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The trouble with paradigms. A historiographical study on the development of ideas in the discipline of castle studies

David Mercer

Abstract

In the years leading up to the First World War the academic understanding of the earthen mounds we know as mottes underwent a series of dramatic changes. Exploration of the paradigms behind these viewpoints tells us much about the way in which knowledge comes into being and is used. In particular, archaeologists have created their own foundation myths, complete with hero and demon figures, which serve to emphasize a progressive account of research. The reality is often somewhat more reactionary. In this paper I argue that archaeological histories have impeded understanding of the discipline as the complexities of research remain hidden. As viewpoints become entrenched, creative thought is stifled because the subject is believed to be understood. In castle studies, one a-priori paradigm is that the English motte-and-bailey castle was introduced by the Normans, an idea commonly attributed to Ella Armitage. This paper explores the context behind the adoption of this idea through a case study focusing on changes in understanding. Acrimonious dismissal of alternative points of view has maintained the dominance of this paradigm. I conclude that critical historiographies such as this best serve the development of future understanding of the discipline.

Keywords

Historiography; knowledge construction; academic consensus; medieval archaeology; earthwork castles

Creating academic consensus?

The trouble with Harry is that he's dead but won't stay buried; old paradigms cause similar concern. There is a positivist streak that still underpins much archaeological thought; how else can we explain the concept that archaeological knowledge has laid old controversies to rest? The idea that questions can be answered definitively is not supported by the history of the discipline. A large part of the blame for this error can be attributed to the way in which histories of archaeology still perpetuate the view that knowledge moves from ignorance to enlightenment; for instance the first history of medieval archaeology in Britain contains sections entitled 'The discovery of ignorance' and 'Into the light' (Gerrard 2003). This study

challenges that assumption by engaging with the complexities of knowledge creation.

Current academic knowledge of the medieval castle in Britain is largely dependent upon paradigms established by Victorian castellologists (cf. Counihan 1986; 1989; Higham and Barker 1992, 21–26; Thompson 1994; Matarasso 1995; Coulson 2003, 30–41; Gerrard 2003, 65). In particular the idea that the Normans introduced the motte-and-bailey castle into England has become one of the most successful theories in the history of archaeology and has come to be widely regarded as a well-established fact (e.g. Liddiard 2003; King 1988, 32–34). As a consequence the subject makes a pertinent case study for an investigation into the creation of archaeological knowledge. How did the theory arise? What were the stages in the evolution, and subsequent acceptance, of the idea? What happened to the dissenting voices? It is only by asking such questions of a forgotten academic context that we can hope to understand the image(s) of the past that we propagate in the present. The significance of this topic is highlighted by the vitriolic debates that have raged over the origin of the castle (Brown 1969). Yet few papers have explicitly explored the assumptions behind these controversies. This paper seeks to explore critically the origin of the archaeological paradigms underpinning contemporary knowledge about mottes in England. Much scorn has been poured upon the antiquarians, yet a reappraisal of their contribution to disciplinary formation often highlights new avenues of research. Though largely forgotten today, in the years prior to the First World War the academic understanding of earthwork castles underwent a series of rapid and seismic shifts. Writing in 1906, Isaac Chalkley Gould recounts these changes:

Fifty years ago most Antiquaries would have claimed ancient British origins for this mount, or at least Roman creation; twenty years since we should have said Saxon or Danish, but the researches of recent years have shown that most moated mounts are of Norman days, some of the time of the conquest, others may be as late as the days of the anarchy. . . (Gould 1906, 20–21).

Whilst these and other problems are specific to the matter in hand, they also reveal how academic discourses work, and how consensus is reached, even if this consensus is only nominal. I will argue that the understanding of past academic context is vital to any understanding of contemporary knowledge. Indeed, it is only through the investigation of past practice that we are able to assess the significance of the knowledge that has come down to us. Increasingly, the past is fragmenting into a series of conflicting pasts and understanding the theoretical context of any given idea allows us to improve our understanding of the process whereby we, in the present, ascribe meaning to the past.

Creating theory? Ideas on the origins of the motte-and-bailey castle

In the history of the study of castles one figure is hailed as conclusively establishing the Norman origin of mottes:

the true nature of the castle proper, and its origins as a French and, more specifically, Norman importation, were scarcely established before the beginning of the present century, chiefly by Mrs. E.S. Armitage in that most fundamental and seminal of books, *Early Norman castles of the British Isles* (Brown 1970, 14).

This work was the culmination of over 11 years of study. Essentially, it reworked and expanded two papers appearing in the *English historical review* for 1904. The eight years taken to turn paper into textbook indicate the difficulties inherent in academic publishing. The text is a catalogue of historically documented 11th-century Norman castles found in the United Kingdom and Ireland, supplemented with thematic chapters developing her argument that the motte was ‘in every case of Norman origin’ (Armitage 1912, p. viii). This viewpoint still remains the commonly accepted academic paradigm:

the work of Armitage, marked a major step forward when first developed, replacing a muddled view of the subject in which mottes had often been identified with Anglo-Saxon *burhs*. It has been of enormous influence ever since, and is still, with remarkably few refinements, the accepted orthodoxy (Higham and Barker 1992, 39).

The context: Saxon mottes? Academic work does not take place in a vacuum and the ‘motte question’ was no exception. Armitage’s work on the motte was basically a polemic against the leading Victorian castellologist, G.T. Clark. In his magnum opus, *Mediæval military architecture in England* (Clark 1884), Clark included a chapter on ‘Post-Roman and English earthworks’. Here Clark defines a class of monument, which he terms ‘the mound and base court’ (figure 1), typified by a large earthen mound connected to an enclosure. For Clark these sites are of a generic type, built by both Danes and Anglo-Saxons, which were superseded by the stone castles of the Normans. It was this opinion with which Armitage took issue. ‘Mr Clark’s work . . . which has the merit of being one of the first to pay due attention to castle earthworks, counterbalances that merit by enunciating as a fact a mere guess of his own, which . . . was absolutely devoid of solid foundation’ (Armitage 1912, 2). So why did Clark feel so confident that he could date mound and base court earthworks?

Clark was aware of the Old English term *burh* used in the Anglo-Saxon Chronicle to describe fortifications. Fieldwork in Yorkshire led Clark to equate the *burh* with mottes. Surviving earthworks at Conisbrough and Mexborough, for example, which contain a *burh* element in their name, suggested that he was on the right lines (Clark 1880). Furthermore, he found Saxon credentials for fortifications not containing the *burh* element, such as Laughton-en-le-Morthen (figure 1), where ‘Edwin, Earl of Mercia, Lord of Strafford Wapentake, in Yorkshire, had an *aula*’ (Clark 1884, i, 23). In this way Clark ‘identified’ the residences of the Saxon thanes Waltheof, Tosti, Sweyn and Harold as being the earthworks at Bradfield, Tickhill, Mexborough and Wincobank (Clark 1884, i, 23–25). Clark enumerated

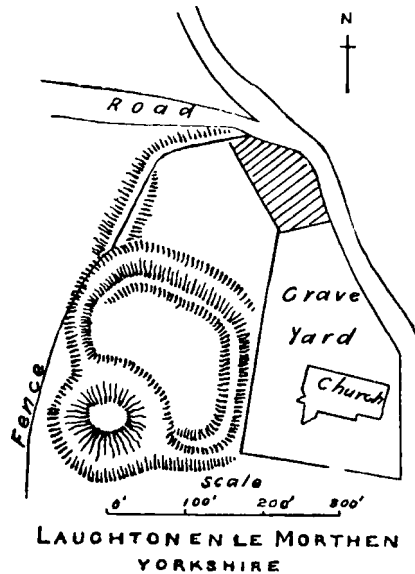


Figure 1 The mound and base court earthwork at Laughton-en-le-Morthen (From Clark 1884, i, 24).

50 places having possessed a *burh*, of which 22 had a surviving ‘moated mound’ (Clark 1884, i, 145). However, this list hides the wide variety between structures; indeed Wincobank possesses no mound and is now regarded as an Iron Age hill fort (Sheffield City Museum).

In many ways Clark placed the study of castles on a serious footing. Given the difficulties inherent in reaching castle sites before the ‘age of steam’, it is fittingly appropriate that this work should have been undertaken by an engineer, who had worked under Isambard Kingdom Brunel on the creation of a national railway network. Unlike Sir Walter Scott, notably when describing Conisbrough Castle in his historical novel *Ivanhoe* in 1819, Clark resisted contemporary expectations to date stone castles to the Anglo-Saxon period. Furthermore, he produced the first castle ‘textbook’, *Medieval military architecture in England* (Clark 1884). This two-volume work is essentially a collection of his previously published papers on individual castles, to which an introductory account providing a general overview of the subject has been added. Unfortunately for Clark, these papers were not revised before reissue, and show the development of his ideas over time; this means that contradictory ideas are juxtaposed. This work describes stone castles, some of which he dated to the 11th century as at Colchester, London and Richmond. This being the case, it is no surprise that Clark thought that earthwork castles must pre-date these Norman stone structures.

Clark’s thesis seemingly brought clarity to the chaotic nature of dating earthworks and by 1900 it appeared that the question had been resolved with the Saxon origin of mottes commonly accepted. Yet within 12 years this had changed from orthodoxy to heresy. We must turn now to a consideration of how this process came about.

Contextualizing Armitage and the 'Norman thesis' The acceptance of academic ideas is still an under-researched area but it is a topic growing in importance (see Latour 1987; 1999). The oft-stated brilliance of Armitage may lead one to assume that the 'Norman thesis' was achieved single-handedly and immediately hailed as an important breakthrough. This was not the case, however, and at the turn of the 20th century the origin of the castle was the subject of fierce academic debate. As Willoughby Gardner reports, 'few archaeological questions have been the cause of [such] great controversy'.¹ Nevertheless, by the time Armitage's book was published in 1912 she felt that the academic argument had been won: 'To many it may seem a waste of labour to devote a whole book to the establishment of a proposition which is now generally adopted by the best English archaeologists' (Armitage 1912, p. ix). So how did a heterodox theory become archaeological orthodoxy?

Despite the primacy accorded to Armitage, the genesis of the thesis was not hers, as she readily acknowledges the work of Round, Hope and Nielson (Armitage 1912, p. viii). In a review of *Medieval military architecture in England*, the historian John Horace Round suggests that Clark

has not, we think, conclusively made out his case for the very early origin of all fortified mounds . . . we hold it proved that these fortified *mottes* were, at least in some cases, erected in the Conqueror's days; and if this is proved of some, it becomes probable of many. Indeed, so far as what we may term private castles are concerned, there is actually, we think, a presumption in favour of this late origin (Round 1894, 43).

This early criticism does not reject the idea of pre-Norman mottes outright. Indeed, he still subscribes to the view of an early origin for certain mottes: 'That they may be traced to the Danish wars we think highly probable, especially from their being so often placed on the banks of rivers' (Round 1894, 31). So for Round at this period the motte was the work of Danish invaders, rather than the invention of the English (Saxons), which he argues for on the grounds that 'their prevalence in Normandy . . . points to their Scandinavian origin' (Round 1894, 32). This we may term 'topographical reasoning'.

Round was by no means the first to use topographical reasoning to advance a Norman date for mottes, though whether he was aware of these antecedents remains unclear. As early as September 1848, in a paper read to the newly founded Bury and West Suffolk Archaeological Institute, Samuel Tymms suggested that mottes were Norman. He felt that the mottes in Britain so closely paralleled surviving mounds 'within twenty leagues of Caen' that it was sufficient proof of their common Norman origin. His argument, that the occurrence of mounds on castles in Normandy proved the Norman origin of similar British mounds, was not particularly sound – all that linked the sites was similarity of form as no artefacts had been discovered to link the sites chronologically. In this respect the sites remained undated; therefore the British sites could just as easily be the earlier. As Tymms's work has only partially survived it is difficult to know whether he was aware of the potential

flaws in his argument. In the absence of excavated evidence what more could he do?

Tymms's argument is based upon the assumption that the area of greatest concentration of a particular feature is also likely to be its origin – an assumption still adhered to by architectural historians (Smith 1992). It is for this reason that he makes so much of the relative concentrations of the surviving mounds. He cites some 60 castles in the vicinity of Caen that possess mottes, yet he is only aware of some 'nearly thirty remain[ing mottes] in England and Wales' (Tymms 1853, 66). Although no complete list survives of these English mottes, Clare Castle was certainly among them, as it was the subject of his paper (figure 2), and it seems clear that Tymms intended to write more on the subject but death intervened. We now know that his estimate was way off; Cathcart King enumerated 741 surviving mottes in 1988 (a figure which continues to increase due to the discovery of 'new' sites), which would have dealt the death blow to Tymms's argument (although of course more mottes are now known from France, too). Nevertheless, even though the French mounds remained undated at this period, they clearly could not be the work of Saxons. Despite the significance of Tymms's observation his work was not followed up and it appears to have been overlooked by subsequent academics; publication solely in a regional journal cannot have helped his views gain wide prominence.

Turning away from the historical evidence, there were a number of sites which were shown to be Norman on the basis of excavated evidence. Expecting to discover prehistoric remains, General Pitt Rivers excavated at Caesar's Camp, Folkestone in 1878. However, finds consistent with a date in the Norman period, and the absence British pottery in the rampart, forced him to conclude that the site was probably post-conquest. Indeed, the general felt that the evidence for a Norman origin of the site was so overwhelming that he was compelled to argue that a horseshoe of a previously supposed Celtic type 'will go far to discrediting the opinion that it was discontinued before the tenth century' (Pitt Rivers 1883, 451). Despite the contribution of archaeology in providing a date for the earthwork, Pitt Rivers felt that monuments of a historical period were the remit of historians: 'Having brought the camp within the pale of historic times, I leave further speculation on the subject to the historian' (Pitt Rivers 1883, 453). Similarly, Colonel Morgan concluded that the finds from Old Castle Camp, Gower 'point to their being of a post-Norman period' (Morgan 1899). This site also demonstrates that Victorian archaeologists could recognize post-holes (figure 3).

It is important to note that it is impossible to separate pure 'history' from 'archaeology' and that at this time being an archaeologist of the medieval period also meant being a historian, and whilst not every historian was an archaeologist, Round for one certainly tried to equate surviving historical documents with actual sites. He was an active member of the Essex Archaeological Association and as a result was well aware of the physical remains of the castles themselves, as his papers on the subject testify. However, it was this simple equation of written history with physical remains that led to so many difficulties. Round criticizes Clark for identifying associating mottes with the fortifications mentioned in the Anglo-Saxon Chronicle, for

CLARE CASTLE.—I.

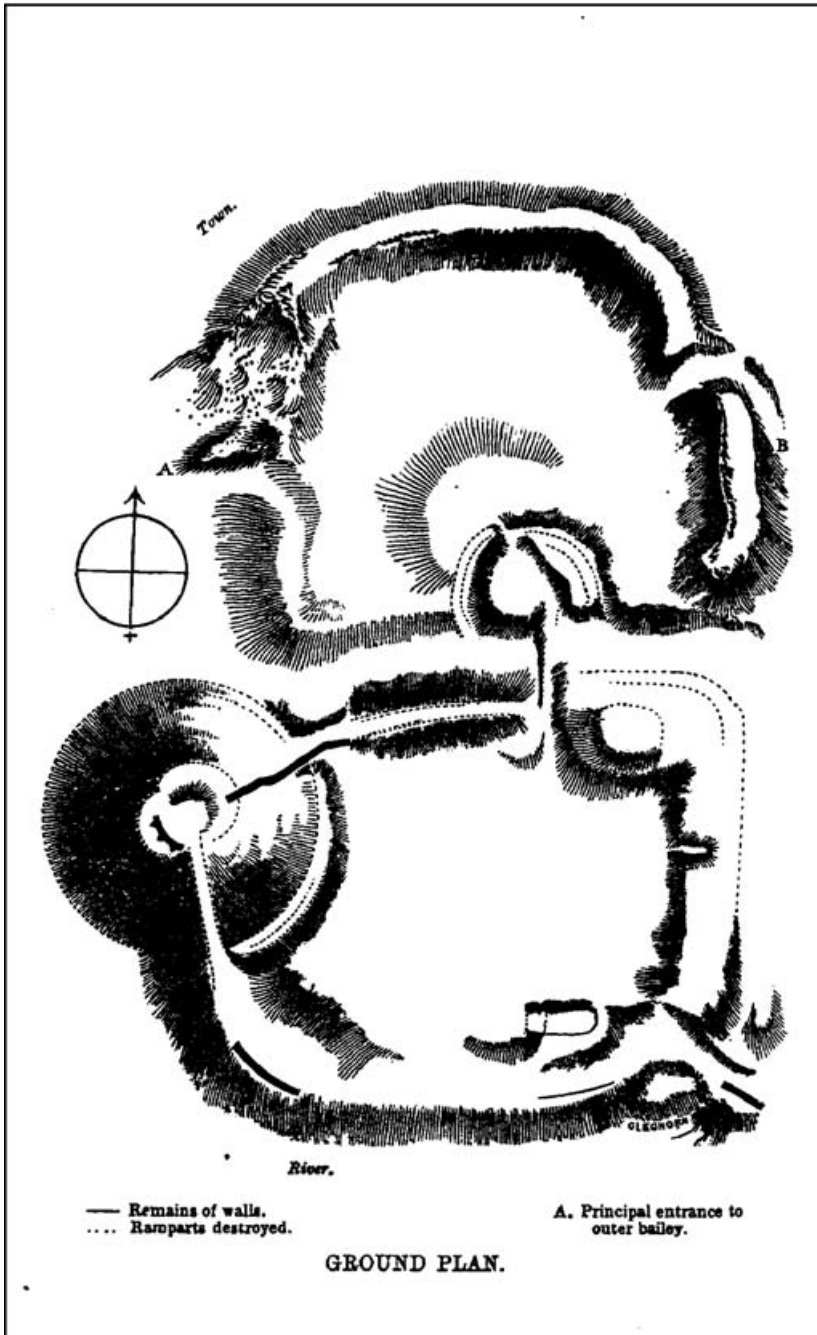


Figure 2 Plan of Clare Castle, showing motte and baileys (from Tymms 1853, plate I).



Figure 3 Post-holes showing in section uncovered at Old Castle Camp, Gower (from Morgan 1899, 255).

the mound could have been a later addition. Yet he is still able to claim that ‘the mound at Kennardington, in Kent, . . . may safely be identified with that which the Danes are recorded to have raised at “Apuldore” in 893’ (Round 1894, 33). Interestingly enough, it was Clark (1889, 206) who had made this identification, a fact unsurprisingly not acknowledged by Round.

As with the invention of the wheel, it seems apparent that separate individuals came independently to the conclusion that at least certain mottes could be attributed to the Normans. Besides Round, George Neilson (1898) established that mottes were Norman in Scotland and William Henry St John Hope contributed to the debate with a historical contextualization of the words for fortifications used in the Anglo-Saxon Chronicle which demonstrated the difference between the Danish *geweorc*, Saxon *burh* and Norman *castel* (Hope 1903). He went on to apply this understanding to argue that the motte-and-bailey at Cambridge was the castle mentioned in Domesday (Hope 1907).

That so many scholars working in the field of castle studies rejected the ‘Saxon’ thesis provided the momentum to overturn Clark’s view. Clearly this rejection by Armitage’s peers had more to do with the adoption of the ‘Norman hypothesis’ than the logic of the position alone.

Reassessing Armitage’s thought

Despite her statement that ‘the spade, and the spade only, can decide the date of an earthwork’ (Armitage 1895, 40), Armitage paradoxically did not use excavated evidence to date earthworks at all. The crux of her argument

rests upon typological and historical factors. All the castles she considers 'are known to have existed [*sic*] in the 11th century, because they are mentioned in either the Domesday [*sic*] Book, or in charters of the period, or in some contemporary chronicle' (Armitage 1912, 94). This has been adduced as evidence that the state of the archaeology of the castle was not able to supply the burden of proof. However, the aforementioned excavations highlight the fallacy of this statement. Why did Armitage not use the archaeological evidence that was available regarding the date of mottes? She certainly was aware of excavations at Folkestone, Penwortham, Hallaton and Burton-in-Lonsdale. Indeed, Armitage had some competence herself as an archaeologist, conducting her own investigations at Almondbury, which demonstrated that the Normans had occupied the site. She submitted pieces of pot found in the course of the excavations to Pitt Rivers who pronounced them to be Norman.² Armitage argued that this provides a *terminus a quo* for the date of the motte, in other words it must have been 'thrown up after the Normans came to England' (Armitage 1900a, 403). It is surprising that more is not made of this evidence in *Early Norman castles*. However, whilst archaeological excavation could prove a Norman date for specific examples, Armitage was more concerned with establishing a general theory (as Pitt Rivers had done at Cranborne Chase). 'Whilst strongly asserting that the spade only can certainly decide in individual cases the date of an earthwork, it was inevitable that General Pitt Rivers should have been led, after the investigation of so many of these remains, to certain general conclusions which are of great value' (Armitage 1895, 42).

Armitage develops Round's criticism in her book *A key to English antiquities. With special reference to the Sheffield and Rotherham district*, which was published in 1897. Our interest in this book is the way in which she interprets sites that had hitherto been used as evidence for the 'Saxon' theory. I wish to focus debate upon three examples which demonstrate how Armitage selectively uses evidence to back up her theory.

Castle Hill, Bakewell, Derbyshire This is an important site in that it had been regarded as the remains of a burh belonging to Edward the Elder (900–24), an association that Armitage is hailed as having disproved (Fry 1996, 121). Though small, this site remains a classic example of the motte-and-bailey form (figure 4; Swanton 1972).

Armitage could not produce 'conclusive evidence' (Armitage 1912, 47) to prove that the castle was Norman. Nor could she deny the historical documentation that attested the presence of a pre-Norman fortification; in the Anglo-Saxon Chronicle for the year 924 we read, 'King Edward . . . went to Bakewell in the Peak District, and ordered a stronghold to be made in the neighbourhood and manned' (translation Swanton 1996, 104). Clark had linked this statement with the surviving earthworks. To refute it, one would expect Armitage to argue that the site was too small to be a *burh* or that the motte was a Norman addition. Surprisingly, she opts to dismiss the site as representing a motte-and-bailey altogether, stating, 'I am doubtful whether

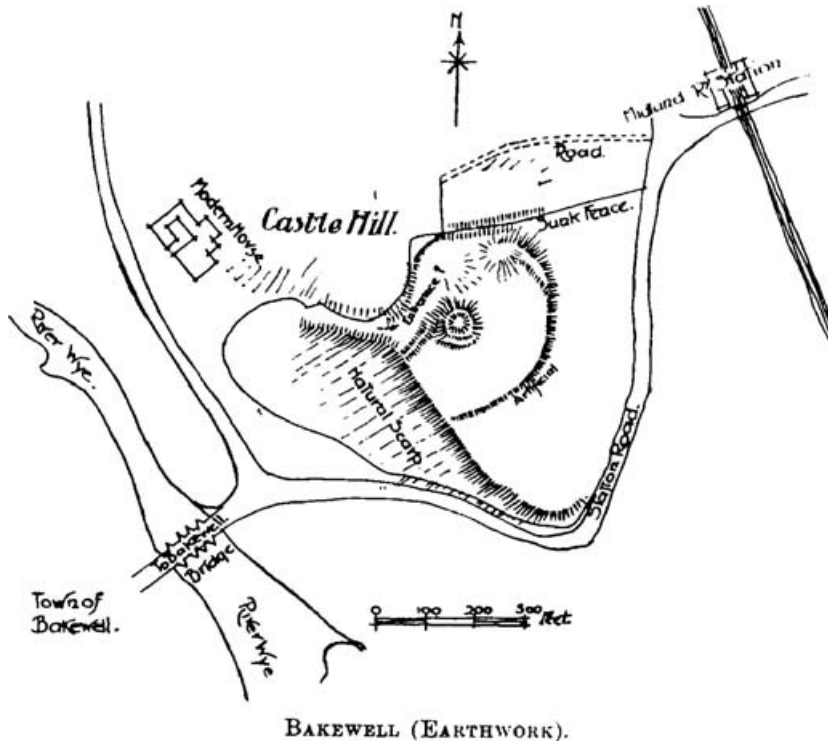


Figure 4 Plan of Bakewell Castle (from Pryce 1904).

it belongs to the type I am describing' (Armitage 1897, 56). Her argument, justifying this exclusion, denies the existence of a motte altogether: 'It is rather remarkable that in this place where there certainly was a Saxon earthwork, the mound which is supposed by some to be characteristically Saxon should be absent' (Armitage 1897, 56).

Though the site is overgrown, the motte survives to a height of nearly four metres. This is not a feature that could be overlooked. Could Armitage have mistaken the site? In fact, overlooking Bakewell, there is a second fortification on a ridge above the castle site (NGR SK 228692). However, her acknowledgement of the existence of 'a very small mound' seems to confirm that Armitage did visit Castle Hill (Armitage 1897, 57). Given the existence of a mound, why does Armitage not accept it as a motte? Instead she argues it away as possibly being 'only ... the covered ruins of a tower' (Armitage 1897, 57). One explanation is that Bakewell was a threat to Armitage's thesis.

Laughton-en-le-Morthen, South Yorkshire As we have seen (figure 1), Laughton-en-le-Morthen is one of Clark's key sites in establishing the 'Saxon origin' of the motte-and-bailey. Armitage argues that the site cannot be shown to be that invoked by the documentary evidence:



Figure 5 The earthworks at Conisbrough Castle (photograph: the author).

Domesday says that Earl Edwin (the brother of Morkar) had a hall at Laughton, and it is therefore frequently assumed that these earthworks are the site of it. But there is no evidence to prove it, and they may just as well have been thrown up by Roger de Busli, the first Norman lord of this estate (Armitage 1897, 55–56).

Yet the fact remains that there is no evidence to disprove it either.

Conisbrough Castle, South Yorkshire Again, Armitage interprets Conisbrough Castle as being of likely Norman origin. However, this position creates some difficulties. First, following Clark, there remains the problem of the place name, which is recognized by Armitage: ‘Conisborough [*sic*], as its name clearly shows, was once a burh or stronghold of an English king’ (Armitage 1897, 96).

Since she is making the case for the Norman origin of mottes, one would assume that she would make much of the fact that Conisbrough has no existent motte, as she tried to do at Bakewell. Instead, she perplexingly makes the case that the entire hill on which the inner bailey stands is a motte. Yet this is a largely natural hill, some 18 metres high and over 73 metres long (figure 5), which one would expect to be reasonable grounds for excluding it from any consideration as a ‘moated hillock’.

This has led subsequent writers to claim that the keep stood on the site of an earlier motte (e.g. Johnson 1989, 20). However, Clark’s published section of the ‘keep’ (Clark 1883, 136) serves to demonstrate how this is highly unlikely. The entire motte would have had to have been entirely swept away, an unnecessary work, as demonstrated by the Yorkshire castles of Tickhill,

Sandal and Pontefract, where the tower was raised on and around the motte; the motte ditch would moreover make subsidence a real threat.

As Armitage was aware that the castle stands on a 'natural knoll', why should she be so determined to place the castle in the same category as other Norman castles? Surely it would make more sense to argue that the site was substantially different from other motte-and-baileys and therefore could possibly represent the site of the 'king's stronghold'? The main obstacle was Clark's usage of the site to argue for the existence of Saxon castles; to demolish his thesis she had to deny the link between the castle earthworks and the Saxon defences. Clark had demonstrated that the surviving masonry at Conisbrough post-dated the early Norman period, therefore Armitage had no alternative but to ascribe the earthworks to the earliest Norman occupation, under Earl Warenne. Her replacement paradigm sees the motte as an integral feature of early Norman castles.

Armitage is further constrained by her view of the original form of the *burh*. Having searched through all the illustrated Anglo-Saxon manuscripts in the British Museum, 'to find an Anglo-Saxon picture of a *burh* or fortress', she 'invariably found a picture of a stone castle, never an earthwork' (Armitage 1897, 310). If this is accepted, then it follows that the earthwork remains could not be the site of a Saxon stone fortress. Moreover her view of the *burh* as a borough led her to the conclusion that a *burh* 'would certainly have enclosed the church' (Armitage 1912, 47). At Conisbrough the Saxon church is situated in the town above the castle and it would thus represent a Norman insertion into the Saxon town. Still, Armitage admits that we are left with a problem: 'unfortunately we do not know with certainty what Anglo-Saxon fortifications were' (Armitage 1897, 98).

Deconstructing the consensus of the 'Norman' paradigm

To refer to the change in interpretation of mottes from Saxon to Norman as a 'paradigm shift' is to obscure the complexity of this process, as well as to mask differences in opinion. Both internal and external factors play a part in the process of knowledge construction. The three examples considered internal factors, demonstrating how commitment to a thesis constrains interpretation of the evidence. We turn now to a consideration of how external factors may influence understanding.

For Round the question was not merely academic but had practical and far-reaching consequences. In Round's view the English were defeated at Hastings due to an 'excess of liberty' which resulted in the 'want of a strong centralised system' (Round 1895, 394). This could be demonstrated by the Norman chronicler Ordericus Vitalis, who, writing in *ca* 1125, maintained 'the fortresses (called *Castella* by the Gauls) had been very few in the English provinces and for this reason the English, although warlike and courageous, had nevertheless shown themselves to be too weak to withstand their enemies'. Round's 'hawkish' politics may be detected in his statement that 'the old strongholds had been neglected' due to 'a long period of peace'. This was where liberal politics seemed to be taking the British Empire at the close of the 19th century. Russian expansion in Central Asia was causing concern as to whether the Tsar had designs on India. Round's history was a warning to politicians on what he saw as the inherent dangers in Liberalism. This is

history with a ‘moral’ agenda. Round was politically Conservative, and it was his politics which first brought him into dispute with Professor Freeman (over the House of Lords), so no wonder the debates between the two were so heated. Indeed, Round’s attack on Clark can be attributed to Clark’s acceptance of the view of the Norman Conquest as enunciated by Freeman: ‘their close connection will compel us, occasionally, to consider the views of both writers’ (Round 1894, 29).

Similarly, we can see the rejection of the possibility of Saxon mottes in the works of Armitage and Thompson as being influenced by the ‘metanarrative’ of social evolution as articulated in Nilsson’s *Primitive inhabitants of Scandinavia* published in 1868. The Norman Conquest was seen as marking the watershed between tribal and feudal societies. The small area of the bailey indicated that they were personal rather than communal fortifications and therefore were incompatible with a tribal state. ‘Perhaps the most fascinating feature in the study of English strongholds is their gradual change and development in the hands of successive races’ (Round 1894, 29). Thompson takes this view a stage further when he argues that

it may safely be said that these fortifications, at any rate until the end of the period, whether their builders were Saxons or Danes, were intended to protect, not private individuals, but a community. Of the private citadel or castle we hear nothing until the period immediately before the Conquest, and then it is only heard of as a foreign importation (Thompson 1912, 24).

Armitage’s legacy

We are now in a position to evaluate Armitage’s legacy. Whilst her importance to the development of castle studies is beyond question, this pre-eminence stems from the fact that her ideas still remain the dominant paradigm within castle studies today:

It sometimes seems to me, with a mixture of admiration and despair, that almost any question one may raise about castles has already been answered, and often definitively answered, by this remarkable woman [Armitage] in her remarkable book, . . . and the main reason why the study of early castles and their origins in this country has not got very much further to go since her day is that she did not leave us very much further to go (Brown 1969, 134).

However, we can regard Armitage’s pivotal status in the history of castle studies as an example of academic hagiography. By focusing on the personage of Armitage, we tacitly accept her thesis. Moreover, by constant repetition of the brilliance of her work it has become sacrosanct, to the extent that any criticism of it is invariably quashed. Whilst Joan Counihan is one of the few scholars to have considered the social context in which Armitage worked, there is an inherent danger that biography reinforces this hagiographic tendency (Counihan 1986; 1990; 1991; 1998). Such continuous reference to the work of Armitage has elevated her status to the realm of myth; as highlighted by Counihan, ‘interest in castle studies is growing and growing and Ella Armitage is becoming the patron Saint of Castellologists’.³

This is not a work of iconoclasm; Armitage's scholarship is beyond doubt. Of more import is to understand how such academic hagiography has affected the development of the study of castles (in this instance). Her elevation is such that Professor Allen Brown 'shame-facedly' corrected a minor point of hers on the dating of Arundel keep (Counihan 1990, 56). This fact alone demonstrates the cultural problems associated with the revision of past scholars' work. Surely this would not be the case if knowledge creation were a simple matter of pushing back the boundaries of the unknown. Indeed, the 'Norman origin' is not so much a case of discovering the unknown, rather it is the imposition of a new framework of understanding upon that which had formerly been held to be known. By losing sight of those who criticized Armitage upon reception of her published work, we lose sight of the process behind the inception of knowledge. It can be highly convenient to overlook the fact that academia is a process, as it is far easier to carry out the tried and tested process of research without asking too many awkward questions of what constitutes acceptable academic knowledge. It is clear that one cannot remain impartial towards one's subject. However, this is not to say that we are thereby excluded from creating valid history. Indeed, it is because perspectives change that we are able to re-evaluate past academic contexts, though contemporary issues will always influence what we judge relevant.

Conclusion: on paradigms and consensus in archaeology

It is clear that there is a danger of arguing from the particular to the universal, and this study has demonstrated the need to contextualize archaeological knowledge. Indeed, the case of medieval archaeology is very different from that of prehistoric archaeology where no historical evidence can be adduced to support academic theories. Therefore this paper is a plea for more critical historiographical studies. Of necessity there is a need for a thesis to gain a 'critical mass', whereby other practitioners join forces to advance a particular argument. In the case of the history of archaeology much work is done that is little more than anecdote and it remains to be seen whether archaeological historiography can break free from its contextual specializations and contribute more generally to the sociology of science. Whilst this study has much to contribute to current debate on castles, I wish to leave that to one side and instead concentrate on exploring the more universal themes which have a bearing on archaeology as a whole: the problem of academic consensus and, further, of the elucidation of archaeological paradigms.

Consensus has been viewed here as a negative factor stifling debate. Our understanding of the medieval castle was stuck in a rut for almost a century. New understandings could not be reached precisely because the archaeological evidence was slotted into a framework established by Armitage and Thompson. This is a cautionary tale; much of what has passed for archaeological knowledge has been found to rest upon untested assumption. The views of certain prominent scholars remain dominant and authoritative even when confronted with contradictory evidence. Tradition compounds the problem, as we see a thesis harden into dogmatic fact due to its repetition over time. The result is the creation of an archaeological hagiography whereby the

views of establishment figures remain sacrosanct. Yet those establishment figures often recognized the limitations of their work; as Armitage states, 'I am not in a position to disprove the assertion [of an Anglo-Saxon origin] conclusively' (Armitage 1897, 92). Yet it is commonly held that Armitage did supply the burden of proof necessary to overturn the 'Saxon theory'.

Consensus impedes academic research in other ways. Preconceptions shape our interpretation of material culture in that we 'find what we look for'; therefore, in the case of mottes, the understanding that they are a Norman importation precludes the possibility of the existence of pre-Norman mottes. Furthermore, research programmes are shaped by what is held to be 'known' and 'unknown'. The implication of this study is that the traditional ways of producing archaeological knowledge need to be reassessed. Categorization and logic can only work with what already exists and new understandings will only develop when we can think 'outside the box'. It is my contention that critical historiographies like this paper represent a tool that allows reassessment of the 'boxes' of academic thought. It is likely that a similar situation is to be found in other areas of archaeology and it is argued that more critical historiographies are needed to explore the assumptions upon which current theory is based.

With the benefit of hindsight we can see which archaeological theories have been successful. However, the study reminds us that knowledge creation is a process. The problem with using the term 'paradigm' is that it obscures this process. 'Paradigm shifts' have been shown to be complex, and to understand the process of knowledge creation we need to address this complexity. If we are to use the term 'paradigm' meaningfully, we must pay closer attention to the academic context within which such constructs emerge. The 'final' archaeological report traditionally presents conclusions as a *fait accompli* without exploring the thought processes leading to this understanding. It seems that archaeologists will increasingly need to articulate these patterns of thought as a matter of course to allow future generations access to this reasoning process. This, I argue, will not only more rigorously define those paradigms employed but, more importantly, it will also allow greater access to the logic underpinning academic arguments. It may be time to reassess the 'final' archaeological report and, rather than trying to establish a definitive position, to pay more attention to problems of interpretation and the ambiguity of evidence.

The assumption that consensus equates with knowledge cannot be sustained. Whilst books on castles proliferate, the majority repeat the same ideas. When a subject is believed to be well understood, it is time to shift the viewpoint from which we are regarding it.

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Notes

¹ Victoria County History, Warwickshire I, 1904, 352–53.

² Letters in the possession of the Salisbury and South Wiltshire Museum.

³ Letter dated 8 February 1993 in possession Newnham College archives, Cambridge.

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