Anglo-Saxon was also a »warrior elite« or that the specific men buried with swords were, in fact, members of the political elite.

First, it must be recognized that in many cases only families with a certain minimum level of wealth likely were in a position to dispose permanently of a very expensive tool in a grave. The model of a warrior elite requires that only a small, indeed, elite element of society was equipped for combat with high quality weapons. But to assert that such a small elite existed on the basis of the limited number of graves that included high-quality weapons is a tautology that ignores the economic reality involved in disposing of such expensive tools. Put another way, just because we find swords in only a limited number of graves does not mean that there were only a limited number of men who possessed high quality swords of this type. One profitable avenue of comparison in this regard would be a comparison of the types of weapons found in graves with those found in other environments such as battlefields where there is a possibility for a more random selection of former weapon owners.

A second problem with the »warrior elite« model is that it presupposes that the decision about whether to bury a particular weapon lay exclusively with the heirs of the man in the grave. However, the possibility cannot be excluded on the basis of the surviving evidence that the men in the graves actually were paid »soldiers«, who served in the military households of the magnates, *e. g.* kings and princes, who in fact, ruled society. Under these conditions, the man in the grave might perhaps have been well off, but could not be considered as a member of the ruling elite. A comparative example might be adduced that some of the men who served in the mercenary forces of captains such as John Hawkwood became very wealthy, however they can hardly be considered to have dominated Florentine society[15].

The second of the essays that does not truly fit the paradigm of dealing with a landscape of defense is the study by Peter Ettel, Professor of Pre- and Early History at the Friedrich-Schiller-Universität, Jena, who has undertaken to survey the vast quantity of »Frankish and Slavic Fortifications in Germany from the Seventh to the Eleventh Centuries« (p. 261–284). As Ettel points out, more than 1,600 strongholds are known from archaeological sources for this period. There are more than 1,000 fortifications identified largely on Germany's eastern frontier and more than 600 from the western borders of the Slavic region. Most of these data, however, were collected more than twenty years ago and undoubtedly more sites have been identified in recent years. Most of these fortifications, both those located on the frontiers of the German kingdom and those identified in Slavic territory, are not mentioned in any surviving written text. Many of the fortifications that eventually are mentioned in one or another medieval text are to be found in records that were compiled a century and more after the stronghold was constructed as dated by archaeological evidence, *e. g.* dendrochronology.

Ettel points out that traditionally archaeological research was focused on dating the initial and subsequent development of fortification and on creating typologies. These data undergird the view that following the so-called »Migration Period«

15 On this topic, see William Caferro, John Hawkwood, An English Mercenary in Fourteenth-Century Italy, Baltimore, MD 2006.

(*Völkerwanderungszeit*) of the 4th and 5th century, there was a hiatus in the construction of fortifications during the first century and a half of the Merovingian era, i. e. before the fragmentation of royal power and the emergence of the *rois fainéants*. From the mid-7th century onward there was a period of continuous fortress construction up through the end of the 10th century. Ettel identifies three types of Frankish fortification built during the period, which are thought to have developed progressively from largely wood and earth constructions to the increasing use of stone. This pattern would seem to be evidence on the one hand for the increasing wealth of those who were responsible for the construction of these strongholds while on the other hand evidence also for an increasing capacity, *e. g.* the development of siege technology, by active or perceived enemies to threaten less well constructed fortifications.

In addition to the traditional aspects of German archaeology, Ettel calls attention to an increased interest in life within the fortifications and their environments. As an excellent example of the high levels of information archaeological research can obtain, Ettel draws close attention to the fortification complex at Karlburg on the river Main where he, himself, made significant contributions. The fortifications at Karlburg likely were begun under the auspices of King Pippin I († 768) and saw continuous development for centuries. Ettel sees Karlburg as a »large early medieval village« which can be compared with early urban sites. Ettel's terminology is not transparent and, therefore, it is important that he describes major aspects of the site. The heart of the complex was the stronghold that was expanded with an increased use of stone to a width of 9–10 meters. The height of the walls now cannot be recovered, but the entire complex was surrounded by a ditch. In addition, there was a second stronghold on the other side of the river called the Grainberg. There was also a monastery, a harbor, and ship landing indicative both of trade and also for the transport of troops along the river and of logistical support. There is evidence for dwellings, stables, granaries, some fifty »pit houses« illustrating iron working and weaving, and finds indicating trade in high quality goods. The defenses ultimately enclosed an area along the river a kilometer in length and 200 meters in width.

Whether modern scholars should consider this complex a large village, a small town, or perhaps even an emporium for the buying, selling, and transport of agricultural products westward and manufactured goods eastward certainly raises questions regarding classification. Demographic estimates are desideratum for both the population of the enclosed area as well as the regions close by that supplied the population with food, as only occasional gardens are to be found within in the walls and there was no space for the grazing of herds for food. Some contemporary texts of the later 8th century refer to Karlburg as an *urbs*, and this provides food for additional thought[16]. Indeed, the classification of other fortifications along the frontiers of the German kingdom with regard to economic and demographic criteria remains to be ascertained.

Ettel devotes a total of four pages of discussion to Slavic fortifications. The main point to be taken away from this part of the essay is Ettel's support for the contemporary Germanist view that traditionally Slavic fortifications have been dated two or

16 See the discussion by Bernard S. Bachrach, Charlemagne's Early Campaigns (768–777). A Diplomatic and Military Analysis, Leiden 2013, p. 563–564.

more centuries too early. The basis for this late dating is not made clear by Ettel and he does not explain the weakness in the methods of Slavic archaeologists. In fact, not a single work in a Slavic language is cited in Ettel's bibliography[17]. Without a detailed examination of the Germanist and Slavicist positions, one wonders whether traditional German views of Slavs as comparatively primitive are at work or whether traditional Slav views that they were at least the equals of their German neighbors are operative. One is reminded here of long-held views by the English of the inferiority of their Celtic neighbors and the efforts of the latter to counter those views.

When, according Ettel, the Slavs begin large-scale construction of fortifications throughout the western parts of the region that they dominated, their efforts seem to be very similar to those undertaken by their German neighbors. In the way Ettel presents this information, the reader seemingly is encouraged to believe that the Slavs learned a great deal from the Germans, which, in fact, may be an accurate appreciation of Slavic-German interactions in regard to construction technology and techniques. In this context, it is to be noted that the literate Germans learned much from Roman books, such as the works of Vitruvius. In fact, parts of Euclid's geometry were translated into German, while the Slavs, in general, lacked access to such texts. As with his treatment of German fortifications, Ettel describes in considerable detail several large and well excavated Slavic strongholds, which are very similar to large German fortifications with administrative functions, churches, manufacturing infrastructure, and trade. They obviously played host to populations of a considerable size. However, no demographic estimates are ventured.

Ettel draws a series of conclusions from his tour of the horizon. First, and most importantly, he recognizes that these fortifications were built for military purposes, although he does not venture to review the scholarship that explains how this was done. He emphasizes, particularly in regard to large strongholds, that they played a role as places of refuge during enemy raids, but importantly makes clear that there also were fortifications that were inhabited with permanent populations and did not remain empty awaiting a military crisis. Ettel also emphasizes that fortifications had a »sacral« function by which he means that churches often were built within the walls. However, »sacral function« does not seem to be the correct language. Fortifications also had economic functions, *e. g.* manufacturing and trade, administrative functions and as »centres of […] lordship« which remains unclear. He also notes that some strongholds »were seats of sovereigns, aristocracy, and clergy«. A more reasonable phrasing would seem to be that kings, bishops and lay magnates sought the safety of fortifications for their homes.

While Ettel has provided a very useful introduction to German and Slavic fortifications in the course of fewer than thirteen pages of text, there is nothing to be found in regard to landscapes of defense, despite the fact that much has been done by German scholars in this vein. One need only review, for example, the extensive literature dealing with the *limes Saxonicus*, the Germar Mark, and the Sorbian march, the latter two of which certainly were comparable to the Burghal Hidage, in order to gain a proper appreciation of Carolingian use of fortifications for strategic and tactical pur-

17 Cf. P. M. Barfort, The Early Slavs. Culture and Society in Early Medieval Eastern Europe, Ithaca, NY 2001, p. 144–146.

poses[18]. In addition, the strategy undergirding the frontier fortifications constructed by Henry I († 936) and his son Otto the Great († 973) make clear that the rulers of the German kingdom were at least as attentive to landscapes of defense and, indeed, of offense as their Anglo-Saxon contemporaries[19]. In short, otherwise uninformed readers of Ettel's essay will come away believing that rulers such as Alfred the Great were more attuned to the role of fortifications in military strategy than, for example, were Charlemagne († 814) and Otto the Great. This would be a highly misleading conclusion.

The majority of the studies in this volume, including those by Ettel and Reynolds, treat numerous sites and, as will be seen below, these often include multiple fortifications. By contrast, Neil Christie with Oliver Creighton and Matt Edgeworth, who are connected to the University of Leister, the University of Exeter, and the University of Leister, respectively, devote their attention to the single, albeit very important, fortress at Wallingford (p. 111–128). Their goal is to examine whether Wallingford »was primarily a frontier foundation or a burh with planned specific urban and economic functions from the outset«. A second goal of this study is to ascertain the ways in which Wallingford was able to »endure the transition to Norman rule«. The highly nuanced and exceptionally well documented study by Christie *et al.* suggest that the questions posed above are rather tendentious and should be answered in a manner that takes into account the chronology of particular situations and the availability of information that is uncontroversial. With regard to the second question, for example, there is some reason to suggest that in the wake of Duke William's conquest, the position of Wallingford may have declined precisely because Norman strategy was not the same as that of some Anglo-Saxon rulers. However, Wallingford again assumed a very important role during the civil war between Stephen and Matilda as their strategic imperatives once again were different from those of the earlier Norman rulers of England.

In the context of understanding the changing role of the fortification at Wallingford, it is important to consider that it was a major element in Alfred's Burghal Hidage, which controlled a key crossing of the Thames west-northwest of London and required garrison defense forces of the same order of magnitude as the late Roman walls of the fortress city of Winchester. Nevertheless, neither the »Anglo-Saxon Chronicle« nor Asser's »Life of Alfred« mentions the fortress. The failure of either text to discuss Wallingford is even more noteworthy in light of the fact that Orosius' »Universal History«, which claims that Julius Caesar fought a battle at Wallingford, was translated at Alfred's court. Thus, the failure to mention Wallingford may be taken as illuminating the lacunose nature of both texts, and suggests that relying on these works alone for information about the extent of the burghal system in Wessex is likely to lead to errors.

Although not treating a »landscape« the cooperative effort by Christie, Creighton, and Edgeworth has produced an important study with regard to the state of the

18 See, for example, the thought-provoking works of Hardt, Hesse, Elbe, Saale (as in n. 9), p. 219–232; and id., The »Limes Saxoniae« as part of the Eastern Borderlands (as in n. 9), p. 35–50.

19 See, for example, Bachrach, Restructuring the Eastern Frontier (as in n. 8), p. 9–35.

question concerning Wallingford, and just as importantly a model for additional studies of other elements in the Burghal Hidage as well as longitudinal views of the areas involved both before and after Alfred, which are much needed. In all, these data will help military historians to develop a deeper understanding of the strategy of fortress building over time as differing conditions led to creativity among military planners. One additional point touched upon by Christie *et al.* is the Roman background of the Anglo-Saxon landscape and what decision makers such as Alfred and those educated men at the court knew both from written and topographical sources about various aspects of the past. It is to be hoped that scholars following the model set out in the essay on Wallingford with delve more deeply into this particular issue.

The fourth of the studies to eschew a specific focus on landscapes is the essay by Juan Antonio Quirós Castillo, Professor of Archaeology at Euskal Herriko University, that offers »a brief summary of recent archaeological studies of defensive sites of the early Middle Ages (5th to 10th centuries) in the north-western part of the Iberian Peninsula« (p. 303–340). This survey, however, is not focused on a discussion of »landscapes of defense« but its aim is »to discuss in social terms the role that these structures played in the formation of medieval societies« (p. 303). Quirós Castillo does not approach this topic by asking how the people who decided that these fortifications were to be built viewed their efforts. Instead, he suggests that the expenditure of extensive human and material resources to build, maintain, and garrison strongholds was intended for largely non-military purposes.

Quirós Castillo begins with an all too brief and, therefore, unsatisfactory survey of various »theories« put forth, largely by French and Italian scholars, regarding what is thought to be the social history of fortifications. A more detailed examination of these ideas might well have been very useful to the attentive reader. Absent from this limited tour of the horizon are the works of German archaeologists and even of Anglo-Saxonists, who have put forth volumes of ideas in this area. What has most impressed us regarding Quirós Castillo's introductory effort is his attachment to the »feudalism« construct. This suggests to us that Quirós Castillo either is ignorant of Susan Reynolds' magisterial and very widely accepted deconstruction of »feudalism« in her 1994 monograph »Fiefs and Vassals«, or that he simply has chosen to ignore this paradigm-breaking work so as to support an argument that relies on an approach that lacks even heuristic value. If one is to ignore Reynolds, it is necessary to defend one's position in detail and at length.

Quirós Castillo's intention is to »explore the active social role played by castle and fortress in the early Middle Ages by means of an integral analysis of territory with a comparative analysis at the regional level« (p. 308). To this end Quirós Castillo divides early medieval fortification construction into three periods. Initially, he focuses on structures built or reused during the period of the dissolution of Roman imperial power in the northwestern part of the Iberian Peninsula. These he calls »first generation castles«, which are dated to the period from the 5th to the 7th century. The »second generation castles« are dated to the 8th and 9th centuries and the »third generation castles« are dated from the 10th to the 11th century. On the whole, there is rather little archaeological work available to draw meaningful conclusions regarding the social role of these fortifications. In fact, the location of many strongholds mentioned in written sources has yet to be identified much less excavated. Thus, Quirós

Castillo finds solace in various theories for which there is a dearth of evidence in Northwest Spain.

In general, with regard to the first generation of strongholds some types are marked by »the poor quality of the available archaeological record« (p. 309), and the immense diversity of examples within this chronological period seem to defy characterization. As a result, Quirós Castillo's generalizations seem to be less than compelling. One point, however, worth noting regarding Quirós Castillo's discussion of »urban planning«, is that these castles frequently have a cemetery beyond their walls« (p. 315). It is regrettable, in this context, that Quirós Castillo does not provide a valid definition of what constitutes an urban site. In addition, given the early date of these fortifications, it is surprising that Quirós Castillo does not raise the question of the survival of the Roman tradition of the *pomerium*. Overall, Quirós Castillo grasps at straws to try to explain various phenomena that are very poorly documented in the archaeological record in a period that is largely bereft of written sources. For example, he presents an argument regarding what he believes to have been a pattern of behavior indicating that there are greater numbers of »castles« in those areas where the collapse of urban networks was most pronounced. To support the complex hypothesis for which there is no evidence, he calls attention to what seems to be a well documented fact that there were »urban abandonments« in the Duero Basin a millennium later. Trying to explain events much less policies by conditions separated by a thousand years cannot be tolerated, at least by specialists in medieval European military history.

As noted above, Quirós Castillo finds there to be a dearth of archaeological evidence in key areas during the first generation. Similarly, concerning the second generation, he observes that there are also very few archaeological studies of fortifications from the 8th and 9th centuries. This problem, for which the author provides no compelling cause, raises question regarding the validity of his three generation model. This problem is further exacerbated by the fact that those fortification that are dated to the 8th and 9th centuries by archaeological evidence often are to be found treated in the written sources only from the late 9th and 10th centuries.

The major point to be emphasized regarding this »second generation« is its orientation toward stopping or slowing down Muslim advances both in terms of territorial conquest and raiding. This defensive mode oriented toward Muslim aggression is very well exemplified by *clusae* of considerable size and sophistication that were constructed to control mountain passes as well as the reutilization and reorientation of older fortifications. While Quirós Castillo likely is correct in seeing these efforts in terms of what he identifies as state development, he simply ignores the fact that the main purpose of these efforts was to defend against the Muslims. Whether it is politically incorrect to see the Muslims as enemies even at a remove of a millennium and more is a question best left to Spanish academics. However, in a more serious vein, this »second generation« in Quirós Castillo's model provides exceptionally fruitful opportunities to compare the defensive methods undertaken in the northwest of Christian Spain with the efforts of the Mercian kingdom and then of Wessex to defend against the Vikings. Indeed, even more fruitful comparisons might be drawn with other regions of Iberia as will be seen below.

As throughout, Quirós Castillo consistently confuses the English usage »early

Middle Ages« and refers to the period as the »late Middle Ages«. However, it is to be emphasized that the third generation of fortifications does not concern the late Middle Ages. Here Quirós Castillo focuses his findings in the »Basque Country«, where some »forty castles« in the »[sic] late Middle Ages«, *i. e.* the 10th and 11th centuries, have been identified (p. 328). Most of the fortifications that have been excavated or studied in recent years »are located high up on steep hilltops from which they dominate large territories« (p. 329). Exactly how this domination took place is not explained as both hand held missile weapons and catapults of various types had limited range. Such strongholds, if properly sited, could control a road that passed by the fortifications. However, after characterizing the strongholds as dominating large territories, Quirós Castillo concludes that they were »›symbol‹ castles« and lacking any offensive function. Rather he believes that they »serve to mark territory« (p. 329). Quirós Castillo does not discuss the likelihood that these strongholds were constructed to hold territory in order to halt Muslim territorial conquests and as magazines to provide government forces with logistic support when moving against enemy assets.

On the whole, Quirós Castillo exposes his readers to large quantities of information, which if properly digested and related to major research questions and/or to the matter of landscapes of defense could be of exceptional value to historians. Two major areas of research clearly could be meaningfully impacted. In the discussion of the first generation of fortifications, the examination of later Roman survivals into and through the Visigothic period is of considerable importance. In regard to fortifications of the second and third generations, an important focus should be on the question of developing landscapes of defense in Christian territory against Muslim aggression. Special attention here to place names, roads, and signal stations are of primary importance. Finally, comparisons between Anglo-Saxon defensive activities against the pagan Vikings and those taken by Christians against Muslim aggression could be very useful.

Of the nine essays in this volume that focus specifically on landscapes, three are written by historians. Barbara Yorke, Professor Emerita of Medieval History at the University of Winchester, provides observations on »West Saxon Fortifications in the Ninth Century: The Perspective from the Written Sources« (p. 91–110). Richard Abels, Professor of History in the United State Naval Academy, focuses on »The Costs and Consequences of Anglo-Saxon Civil Defence, 878–1066« (p. 195–222). The essay by Julio Escalona, Senior Researcher at the Instituto de Historia, Centro de Ciencias Humanas y Sociales – CSIC Madrid, is intended to examine the role of fortifications on the political frontier marked by the river Duero as means of treating Christian advances against the Muslims (p. 341–368). All three studies would seem to break new ground insofar as they work at a multi-disciplinary level to integrate history and archaeology.

It is Yorke's aim to demonstrate that references to fortifications in narrative sources for the 9th century enable us to expand the number of known defensive sites in use in Middle Saxon Wessex« (p. 104). She initiates an effort to compare the terminology used in Anglo-Saxon sources for fortifications with those used regarding the same strongholds in Latin sources and also calls attention to aspects of *imitatio imperii*, *i. e.* copying the way in which the Romans behaved, as an important element in the

construction of new fortifications. However, when discussing marching camps used by both Anglo-Saxons and Viking armies, she neglects to call attention to the working of imperial influence. Here, attention is due to the broad reading of Roman history at the court of Alfred the Great († 899), as well as the fact that many Norse adventurers in English service had previous military experience under Byzantine emperors. While Yorke's innovative approach in regard to Anglo-Saxon studies is to be applauded, it must be remarked that interdisciplinary efforts regarding the continent, which deploy terminology, archaeological remains, and accounts of military operations in the course of sieges to expand our understanding of fortifications have been a commonplace for a long time[20].

Abels, one of the leading specialists in Anglo-Saxon military history and especially Alfred the Great argues that »the development of a complex and costly civil defense system in late 9th century Wessex played a critical role in the state formation of late Anglo-Saxon England« (p. 217). Whether Alfred's Wessex can be considered a »state« depends, at least in part, on the definition of »state« one chooses to employ. Abels, unlike some of his colleagues in this volume, does not choose to enter the lists on this question. In addition, it is controversial, at the least, whether the term »civil defense« is so anachronistic that even its heuristic use may be challenged. In much less controversial terms, it is clear that the government of Alfred the Great saw increasing sophistication and centralization during the process of developing the military resources of Wessex for the purpose of combating the Vikings. Moreover, the concomitant development of military and institutional sophistication cannot be isolated to periods of impending danger. While Alfred developed the Burghal Hidage for defensive purposes, his heirs used these relevant fortifications as bases to press offensive operations.

Abels' thesis, *i. e.* the development of military assets played an important role in the growing sophistication of governmental institutions, in itself, is hardly novel, although throughout his essay he adds additional evidence to an already solid case for King Alfred and his successors. Abels' recognition that Alfred's major military innovation was the development of a system of defense that was constituted strategically as a defense in depth also is not new but no less accurate for that. What is refreshing is Abels' willingness to focus on military decision-making that looked toward a long-term strategy on a large scale, and his further observation that these projects were exceptionally expensive in terms of the investment of human and material resources. The examination of aspects of the economic underpinnings of governmentally directed military building is somewhat of a departure for Abels. Thus, he begins by accepting the »revisionist« view of Offa's Dyke and recognizing that it was, in fact, a military project. However, when he enters the discussion regarding its costs, which is a subject of great importance for understanding the availability of surplus

20 See, for example, ID., Early Medieval Fortifications in the »West« of France (as in n. 11), p. 531–569; and ID., Fortifications and Military Tactics, in: Technology and Culture 20 (1979), p. 531–549. Both are reprinted with the same pagination in ID., Warfare and Military Organization (as in n. 2); now also see ID., Restructuring the Eastern Frontier (as in n. 8), p. 9–35.

resources for military projects, Abels' estimates regarding these matters represent only about one-third of the actual costs (p. 198–199, esp. n. 2)[21].

Abels' too low levels for the cost of building Offa's Dyke, diminishes our understanding of the great expense of building such monumental military projects. However, despite the relatively low level of resources that he associates with the construction of this landscape of defense, Abels' recognizes that »the sheer scale of construction implied by the Burghal Hidage is enormous« (p. 201). This is an important step in the right direction as scale obviously evokes matters of costs. However, as with Offa's Dyke, Abels' citation of cost estimates, based largely on claims, adduced by English scholars, such as Halsam, are too low as they are intended to prove that the construction of the entire system could have been completed in about fifteen months. Abels also takes note of the costs involved in maintaining the fortifications and garrisoning them. Once some sort of estimate regarding the costs of systems such as the Burghal Hidage has been established, it will be necessary to use these data for the purposes of trying to make sense of the Wessex economy, *i. e.* the goods and services produced in Alfred's kingdom during the later 9th century.

The final essay by a historian, which is also the final essay in the volume, is Julio Escalona's »Military Stress, Central Power, and Local Response in the County of Castile in the Tenth Century«. Escalona sees a multifaceted process of combined central government and local efforts undertaken in a period of »intense social change, largely triggered by military stress and incorporation in a large-scale, and more complicated system« (p. 342). However, as will be seen below, the documentation presented by Escolona does not permit a clear demarcation of the orchestration by governmental authorities of what may seem to be a fortress building strategy. One way to treat this problem is to ascertain how and under what circumstances resources were laid under contribution to effect a particular strategy.

However, before Escalona focuses on his main goals, he provides a tour of the history of fortifications in this area, which shows that very little is known of the region often referred to as »Old Castile« from the dissolution of imperial authority until sometime in the later 8th century or, perhaps, somewhat earlier. By ca. 850, the original name of the region, which was »Bardulias« came to be called »Castella« in Latin and »al-Qila« in Arabic (p. 344). This phenomenon of castle building, recognized by both Christians and Muslims, already was well developed, likely for a generation or more, when the name change became so firmly established as to be recorded in written documents, which by their nature are exceptionally conservative in their usage. Since the kingdom of Asturias of which Bardulias was a distant frontier ca. 750 cannot be shown to have much administrative reach southward toward the Duero even in the later 8th century, it should be concluded that the castle building phenomenon was spearheaded by local aristocrats intent upon establishing the defense of Christian territory against Muslim aggression.

From an archaeological perspective, Ascalona does not explain whether the making of »old Castile« was done on the basis of already existing strongholds that were reused or through largely new construction. It is also unclear whether this newly baptized »land of castles«, was thickly covered with stone strongholds or fortifica-

21 See the studies cited in note 2.

tions constructed of timber and/or earth. Whatever the case, the reader needs to be given some sense both of the costs of castle construction or refurbishment and also of the demography of the region so as to ascertain the economic and social conditions that made available the surplus human and material resources that were required to build these strongholds. Without a better sense of the material realities, Ascalona's hypotheses regarding competition between local elites and elites connected to the royal government must remain little more than interesting possibilities. Or to put it another way, Ascalona's claim that local elites likely were »farmers« themselves »even if they had access to more lands and extra workforce by means of their control of king relations and community leadership«, remains only theoretical (p. 348).

The question of the strategy undergirding the castle-driven defense of the Duero frontier in the late 9th and early 10th centuries, like most scholarly matters, is divided between those who see the direction of the central government as of primary importance and those who focus on local initiative to the detriment of royal initiative. However, at this time the formulation of hypotheses seems fruitless, because as Ascalona bitterly recognizes, scholars, and here one might read government support, are focused on pre-historic sites and on the archaeology of Muslim fortifications. Ascalona's efforts to bring further attention to watch towers and signaling systems also are undermined by a lack of archaeological evidence as are his efforts to look into the matter of fortifications constructed as refuges in the face of Muslim raids. By contrast, studies treating place names are of value to understanding military matters and seemed to have fared very well by comparison with more costly archaeological efforts.

Unfortunately, Ascalona's treatment of military organization still suffers from the voluminous efforts by Claudio Sánchez-Albornoz to place Spanish military institutions within the now discredited »Germanic Freeman« model, which supposedly developed over time into the equally discredited European-wide feudal model. In addition, Ascalona's reference to Guy Halsall's primitivist non-discussion of the Duero frontier is rather more misleading than useful (p. 356). Reading of James F. Powers magisterial »A Society Organized for War. The Iberian Municipal Militias in the Central Middle Ages, 1000–1284« remains exceptionally useful for the background material that it provides.

Of the remaining six essays in the volume, the most important from the perspective of the study of military history is that by Stuart Brookes, »Mapping Anglo-Saxon Civil Defence« (p. 39–64). Brookes begins with the long-held observation by military planners that »fieldcraft«, as this term is used by specialists in military science, makes clear that »elements of the landscape interrelate« and as a result landscapes can be read as »palimpsests of past action« (p. 39). Brookes' aim is to discuss »militarism«, which he defines as »geographies of military preparedness«, in terms of »continued preparations which states make for war and the geographical impacts such measures have« (p. 41). Brookes' focus throughout the essay is on the mapping of various aspects of militarism in southern England during the Viking Age.

Brookes' initial concern is with borders and their defense by »states«, a term that he believes can be used without reservation to denote Anglo-Saxon polities. Brookes further enters into controversy by adopting or at least adapting some of the ideas of Edward Luttwak, whose effort to apply the idea of »grand strategy« to the defense

of the Roman Empire was widely condemned by British specialists in ancient history, although often accepted by scholars elsewhere. Brookes, like Luttwak, does not provide an epistemologically valid definition for »Grand Strategy«. Brookes consequently is in the ironic position of setting out a conceptual framework for examining the Viking Age that many if not most British specialists in Roman history have vigorously rejected as beyond the capabilities of the empire's intellectual, planning, and material resources. Of course, Rome commanded a wide variety of assets that were far superior to those available to Alfred the Great and his fellow Anglo-Saxons.

This being the case, it is to be noted that Brookes has sufficient information available from a variety of sources without adducing vague and undefined concepts such as »Grand Strategy« or the work of Luttwak[22]. Brookes has available substantial bodies of information regarding important aspects of »militarism« in regard to assets such as fortifications, signal systems, place names and roads in relation to the likely movements of men and information, and these can be shown to articulate effectively in regard to the topography. Brookes alludes to matters of logistics, which are important both in constructing infrastructural components, see Abels above, and also for the supply of both troops on the move and garrison troops who were deployed to defend various fortifications. In order to add logistic support to the *armentarium* for the support of Brookes' compelling arguments it will be necessary to examine the written sources, particularly »Domesday Book«, from which it will be possible to identify estates that were positioned to provide logistic support.

John Baker, one of the co-editors of this volume, has contributed »The Language of Anglo-Saxon Defence« (p. 65–90), which is focused on place-names that have military and military-related importance. This work supplements part of Brookes' efforts to integrate fortifications, signal systems, and roads in relation to the likely movements of men and information, as demonstrated both in regard to natural and man-made topography. The large number of illuminating place names for military roads, *e. g.* »here-paed«, »fyrd-road« and »fyrd-straet«, are of great importance in understanding connections between fortifications and troop movements, which also have implications for logistics. However, Baker makes clear that terms such as »burh« do not always have the same meaning of fortification over time or at least not that primary meaning as the function of places can change. Also it is important that fortifications are not always called »burhs«. These nuances are not unimportant as particular places that at one time likely were constructed for military purposes over time can be seen to cease to play that role at a later date. Like Brookes, Baker sees the interrelated aspects of forts, roads, and signal stations as evidence for governmental planning.

Gareth Williams, Curator of Early Medieval Coinage in the British Museum, aims to explain the multivalent meaning of the term »burh« in a useful but sometimes misleading essay entitled »Military and Non-Military Functions of the Anglo-Saxon Burh, c. 878–978« (p. 129–164). Williams begins on the wrong foot by following the

22 This model has been defended successfully by Everett Wheeler, The Methodological Limits and the Mirage of Roman Strategy, in: Journal of Military History 57 (1993), p. 7–41, 215–240; and Kimberly Kagan, Redefining Roman Grand Strategy, in: Journal of Military History 70 (2006), p. 333–362, although these authors have significant disagreements.

notion, espoused by Abels and others, that Anglo-Saxon military organization was based upon »personal lordship« and that this view has been »expanded to early medieval warfare in general« (p. 133). Here Tacitus' early 2nd century »Germania«, largely cleansed of his views regarding the centrality of kingship, and literary fantasies such as Beowulf traditionally have served as the framework for this argument. Thus, Hollister's accurate assessment of Anglo-Saxon military organization, which owes much to Frankish influences, *e. g.* Charlemagne's military organization, is ignored. Williams and other defenders of the lordship argument raise the red herring of a dichotomy between lordship and an archaic military organization, often based on notions of *Gemeinfreiheit*, that it was the obligation of the so-called *Volk* as a whole to bear arms[23].

The late antique military system in Gaul that influenced the Anglo-Saxons was tripartite in structure. For purposes of local defense, all able-bodied men, even including slaves, were mustered to defend their home territory regardless of military training. This was done on the basis of later Roman legal enactments dating at least to the early 5th century if not even earlier. Also following imperial legislation, locals, whom the government determined possessed sufficient means, were mobilized personally or required to provide one or more substitutes on the basis of their wealth in order to undertake offensive military operations outside their home territory. Magnates, who owned large expanses of land, were obligated to attend the muster and potentially bring with them very large numbers of men. Finally, the king and his magnates, both lay and ecclesiastical, supported military households, *obsequia*, which were composed of professional soldiers. These men, who served in a magnate household, were mobilized for war as part of their employers' obligation to produce high quality fighting when called upon to do so by the king or relevant prince. In short, neither ill-defined notions of »lordship« nor so-called *Volk* obligations determined the basis for military service. Rather, wealth was the basis for assessing military obligations[24].

In addition to the tripartite Frankish system that the English adopted, the *trimodae necessitates*, which the Anglo-Saxons also adopted, are a well known example of borrowing from later Roman legal enactments. Very similar obligations were geared to the support of military and military related operations in the *regnum Francorum*. Indeed, Williams argues that burhs were modeled on Frankish *civitates* as the focus of both royal military authority and administration (p. 145) and also accepts the view that the Danish fortress system was also constructed under Frankish influence. In this context, it should be noted that large fortifications served this same function in the Ottonian kingdom during the 10th century[25].

In addition to the copying of Frankish military institutions, it bears emphasis that *imitatio imperii* was well developed in England with regard to the design of some

23 See C. Warren Hollister, Anglo-Saxon Military Institutions on the Eve of the Norman Conquest, Oxford 1962; and id., The Military Organization of Norman England, Oxford 1965. For later Anglo-Saxon England as a Carolingian-type state, see James Campbell, Essays in Anglo-Saxon History, London, Ronceverte, WV 1986.

24 This system is described in detail in Bernard S. Bachrach, Early Carolingian Warfare. Prelude to Empire, Philadelphia, PA 2001 (Middle Ages Series), p. 51–83.

25 Werner Emmerich, Landesburgen in ottonischer Zeit, in: Archiv für Geschichte von Oberfranken 37 (1957), p. 50–97.

new strongholds and the refurbishment and use of others such as the fortress city of Winchester, which played an important role in the Burghal Hidage. In fact, Williams sees the »Germanic« north as much influenced by Rome. Williams, himself, recognizes Roman influence as he claims that Alfred the Great was influenced directly by his experience of the fortifications at Rome (p. 149).

Once Williams gets beyond matters of early medieval military organization, he goes on to examine various of the functions that would appear to have occurred in more than a few royal burhs. Given his position at the British Museum, it is hardly surprising that he gives most attention to mints and coinage. He also notes that some burhs likely had the capacity to serve as refuges for habitants in the neighborhood. However, in his discussion of the Burghal Hidage, Williams observes that »the ability of the civilian population to outdistance pursuit by Viking forces« required that »civilians of all ages and in all states of health could travel twenty miles […] in the course of a day« (p. 131). This model assumes, contrary to fact, that some pairs of burghal hidage fortifications were forty miles apart, when on the whole they were a maximum of twenty miles apart, and, thus, fugitives were required at a maximum to travel only approximately ten miles to reach the safety of their walls. It is also assumed that there were no refuges along the roads between burhs. On the whole, Williams is on the right track in seeing the importance of burhs and this especially in the context of mainland influence. His approach would be far better served if he also saw military institutions in terms of Frankish influence.

Andrew Agate, a Ph. D. candidate at the Institute of Archaeology, University of London, currently is undertaking research regarding »suburbs« in Anglo-Saxon England and in this volume he treats »Aspects of Suburban Settlement at Early Urban Centres in England«. Agate devotes several interesting pages to how suburbs are discussed, especially by sociologists and urban planners, and concludes, that for the most part, this work, while perhaps stimulating in various ways, especially in terms of post-medieval views of suburbs, is not of much value to a specialist working primarily with archaeological evidence available for the Anglo-Saxon period. What emerges from this tour of the horizon is that there are valid epistemological definitions neither for cities nor for suburbs, although Agate seems to find Susan Reynolds bundle of characteristics to denote a city of value. Here it is to be noted that American English calls for the use of the word »city« while the English tend to prefer »town«. This may confuse some readers who are not familiar with this difference.

There are two important problems with Agate's approach to the matter of Anglo-Saxon suburbs. As noted above, Agate focuses on an examination of post-medieval observations regarding suburbs, but markedly absent from his tour of the horizon is the uncontroversial fact that suburbs were connected to cities and lesser walled population centers throughout the ancient world and especially in the Roman Empire, not excluding Britain. It seems to us that developing various ideas regarding suburbs, however defined, in the Anglo-Saxon period would benefit as much from an understanding of the subject in the pre-Anglo-Saxon period as from the treatment of discussions of this topic by modern city planners. A second problem is the lack of focus on suburbs within the framework of »Landscapes of Defence«. For example, if a suburb is seen to develop or to be developed in the neighborhood of a fortification, it is certainly important to know whether this was due to government action, or re-

sulted from ostensibly »private« initiative. A second important question concerns the fate of suburbs, outside the walls of a fortress when the city was under attack. Here one might expect that archaeological evidence could cast light, for example, on suburbs that were destroyed while the settlement within a walled fortification survived. Contemporary efforts in the East Frankish/German kingdom to expand the walls of fortress cities such as Worms and Mainz to enclose suburbs make clear that government planners, here bishops, were aware of this problem[26].

The efforts of the Anglo-Saxon specialists in landscape archaeology in this volume are complemented by two additional works that focus in turn on the Low Countries and Scandinavia. The first of these is by Letty ten Harkel who is a Researcher at the Institute of Archaeology in the University of Oxford and holds a Ph. D. from the University of Sheffield. Her work focuses on »A Viking Age Landscape of Defence in the Low Countries?: The ›ringwalburgen‹ in the Dutch Province of Zeeland« (p. 223–260). It is Harkel's aim to reexamine the *status questionis* regarding the characterization of five Zeeland »ringwall« fortifications with the aim of ascertaining whether they belong to a single defensive system of some kind, which has been suggested to some scholars on the basis of their style and construction methods as well information provided by some contemporary written sources. She also seeks to reexamine the argument that all of these strongholds were *vluchtburgen*, *i. e.* refuges constructed so that the local population could find safety during Viking raids. On the whole, Harkel rejects both of these arguments.

A study of various contemporary written sources has led some historians to see these fortifications as the work, for example, of the Frankish king and emperor Louis the Pious († 840), who is recorded to have had strongholds constructed to deal with Viking invasions. However, as Harkel emphasizes, the dating of these strongholds according to archaeological methods fits neither the chronology of Louis' reign nor any other period for fortress construction that is indicated in the written sources. If one accepts these archaeological dates, then the obvious question must be asked, but not in this study, is whether archaeologists have been able to identify any of the fortifications that fit the information provided by these contemporary written sources? In addition, Harkel points out that the archaeological dates for the construction of these fortifications do not fit with times when all of the strongholds were under the control of a single government. Therefore, she argues that they cannot have been a system or part of a single system. She suggests instead (p. 249) that they may have been elements of competing systems as the region of Zeeland formed the frontier between emerging polities.

These fortifications, especially Domburg with a diameter of likely 265 meters, undoubtedly required the mobilization of considerable quantities of human and material resources. The presence of what clearly were permanent populations in these strongholds, engaged in some cases both in manufacturing, *e. g.* spinning of wool and

26 See, for example, the discussion by Heinrich Büttner, Zur Stadtentwicklung von Worms im Früh- und Hochmittelalter, in: Aus Geschichte und Landeskunde. Forschungen und Darstellungen. Franz Steinbach zum 65. Geburtstag gewidmet von seinen Freunden und Schülern, Bonn 1960, p. 389–407; and Ludwig Falck, Mainz im frühen und hohen Mittelalter (Mitte 5. Jahrhundert bis 1244), Düsseldorf 1972 (Geschichte der Stadt Mainz, 2), esp. p. 75–83.

iron work, also would seem to support Harkel's suggestion that these were elements of well-developed defensive systems[27]. In conclusion, we must agree with Harkel's observation that the study of *ringwalburgen* in Zeeland will benefit from further comparative research. However, it seems to us that more archaeological work on the *ringwalburgen*, themselves, is required. This is the case both concerning their role in the local economy, in respect to manufacturing and trade, as well as their use as fortified points. Also demographic estimates must be ventured both in terms of the labor required to construction these fortifications and to defend them. Finally, more archaeological effort must be expended in regard to chronology, and it would seem necessary for archaeologists working in the region to examine the landscape more thoroughly in relation to information provided in the written sources regarding the construction of fortifications during the reign of Louis the Pious and others.

Charlotte Hedenstierna-Jonson, Researcher at Statens Historiska Museum, and Lena Holmquist, Assistant Professor at the Arkeologiska forskningslaboratoriet, both in the University of Stockholm, along with Michaeol Olausson, Curator and Researcher at Stockholm's Länsstyrelsen, have collaborated to author »The Viking Age Paradox: Continuity and Discontinuity of Fortifications and Defence Works in Eastern Scandinavia« (p. 285–302). It is the aim of this essay to focus on the knotty question of chronological patterns in efforts to build fortifications and the abandonments of such efforts, or as the authors put it discontinuity and continuity of fortifications in eastern Scandinavia. Their effort is aimed at coming to an understanding of what would seem to be the paradox of the landscape of defense in the region during the Viking Age, when there was little construction of fortifications despite the warlike nature of society.

In their discussion of the pre-Viking era, Hedenstierna-Jonson *et al.* begin with an obvious paradox by claiming that a »rampart« does not »necessarily indicate military or defensive connotations« (p. 286). While the English is not as clear as one might prefer, the authors would seem to want us to believe that the effort and expense entailed in the construction fortifications, or so we understand »ramparts«, were invested when no military purpose was at issue. They go on to assert: »Only a minor part of the enclosures represent actual fortifications« (p. 286). Either something very important has been lost in translation or these observations constitute meaningless posturing. In short, the authors seem to be using the English word »ramparts« in ways that English speakers do not, and one wonders why the editors permitted this type of double talk to remain uncorrected. However, after denying a military purpose for most »ramparts«, the authors go on to describe defense functions for these enclosures but seem to insist that they were constructed for purposes of social status. The basis for such conclusions is unclear.

The paradox that most concerns the authors, however, is not the matter of constructing expensive fortifications for non-military purposes in the pre-Viking era.

27 Concerning the order of magnitude of fighting men required for wall defense as laid out in the Burghal Hidage, see Bernard S. Bachrach, Rutherford Aris, Military Technology and Garrison Organization: Some Observations on Anglo-Saxon Military Thinking in Light of the Burghal Hidage, in: Technology and Culture 31 (1990), p. 1–17; and reprinted with the same pagination in: Bachrach, Warfare and Military Organization (as in n. 2).

Rather, their focus is on the lack of fortifications constructed or apparently reused during the Viking period in Scandinavia. Here Hedenstierna-Jonson *et al.* are struck by the fact that while operating within the region of the *regnum Francorum* and neighboring lands, the Vikings constructed fortifications, apparently for military purposes, but at home, by and large, this was not done for any purpose. They are adamant, however, in asserting that the lack of fortification cannot be taken as an indication of general peacefulness. Although an atmosphere of general peacefulness might be concluded from a study of the planned town of Sigtuna in the mid-10th century, which the authors do not mention[28]. In their discussion of Norse operations in the *regnum Francorum*, the authors also do not consider the possibility that the Vikings learned to construct fortifications of the contemporary type from the Franks, who themselves were influenced by Roman imperial traditions. In this context, it is noteworthy that among the few Viking age fortifications constructed in Scandinavia during the Viking era, the strongholds or »camps« built in Denmark, such at Trelleburg, were measured out according to the Roman foot.

In any case, the authors seem to have been led to claim on the basis of the rampart and pile barricades at Stegeborg that during the Viking age there were efforts to develop a defense in depth strategy. Unfortunately, additional strongholds, roads, and signal stations simply have not yet been discovered to sustain such a model of a landscape of defense as evidence for supposed growing central power. The project, »Strongholds and Fortification in Central Sweden, AD, 400–1100«, initiated in 1998, may well provide evidence, especially if the necessary work is done on roads, signal stations, and place names, so that in the future more compelling evidence for defense in depth may be developed. Any generalizations at this point in regard to landscapes of defense are premature as evidence to support arguments for centralized power. By contrast, the detailed studies that have exposed much of the emporium at Birka, the examination of which is the strongest part of the article, does provide support for the development of central authority capable of controlling trade among other key aspects of government.

Some Conclusions

»Landscapes of Defense« is an especially important collection of essays for specialists in pre-Crusade military history and also for archaeologists from outside England, who have much to learn from the works of specialists on the Anglo-Saxon period. Unfortunately, the editors of »Landscapes« chose not to provide a badly needed conclusion that related the essays to each other and outlines areas that are important for future research, and so we have undertaken some aspects of that task.

The most important lesson of the volume taken as a whole, and a point that is made with particular cogency in the article by Stuart Brookes, is that the study of early medieval history, in general, and military history, in particular, requires an interdisciplinary approach in which a wide range of sources of information are brought to bear

28 See Sten Tesch (ed.), Makt och människor i kungens Sigtuna. Sigtunautgrävningen 1988–90. 28 artiklar om de preliminära resultaten från den arkeologiska undersökningen i kv. Trädgårdsmästaren 9 och 10, Sigtuna 1990.

to answer salient questions. Such an approach, however, should not be taken to mean that specialists in one field pick and choose individual pieces of information out of context that, as is often the case, suit the ends of a particular argument. Nor may historians simply provide a photograph or illustration of a site without explaining its meaning as if the example were self explanatory. Rather, it is incumbent upon historians, archaeologists, numismatists, specialists in onamastics, and others, to develop an understanding of the methodological imperatives and practices of other fields, so as to be able to draw intelligently from studies beyond their own areas of specialization. In addition, it is crucial to understand that specialists in fields outside of history also have biases, and these must be understood. For example, efforts to diminish the importance of military motivations and to highlight what often euphemistically are labeled as social concerns distort our understanding of both.

A second major lesson to be drawn from this collection is the essential role of economic resources, including most importantly demography, for the conceptualization and actualization of landscapes of defense. Here, it is necessary to understand demography both in relation to labor resources and men of fighting age and capacity[29]. Several of the studies in this volume, most notably that by Richard Abels, draw attention to the massive expenditures in manpower, money, and materiel that were required for the military infrastructure of early medieval polities. In particular, Abels' work makes clear that it is necessary to ascertain the costs of these efforts, and to develop models for the construction of landscapes of defense. This approach has much in common with the practices of *Sachkritik* pioneered by the great German military historian Hans Delbrück. As we suggested above, however, much more research remains to be done.

This volume also demonstrates that archaeologists have a great deal to teach historians about the ways in which material reality can illuminate both human action and human thought. In particular, the understanding by archaeologists that material remains of a landscape of defense can be understood as illuminating planning undertaken by human agents, has the possibility of revolutionizing historical inquiry regarding the intellectual activities of early medieval rulers and their advisors in the military sphere, who often were very well educated not only in regard to history, but also were familiar with technical texts such as Vegetius' »Epitoma rei militaris« and Frontinus' »Stratagemata« as well as Vitruvius' »De architectura«. This process of reading systematic planning into the material record may have the most impact for those periods in which the written word is particularly scarce. Indeed, one very compelling lesson for historians is that our surviving written sources provide information about only a small percentage of the fortifications that have been unearthed by archaeologists, and this percentage likely will become smaller as more excavations are undertaken.

29 See, for example, Bernard S. Bachrach, Some Observations Regarding Barbarian Military Demography: Geiseric's Census of 429 and Its Implications, in: Journal of Medieval Military History 12 (2014), p. 1–37; and specifically with regard to military demography see Bachrach, Aris, Military Technology and Garrison Organization (as in n. 27), p. 1–17.