3 THE CRITICAL HISTORY OF A CONCEPT: A BREAK WITH CONTINUITY SCHOLARSHIP

…It is stability rather than social change that needs explaining

(Shanks and Tilley 1987:212)

In this chapter, I offer an overview of the main lines of Norwegian settlement studies up to the present. I shall show:

1. How a specifically Norwegian research tradition came about.
2. That this has taken little account of the primary settlement evidence.
3. How current questions can no longer be answered in the framework of this model.
4. That internationally oriented settlement studies have also been conducted in Norway, parallel with the specifically Norwegian tradition.

The history of research in Archaeology concerned with human settlement is closely bound up with the history of the subject of History. This is naturally connected to the fact that the first to take up the issue of prehistory in Norway were not archaeologists but inter-disciplinary scholars of the Humanities. Academics such as Oluf Rygh, Christian Magnus Falsen, Gerhard Schønning, Peter Andreas Munch, Magnus Olsen and Rudolf Keyser made use of both archaeological and historical source material. Even after Archaeology became a separate discipline, theory and methods from History were widely used in settlement research. I shall argue that some key premisses for current settlement research — the 'paradigm' or the 'discourse', if you like — were established already by the 1814 generation of Norwegian historical scholarship (where I include Schønning, who strictly was of an earlier generation). Important foundation stones of Norwegian settlement scholarship were therefore laid around a hundred years before Shetelig undertook the first Norwegian excavation of prehistoric buildings (Ch. 2). Since historians have been at the heart of the development of the retrogressive method, there is a research history that irradiates studies otherwise shedding light on the Middle Ages and which have been of major methodological influence on Iron Age research.

Reviewed in this chapter is that field of previous research which concerns the farms, the farmers, the agriculture, and their role in Iron-age society in Østlandet. Theories and questions concerning immigration, diffusion and ethnic groupings are considered less significant to my research foci but are noted briefly where relevant. I shall firstly review the origin of settlement scholarship's firm belief in continuity, and then show how this faith in continuity was a key part of the development of a specifically Norwegian form of the retrogressive method. I shall also show that although this approach has dominated Norwegian settlement research, it has not monopolized the field, or been without its critics. The largest part of this chapter is concerned with a general, national history of research, but at the end I shall take a closer look at a number of important works which are concerned with the evidence from Østlandet, where I place particular emphasis on results that diverge from what I have labelled continuity scholarship.

The growth of History, and somewhat later of Archaeology, as distinct academic disciplines in Norway coincided substantially with the nation-building of the 19th century. Arnfrid Opedal (1999) has shown how, far into the 20th century, archaeological research on farms can be viewed in the light of the construction of a Norwegian identity. Wenche Helliksen (1996a) has shed light on how evolutionism has marked Norwegian archaeology. Together with other circumstances, this, according to Lars Pilø (2000; 2002; 2005), led to a specifically Norwegian history and tradition of research. He demonstrates how much of Norwegian farm research from c. 1920 onwards can be understood in light of the 'primeval farm model' (*Urgårdsmodellen*), and that the archaeology and history of settlement have largely been written on the basis of the distinctly Norwegian version of the retrogressive method. Helliksen, Opedal and Pilø provide a welcome insight into aspects of settlement research in Norwegian archaeology (and for other approaches to the research history, see Henriksen 1994; 1999; Olsen 1997; Martens 2004; Guttormsen 2013) — but by studying continuity rather than evolutionism, nationalism or particular models, it is possible to see a clear thread running

from the 1814 generation through the primeval farm model into contemporary scholarship. By following this thread backwards in time it becomes possible to see that continuity was a basic premiss for an enormous amount of Norwegian settlement research. It may be objected that I too am using backward-looking or retrogressive methods in my own history of research — a method that I am otherwise critical of. I shall put things more precisely, therefore, in explaining that my criticisms are directed primarily at the specifically Norwegian version of retrogressive analysis, in which continuity is an underlying premiss while concurrently the method is used to shed light on periods with weak or no primary evidence (see Ch. 3.1.3 for the relationship between retrogressive and retrospective methods). In my view, the method may indeed be employed to explain how the situation from which one is looking back has come about. Retrogressive methods can therefore be used to explain a contemporary settlement pattern. But the contemporary settlement pattern cannot be used as a starting point for a retrogressive study to explain the settlement pattern of the 4th century AD (for example). No event can be explained by something that happens later.

CONTINUITY AND RETROSPECTION — FROM 1814 TO THE PRESENT

The 1814 generation in Norwegian historical scholarship was preoccupied with the population of the land area, the unification of a kingdom, democracy, property (*óðal*) rights, governorship (*lensvesen*) and aristocracy (for the most important contributions, see Schøning 1771; 1773; 1781; Rothe 1781; Falsen 1815; 1821; for an overview, see Dahl 1990:15–40). Their ideas were enshrined by the Norwegian School of History, to which P. A. Munch and Rudolf Keyser were central. Presented below are four concepts which I believe formed the bases for practically all later settlement research.

The 1814 generation and the Norwegian School of History

The historians who formed the Norwegian School of History wanted to demonstrate "the Importance of Norwegian Nationality in Prehistory and Nordic Authenticity in the Present" (Munch 1874:II:28 [translated]), and must of course be understood in light of the fact that Norway gained its own constitution in 1814 when it broke free from Denmark, while in the second half of the 19th century there was a movement to gain full independence from Sweden. They were also committedly cross-disciplinary. According to Munch (1852a), "the so-called modern School of History in Norway" based itself upon historical sources, philology, archaeology, geography and anatomy. The latter was significant because it showed the differences between human races and their particular anatomical characteristics. Keyser and Munch were internationally orientated, and drew inspiration from German and especially Danish historians (Andersen 1960). It is four of their notions in particular that have left their mark, even though the ideas themselves have long since ended up on the scrapheap of History. They believed that Aryan invaders (from the east or the north) settled here in a colonization of an unpopulated Norway. Since these colonists came to an unpopulated land, there was no indigenous population to exploit as slaves or sharecroppers and so no basis for a nobility. Norway "in the olden time" — which in Munch's case (1852b:467) meant before Harald Finehair — was consequently viewed as a democratic society of free, equal, landholding farmers with no aristocracy or unfree peasants, in contrast to Denmark and in part to Sweden. Property rights and the right of undivided succession ensured that the farms were not sub-divided into smaller units but remained whole. Finally, either race — the supposed fact that Germanic settlers of Norway were not mongrelized with an indigenous population, unlike the Swedes and the Danes — or special ecological or topographical circumstances, promoted and maintained a distinct conservatism in settlement and subsistence, a sparse aristocracy, and a class of free farmers (Falsen 1815; 1821; Keyser 1843; Bull 1920:53; Andersen 1960; Dahl 1990:53; Kjeldstadli 1992:58–9). These beliefs gradually generated the framework for research into the farms. The major settlement-phase farms, the primeval farms (*urgård*) in Pilø's terminology, were, according to this tradition, established immediately following colonization. Legal protection via the right of óðal and ideological protection produced by the conservatism of the population stood in the way of change. The result is strong continuity in the physical bounds of the farms. Such changes at farm level that did come about were divisions that can be reconstructed by using the retrogressive method. At a more general level, marginal areas were occupied if the population grew, and the farms which were founded there were also the first to be abandoned if the population fell.

The distinctness of the Norwegian people

The origin of 'Norwegianness' was a matter of long dispute — but not the fact that there was such a thing. Was Norwegianness produced by the fact of a distinct Norwegian race or was it created through adaptation to the distinct Norwegian natural environment (Hesjedal 2001:52)? In 1868, Ludvig Kristensen Daa gave "a funeral address for the Norwegian colonization theory" and argued that the people of Norway had invaded from the south and so did not differ from the Swedes or Danes in respect of race (Daa 1869; Grundtvig 1869; Dahl 1990:80). Daa was influenced by new currents in ethnography and geology, and abrupt events such as migrations or natural disasters were for him no longer important as explanations. Rather, he explained the cultural characteristics of individual peoples as the product of long-term processes; several historians stressed that a national spirit was the result of common European trends (Dahl 1990:126–9, 142–4). The distinctiveness of the Norwegian people was thus explained partly through the natural conditions in which they lived and partly by the level of culture they had achieved, itself partly through internal development and partly under influence from other populations. The thesis of an independent Norwegian farmer — through to the age of Harald Finehair, at least — was preserved by later historians such as Ernst Sars and T. H. Aschehoug. According to Aschehoug (1866) and Daa (1869) it was the special characteristics of the land — the lack of arable — that yielded a sparse aristocracy and therefore a stronger farmer class. Ernst Sars had similar views, and somewhat later on concluded that the clear thread that runs through the history of Norway was formed by a social order based upon the farmer class (Worm-Müller 1920:20; Dahl 1900:152, 163). Sars (1877) was, according to Jacob S. Worm-Müller (1920:30–1), influenced by the doctrine of evolution, and particularly in Herbert Spencer's Social Darwinistic mode, and consequently saw the formation of nations as a product of natural conditions and historical development. He therefore dismissed the idea of a particular, racially rooted, national spirit. Since then few have explicitly claimed that the distinctiveness of Norwegian agrarian settlement when compared or contrasted with that of Denmark and Sweden can be linked to race (although Andreas M. Hansen is an important exception: Ch. 3.1.4). For a long time, a pressing question, particularly for archaeological scholarship, was rather whether the expansion and contraction of settlement was due to immigration or emigration (Herteig 1955a).

The retrogressive method was also employed in the study of building practice. Research on the buildings themselves was initially conducted without direct primary evidence. Keyser (1847) wrote on this topic on the basis of the written sources, particularly the Icelandic sagas. Nicolaysen (1849:313) criticized that article and posited that building practice was static and so can be traced back using the stave churches and contemporary rural buildings as starting points. It was certainly a long time before the first Norwegian excavation of any prehistoric building ruin was carried out, even though it was known that such sites existed (Neumann 1842; Nicolaysen 1862–6:313; de Fine 1870 [1745]:109–10).

The retrogressive method and the Institute for Comparative Cultural Research

To draw conclusions concerning earlier, more obscure, situations from historically known circumstances was a common practice throughout the 19th century. Such back-projections were possible in light of the strong assumed continuity and conservatism in settlement. Around 1930, however, this way of approaching things was developed into the retrogressive method as we know it today. A major part of this methodological development came about in the Institute for Comparative Cultural Research. This institute was established at the University of Oslo in 1922 on the initiative of Professor Fredrik Stang. The key idea behind the Institute was internationally collaborative scholarly research, and in the inter-war period the Institute was an important research centre for both Norwegian and international scholars. A number of key works representing various disciplines were published in the Institute's series.

In Norwegian and Scandinavian archaeological literature, retrospective and retrogressive methodology are frequently treated as synonymous. In strict terms, however, they are two rather different methods, even though time-depth is crucial to both (Baker 1968; Friedman 1996; Pilø 2005:8). The retrogressive method or *la méthode régressive* was developed by historians, starting from later periods or the present with a view to understanding earlier times. The restrospective method or *geographie humaine retrospective* was developed by cultural geographers, using the geographical structures of earlier periods in order to understand later situations. *Retrospektiv* is a firmly embedded term in Norwegian and Danish archaeological literature, and is also used where 'retrogressive' would strictly be correct in relation to international literature. I have opted to

use the internationally recognized term, even if that risks confusion (cf. Thrane 2006). Swedish readers, however, will be able to follow the text (Widgren 2000; Karsvall 2013).

The retrogressive method is best known internationally through Marc Bloch's *Les Caractères originaux de l'Histoire rurale française* (1931), published in English as *French Rural History: An Essay on its Basic Characteristics* (Bloch 1966). The French version was published in the Series B Texts (*Serie B Skrifter*) of the Institute for Comparative Cultural Research, and is based to a great extent on a set of lectures held at the Institute. It is in any event also partly a product of Bloch's period as a research fellow at the Institute, cooperation and correspondence with the historian Edvard Bull, and influence from other Norwegian and overseas historians (Bloch 1966:xxiii; Baker 1968; Friedman 1996; Iversen 2004:51; Imsen 2010). The method which Bloch described as *la méthode régressive* thus had predecessors both in Norwegian and other countries' historical and archaeological research (Meitzen 1895; Widgren 1997; Rønneseth 2001), but in the present context those appear less relevant to the development of a uniquely Norwegian variant.

Asgaut Steinnes (1927) took elements from previous Norwegian and Swedish scholarship in his dissertation *Leidang og landskyld* (Leidang and Land Tax), where the retrogressive method was defined in narrower detail for the first time in Nowergian historical study (Holmsen 1942; Dahl 1990; Kjeldstadli 1992). The retrogressive method thus was used and developed before Andreas Holmsen in many ways perfected the method for use in studies of the development of the farm and application in a considerable number of local histories (Bull 1927; 1929; Bull et al. 1929; Steinnes 1929; Holmsen 1930; Steinnes 1930; 1932; Holmsen 1942; Steinnes 1953; Holmsen 1976). Holmsen (1977) has been a major influence on History students for generations through his textbook, first published in 1939. In keeping with the heritage of the 1814 generation, Holmsen (1942:32) declared that "structural geographical units are often quite autonomous and have their own particular nature, which are consistently greater the further back in time one goes" [translated]. In other words, the further back one goes, the higher the likelihood of continuity there is. Since the primary evidence is also steadily more slender the further back in time one goes, this allows one to draw conclusions from known historical situations concerning earlier periods which are otherwise difficult to shed any light upon. Thus the unknown prehistory becomes more like the better known and later prehistory.

Holmsen used the term 'structure' in his methodological article, but provided no references and does not indicate that he knew of Bloch's 'méthode régressive' either: in contrast he referred to a number of Norwegian scholars and one from Sweden (see also Imsen 2010, with refs.). Bloch's version was much more sophisticated in terms of source criticism; he emphasized, amongst other things, that to presuppose continuity is a serious error, and also that the structure of agriculture was the product of many small changes and a small number of revolutions (Bloch 1966:xxix–xxx). The germ of the peculiarly Norwegian version of the retrogressive method may indeed reside in Holmsen's lack of reference to Bloch.

In what may have been the most influential Norwegian archaeological work of this time, *Det norske folk i oldtiden* (The Norwegian People in Prehistory), Brøgger used the retrogressive method extensively without explicitly referring to it as such (Holmsen 1942:34). He did, however, explain on what basis the method could be employed: what, in other words, continuity relies upon. The initial premiss was a distinct Norwegianness, which nevertheless varies between the different economic zones: the psyche of the forest and fell is different from that of the coast and the sea (Brøgger 1925a:53, 170–1). The natural conditions in Norway, with marginal agricultural areas, meant that hunting, trapping and fishing were more important than in comparable agrarian regions, which in practical terms meant Sweden and Denmark. Norwegians were not unaffected by influences from European currents either, but those they adapted to the Norwegian circumstances.

The natural conditions, economic realities, and external influence also co-acted to form a distinctly Norwegian mentality, or as Brøgger himself (1925a:217) wrote: "… that right from the very first arrival of people in Norway, the natural constraints of the land were bound to make them Norwegian" [translated]. As he saw it, this mentality was necessarily conservative, and there have also been few changes to the agrarian settlement pattern from its foundation. Brøgger did not view Norway's prehistory in terms of different chronological periods but rather in terms of different subsistence strategies conforming with the ecological contexts and yet as almost unvarying adaptation through centuries if not millennia (Brøgger 1925a:28). He argued that the hunting and pastoral culture had an almost unbroken tradition from the Palaeolithic far into the Iron Age and in some cases as late as the 19th century. In caves and shelters in Vestlandet flint and schist tools have been found in association with pottery of the

Migration Period. For Brøgger, this indicated the use of flint and schist implements in the Migration Period and thus an extremely conservative material culture. He was quite clear that this interpretation was in conflict with the common Three Age System but did not regard that as important (1925a:44–5). Nowadays it seems obvious that the flint and schist are from much earlier times than the pottery. These finds are thus no argument for a conservative material culture of tools but rather for the recurrent use of the same site in different epochs.

In a speech marking *Universitetets Oldsaksamling*'s centenary, Brøgger viewed research into Iron-age settlements as an element in the basis for understanding contemporary farms (Brøgger 1930:14). An especially important factor in Brøgger's perception of the farm is that it was the product of colonization in (practically) unoccupied regions. As a result, the major primary settlement-phase farms — the primeval farms — could have been founded in the course of the Roman Iron Age and the Migration Period (Brøgger 1925a:211; 1925c:23–4). The Norwegian farm had a distinctive profile because extensive livestock farming was supplemented by cereal cultivation and hunting, in contrast to Denmark, for example, where arable farming was most important. The locally owned óðalland was the foundation of the farm, which in turn provided the basis for a democratic system (Opedal 1999:38–9). On the whole, Brøgger preserved the heritage of the 1814 generation in the Norwegian Historical School quite intact.

Eldrid Straume (1986) perceived the difference between Brøgger and the other leading archaeologist of this period, Haakon Shetelig, as being so clear that she distinguished Shetelig and Brøgger 'schools'. For the European Shetelig, it was external impulses that shaped the prehistory of Norway, even if the position and character of the land were undoubtedly significant. For Shetelig, too, Norway was a natural part of Scandinavia, and although Scandinavia was distinct in many respects, similarity with the remainder of Europe was also present (Shetelig 1925:2–3; Hagen 1970; Marstrander 1979; Helliksen 1996a).

Brøgger gave few bibliographical references in *Det norske folk i oldtiden*; *Sigd, ljå og snidill* or *Veid og vær* [The Norwegian People in Prehistory; Sickle, Scythe and Leaf-hook; Hunting and Weather] so that it is difficult to show what he was influenced by beyond Andreas M. Hansen's works, and in particular Hansen's (1904) *Landnåm i Norge* [The Primary Settlement of Norway]. Brøgger and Shetelig were, however, members of the Institute for Comparative Cultural Research, and must have discussed ideas there with both Norwegian and international scholars. In this light, it is interesting that the sociologist and ethnographer Marcel Mauss, the historian Marc Bloch, and the anthropologist Franz Boas — all of them in time hugely influential in their own fields — were connected with the Institute (Kyllingstad 2008:3). The international lecture series of the Institute were organized from 1925 onwards, the same year that Brøgger published *Det norske folk i Oldtiden* and Shetelig *Norges forhistorie* [The Prehistory of Norway]. Boaz declared that ethnological phenomena were the product of humans' physical and mental capabilities and their development under the influence of the physical contexts (Boaz 1974:63), and is reported later to have stated that "…I recognized the importance of studying the interaction between the organic and the inorganic, above all the relation between the life of a people and their physical environment" (Zumwalt 1988). Brøgger's view of history was clearly rooted in the Norwegian Historical School, but elements of his arguments were common with Boaz, Mauss and Bloch, who will certainly also have had some influence on his historical perspective (Andersen 1960; Helliksen 1996a; Østmo and Bergstøl 2004).

The links between French and Norwegian historians that were established through the Institute for Comparative Cultural Research were maintained after the Institute lost a greater part of its importance towards the end of the 1930s. Bloch eventually became a central figure in the *Annales* School, and his ideas progressed and developed in the direction of a history of cognition. The theoretical basis for retrospection was reinforced further by Fernand Braudel's work (1949). He viewed history as three processes of different duration, which operate partly in parallel and partly shape one another. He distinguished between individual events (short-term *événements*), conjunctions (medium-term *conjunctures*) and enduring structures (long-term; the *longue durée*). Braudel's ideas became current again in Norwegian farm scholarship in the 1990s, particularly, then, at the University of Bergen and its Vestland Farm Project (*Vestlandsgårdsprosjektet*).

Andreas Martin Hansen — race and settlement pattern

Andreas Martin Hansen was strongly committed to immigration as an explanatory model, a position that has recently gained a new relevance (Prescott 2012; Prescott and Glørstad 2012). Hansen, however, conformed to an ideological framework that

is unacceptable nowadays. His great influence on settlement archaeology generally, and on Brøgger in particular, has consequently largely been passed over in silence in the post-War era (although see Haavaldsen 1984; Hesjedal 2001; Pilø 2005:10). To make clear what Hansen's influence on research concerned with agrarian settlement in South Norway was, I have dedicated some space to his work even though he is now discredited within the academy. Hansen combined geology, toponymics, archaeology, history, anthropology and psychology. He died in 1932 but his books came to be printed as late as 1943. Nowadays he is perceived first and foremost as a representative of phrenology, the doctrine of a connexion between human capacities and characteristics and the outer form of the skull or the head. This branch of physical or biological anthropology has now been stripped of any scientific status, and the close links between phrenology and racial ideologies make it outdated in the extreme. Hansen was, as a result, rarely referred to following the Second World War and yet he was a recognized if still controversial scholar in his own time. He was an important quaternary geologist and demonstrated that the coastlines in the valley systems of Østlandet were the product of dammed up glacial lakes, and that the thickness of the ice caps during the Ice Age led to uneven land-rise. He became a State Research Fellow in 1908, in order to study the history and anthropology of the Norwegian population, and a member of the Christiania (Oslo) Scientific Society (*Videnskabsselskabet*) in 1910 (Kyllingstad 2004:58). He also used land-rise to date Stone-age settlement sites and distinguished between hunting rock carvings and farming rock carvings, while his theories concerning immigration and the Arctic Stone Age have been of major significance, directly and indirectly, in Norwegian archaeology. His conclusions concerning prehistoric settlement are still remarkably relevant in many areas, while in the preface to *Veid og* Vær, Brøgger (1925b) expressed his admiration of Hansen. Brøgger also (1917) emphasized Hansen's book *Landnåm i Norge* as the "clearest, the most deep and thorough, that has been written on our earliest settlement history" [translated]. This is in stark contrast to how it was received in historical circles. "It is at war with seven sciences," is what Gustav Storm is recorded as saying (translated, quoted from Bull 1920:63).

Hansen's starting point was that Norway had an original population of non-Aryans (*anariere*) who supported themselves by hunting, gathering and fishing, and that agriculture came to Norway with immigrant Aryans — the very opposite of the view of the Norwegian School and the 1814 generation. Hansen further believed (1899) that the two populations — or races — had different physical and mental capacities, and claimed, in line with the phrenology of his time, that there was a connexion between the form of the skull and mental characteristics. The original population was brachiocephalic or round-skulled, with the width of the skull being more than 80% of its length. The immigrant agrarian population was long-skulled or dolichocephalic, with the width of the skull being less than 80% of its length. There were other physical differences between the races too. The round-skulls were shorter, had rounder faced, wider noses and darker skin, hair and eyes than the Aryans. According to Hansen (1904:114), when the long-skulled Aryans arrived in the land around 1200 BC, they seized the good agricultural land and subordinated the round-skulls. The round-skulls are first found as slaves, and in later periods as dependent tenant farmers, coastal squatters, household hands and unfree farmers subject to the aristocracy of Vestlandet. The long-skulls conversely were conquerors and warriors, possessing their own extensive farms with no aristocratic overlay (Hansen 1899:70). The separate farm was the preferred settlement-type of the long-skulls, but this presupposed that the subordinate population was extremely weak or non-existent. This meant that the Aryans could settle in free, independent households, with each family colonizing its own land and no need to worry about defence. Where the hostile original population was numerous, by contrast, the Germanic folk sought mutual protection with one another, and settled in villages. Hansen saw relic traces of the villages, inter alia, in the clustered settlements. He thus assumed that there had been villages in Iron-age Norway long before any such was excavated for the first time at Forsand (Løken 2020). Hansen's assumption that an immigrant population introduced agriculture to what is now Norway is to a large extent supported by recent science. Christopher Prescott and Eva Walderhaug argue that agriculture involving livestock, cereal cultivation and two-aisled buildings were introduced to southern Vestlandet by an immigrant agrarian population (Prescott and Walderhaug 1995; see also Prescott 2012). Significantly, though, Prescott (2012) postulates a smaller group whose ideas were adopted by the extant population rather than a large-scale invasion. Although Hansen's conclusion is acceptable in this light, his mode of reasoning is unsustainable by contemporary standards.

After Brøgger

Most archaeological and historical settlement studies concerning Østlandet down to 2012 are heavily based on the retrogressive method, as Pilø has shown. Even after the revelation of incompatible evidence from Forsand (Løken and Særheim 1990), and Pilø's own refutation of the theoretical basis (2000; 2002; 2005), the method has continued to be used by several scholars (e.g. Iversen 1999; 2004; Øye 2005; Iversen et al. 2007; Hobæk 2008; Ødegaard 2010). Despite a growing quantity of excavations with quite contrary results, the retrogressive method and a belief in strong continuity in settlement is still firmly held to in both historical and archaeological circles (Salvesen 1982 with refs.; for an overview, see Pilø 2005:10–16; Orning 2006). Like Pilø, I wish to draw attention to the fact that Jørn Sandnes (2000:205), one of the leading exponents of the Norwegian deserted farm project, wrote "Quite generally, I would otherwise assert that one can learn more about the old Norwegian clan system by reading, for instance, *Juvikfolket* [The People of Juvik] by Olav Duun or *Gamalt or Sætesdal* [Heritage from Sætesdal] by Johannes Skar than by studying American ethographers' and anthropologists' theories, often based upon non-European, primitive cultures" [translated]. Knut Helle (2009), another Nestor of History in Norway, followed up the critique of the use of social anthropological models in his article 'Den primitivistiske vendingen i norsk middelalder forskning' [The primitivistic turn in Norwegian medieval research]. Helle's primary objective was in fact to emphasize the necessity of strict source criticism. He nonetheless reveals preferences for continuity and the retrogressive method, and little faith that social anthropological models can offer much new. The primitivistic turn is also often alluded to as the anthropological turn, and both the usages and the argumentation can be recognized in much earlier debates in Norwegian archaeology (see, e.g., the discussion following Odner's article in *Norwegian Archaeological Review* 1974).

While Norwegian settlement research was long integrated within Scandinavian and European practice, possibly even in the vanguard at times, it became more isolated in the post-War period down to the 1980s. Petersen's (1954), Odmund Møllerop's (1958) and Wencke Slomann's (1971) works are examples of settlement archaeology that embedded continuity as its underlying premiss. Oddmunn Farbregd's (1983; 1984) and Håkon Hovstad's (1979; 1980) analyses of farm boundaries can to some extent be said to presuppose continuity even if the historian Hovstad, in particular, saw the Viking Period as the earlier limit of such a sequence. In processually inspired studies too, continuity appears as a basic premiss, for instance in Harald Jacobsen's (1984) thesis on Iron-age settlement in Ringerike, Ellen Anne Pedersen's (1989) thesis on Hadeland, or Birgitta Wik's (1982) study of Trøndelag. Likewise in the multi-disciplinary *Vestlandsgårdsprosjekt*, based at the University of Bergen, it was assumed that modern farm boundaries have very long histories, even though the project was open to changes in how they were operated (Julshamn et al. 2002:18–19).

THE CRITIQUE OF THE RETROGRESSIVE METHOD AND THE CONTINUITY MINDSET

The retrogressive method, as already noted, involves drawing conclusions from historically known situations regarding earlier times. In archaeological studies, especial significance is attached to grave monuments, stray finds and historical evidence in preference to direct settlement evidence in the form of prehistoric buildings (Pilø 2000; 2005). This may be due to the fact that, until recently, relatively few prehistoric buildings were known, although there is also a clear tendency for the few that were known to be neglected in studies of settlement history (as Eriksen 2019 has shown in respect of the Late Iron Age). Settlement is commonly studied at widely varying scales. In a micro-perspective, details of the construction of the building are examined; at a macro-level it is the long lines or enduring structures that are considered. The latter have gradually turned into an argument in favour of the retrogressive method (Iversen 2004). Both the method and the evidence mean that only stable components are illuminated at a macro-level. Additionally, conclusions are drawn from the macro-level to the micro-level, and studies based upon the retrogressive method have, as a result, developed no interest in detailed analyses appropriate to the exploration of the micro-level. Archaeological settlement material that does not fit a hypothesized straight line from known present-day settlement structures back to those of prehistory is commonly ignored or explained in an ad hoc manner (Pilø 2005:14). The Norwegian variant of the retrogressive method was used by Norwegian researchers in the Nordic Deserted Farm Project. The employment of this method was, however, strongly criticised by Swedish and Danish historians (Gissel 1976; Österberg 1977; Porsmose 1982; Salvesen 1982) but that critique was largely ignored (Sandnes 1978:18; 2000). The reason why this method has shaped Norwegian historical — and I would add, archaeological — scholarship is, in

the words of Jørn Sandnes (1978:17, translated), "the great stability and continuity which characterized the Norwegian farming community and farm history in earlier times." Once more, the inheritance from the 1814 generation and the Norwegian Historical School is manifest. Ole-Jørgen Benedictow (1992:42) has pointed out that the rejection of the critique bordered on sectarianism, while in one case reference was made to non-existent documents to refute it.

The many retrogressive studies consistently avoid the areas on Jæren with so many recorded farmsteads. It is typical that Brøgger mentions the excavated building remains on Jæren only in passing, while Shetelig (1925:169) treated them solely as houses not as part of the farm. On Jæren, it is clear that the historically known farm boundaries often cut across the settlements or through stone walls of the Early Iron Age. Hovstad (1980) quite logically, therefore, concluded that the boundaries cannot be traced back ealier than the Viking Period. Little attention has been paid to these observations, however (e.g. Skre 1996; Iversen 2004; Ødegaard 2010).

The retrogressive method is also employed in the rest of Scandinavia, although without the Norwegian variant's trust in continuity. The distinctively Norwegian application of the method is particularly striking when one compares it with, for instance, the Swedish approach. "When Swedish geographers use the retrogressive method it is done to find differences between the 18th-/19th-century landscape and earlier landscapes — not to demonstrate continuity" (Widgren 2000:42, translated). The retrogressive method was likewise used in connexion with empirical evidence to refute Meitzen's ideas of continuity in German scholarship in the 1950s, not to reveal continuity (Widgren 2000:41).

As noted, nation-building was at the core of much of Norwegian settlement scholarship (Opedal 1999). The young state needed a prehistory which made it different from the neighbouring Denmark and Sweden. Norway's landowning and free farmers provided just such a premiss, that has both underlain the narratives and been reinforced by them (Skre 1998:23–7). At the same time, it was important to emphasize that the young nation had ties back in time to a rich and proud prehistory. One consequence of these factors has been the centrality of the retrogressive method. That method can only be used to study a phenomenon where there is a certain continuity, and is difficult to use to identify breaks if the source material is slender. The farming settlements were thus assumed to have remained on the same spot since the farm was founded or its lands were marked out. Earlier buildings should not be found, therefore, because the theory dictated that they should be lying underneath the building of the next generation. An unexpressed and unanswered question in Norwegian settlement history, as a result, is "Why do we find housing of the Iron Age at all, then?" This question can itself be split into two. To begin with, why did the settlement that we do find come into being? And then, why was it abandoned? Initially, our answers were usually that the farms excavated were marginal, and only in use when the population level was higher than normal. They were not real farms, therefore, but marginal smallholdings. Gradually, as an increasing number of abandoned farmsteads were revealed in what are now the central agrarian areas with no direct links to contemporary farms, the answers changed a bit, and the division of farms was introduced as an explanatory element (Iversen 2013).

Most recently, historians have propounded a fundamental critique of the scope for using the retrogressive method. Amongst other things, it has been pointed out that stability in property conditions has been much lower than assumed by core retrogressive analyses (Weidling 2003; Dybdahl 2008; Weidling 2008). The medieval concept of ownership was essentially different and more complex than that of the present day, while property relations cannot be separated from relations of political power (Dørum 1994; Iversen 2001). All the same, a virtually ahistorical use of the concept of property remains at the base of the retrogressive analyses (Iversen 2001:79–82). In pre-state societies there is no central authority that preserves a hypothetical property right; in practice, therefore, the right to property is just as strong or weak as the level of self-defence the landowner can demonstrate. Unbreachable boundaries such as those described in the medieval laws were thus more ideal than real. In the 14th century there was a string of disputes between the Church and farmers over ownership. These disputes have crucial implications. In the first place, the Church was introducing a new form of property right; secondly, the Church was gradually establishing the principle that written documentation of property carried more weight than oral testimony (Iversen 1996; Emanuelsson 2005; Orning 2006). The Church was thus introducing both a new form of property right and a new mode of recording it. It would appear logical that the Church benefited from the new approach, and so also that property relations themselves were fundamentally altered as late as in the 14th century. Several archaeologists, in critical studies, have recently pointed out how the retrogressive method, and to a certain extent continuity

scholarship, have exercised influence beyond settlement studies too, and still influence interpretations of both individual objects and of society as a whole (Axelsen 2012; Berg 2013).

ALTERNATIVE VOICES

Although the idea of continuity has been a starting point for much archaeological research, Norwegian archaeology has not been lacking in alternative views. I would draw particular attention to Harald Egenæs Lund, Ottar Rønneseth, Knut Odner and Bjørn Myhre. In 1936, Harald Egenæs Lund (1937) excavated a barrow of the Late Roman Iron Age and discovered hearths, post-holes, charred birch bark, charred twigs and daub underneath the mound. In the post-holes were preserved remains of roof-bearing posts of oak that were rectangular in cross-section. The daub and charred twigs meant, for Lund, that the walls had been woven withies of alder 10–25 mm in diameter, plastered with clay 20–40 mm thick on both sides, while he supposed the bark to have come from the roof. He inferred that these were the remains of a small building, measuring about 3x3 m. Whether that interpretation is correct or the remains were part of a larger building is of minor relevance here. It is, conversely, a matter of huge interest that on the basis of this discovery he concluded that buildings with daub-covered walls and wall posts supporting the roof must also have been found in Rogaland. From the evolutionary perspective of that time, he concluded that buildings without wall banks must have been earlier than the then known Rogaland buildings which had such banks. He also supposed that buildings of the Bronze Age without wall banks must surely be found in Rogaland (Lund 1937; 1939). Such buildings were first discovered considerably later, through Egil Bakka's excavations in Sunnmøre, and in Rogaland after the introduction of open-area stripping (Johnson and Prescott 1993; Løken et al. 1996). Lund's deductions stimulated a dawning awareness of a more complex settlement history, but his ideas were largely ignored.

In the post-War period too, certain studies stand out. Anders Hagen (1953) did not take continuity for granted, and would emphasize rather the importance of considering the farm as a component of its contemporary context. Social anthropological models were applied to understandings of settlement somewhat later, initially by Knut Odner (1969; 1973; 1978) and subsequently by Bjørn Myhre (1978) and Bjørn Ringstad (1992). Myhre (1973a; 1974; 1978) saw a break in settlement around AD 200, a further break around AD 500 and a final break around AD 1350. In the context of Scandinavia, cultural geographical methods were applied to evidence from Jæren quite early on by Ottar Rønneseth (1966; 1974). Of these four scholars, however, it was Myhre who could be said to have had a crucial influence on Norwegian settlement scholarship. Rønneseth was subject to severe criticism (Myhre 1966) and was largely ignored, a situation which may partly be due to personal considerations. Although this is unmentioned by Myhre, it may be down to the fact that Rønneseth was on the 'wrong side' during the Second World War, took his doctorate at a German university, had the German Herbert Jankuhn as supervisor, and published his thesis in German (Stylegar 2001:9–10; Solberg 2014:617). All of these together were not well received so soon after the Second World War. Jankuhn had been a member of the SS, a key figure in Himmler's cultural and propaganda organization *Ahnenerbe*, and denounced Professor Brøgger to the German political authorities and so contributed to Brøgger being arrested (Hagen 1986:269). It is, perhaps, typical that it was a Swede who noticed the potential of Lund's evidence (Stenberger 1953:58). New and foreign ideas appear to have been linked to individual scholars whom one preferred to distance oneself from. Explicit opposition to foreign and innovative lines of thought appear to have afflicted Odner's use of social anthropological models: the vocabulary is rather harsh and hostile (see 'Discussions' in *Norwegian Archaeological Review* 1974 7(2); especially Blindheim 1974). Myhre, however, gained great influence over settlement archaeology in Norway, and is frequently cited — although in my own view, this is the case to a large extent in relation to those of this works which rely less upon social anthropological models (Myhre 1972; 1980; Gjærder et al. 1982; Myhre 1982; 1983; 2002). Myhre's use (1978) of the chieftainship model and identification of the breaks in settlement is one of the few Norwegian studies concerning settlement history that have had an international impact more recently (although not the only one: also Skre 1998).

In the 1990s, landscape analyses inspired from Swedish Cultural Geography and English theoretical trends provided a new view of the agrarian landscape, and indirectly of settlement. Some important studies of this kind are discussed in greater detail along with settlement research around the Oslofjord (Ch. 3.5). Trond Løken, moreover, in connexion with the excavations at Forsandmoen, has both directly and indirectly pointed out a number of aspects of the settlement there that are to some extent in conflict with the idea of continuity, and in part are of such a

character that they could not be revealed by the retrogressive method (Løken and Særheim 1990; Løken 1991; 2001b; 2020) — albeit without having had a fundamental impact on the continuity mind-set. Siv Kristoffersen (1993) has interpreted Modvo in Sogn as part of a model labelled a 'decentralized farm structure'. Although certain principles from continuity scholarship can still be recognized — for instance that burial mounds are linked to the marking of property rights (Kristoffersen 1993:201) — she lays the ground for an approach to understanding Iron-age structures in the mountainous areas in terms of the social, economic and political systems they belonged to while they cannot be uncritically interpreted in terms of the historically known shieling system (Kristoffersen 1993:199). Kjetil Skare (1999) was as far as I am aware the first Norwegian archaeologist who, starting from Anthony Giddens's structuration theory, discussed social conditions using buildings as his primary evidence. Amongst other things, he stressed that building practice in the Roman Iron Age and Migration Period in Rogaland reveals changes moving in the direction of more durable property holding. Most recently, several scholars have been critical of continuity scholarship, and have explored topics that lie outside the bounds of traditional settlement research. In this way, they have contributed to the understanding of issues which cannot be investigated through the retrogressive method, and the results are, in part at least, incompatible with continuity scholarship. Marianne Hem Eriksen (2019) studied the interwoven relationship between architecture, social praxis and the conceptual world, and has contributed much to an understanding of the farm of the Late Iron Age in terms of prehistory's suppositions. Two related studies show that there may have been conscious ideas behind the abandonment of a farm and these demonstrate, therefore, what is lacking from continuity scholarship (Amundsen 2013; Amundsen and Fredriksen 2014).

FARM RESEARCH INITIATIVES

Several initiatives or programmes concerned with research into the farm have either been proposed or partly carried through since Shetelig's first publication of a prehistoric building ruin in Norway around a century ago. The key role of the Institute for Comparative Cultural Research in the many excavations of prehistoric buildings and farms is relatively little known (see, however, Opedal 1999). In 1928, after extended informal discussions, the Institute agreed "to launch comparative investigations into the patterns of development of the farming society as part of its programme" (Stang 1931:113, translated; note the evolutionist premiss), and this was known as the Farming Society Programme. Both Shetelig and Brøgger were associates of the Institute, and very familiar with the discussions, while they also probably knew that the Institute was quite well funded. The Institute had fully 160,000 kroner at its disposal annually from 1925–30: a significant sum at the time when the average annual income was around 2,500 kroner (http://www.ssb.no/histstat/aarbok/ht-0901-lonn.html) (Amundsen 1972:131; Kyllingstad 2008:3, 478, 523). Thus the Institute had an academic programme which archaeological questions matched nicely, as well as having substantial reserves to make use of. It can hardly be coincidental, then, that the first meeting of archaeologists at Universitetets Oldsaksamling in Oslo in 1927 discussed a similar programme of work (Mowinckel 1928:91–8, 104–5). Altogether eight fields of research were presented, and four were prioritized. The building remains panel, comprising Shetelig and Grieg as well as Petersen in the chair, proposed four topics for further investigation:

I. To supplement the portrayal of culture with daily practical objects from the building ruins and the study of the form of the structure itself.
II. The style and form of the settlements in different periods.
III. Population trends; the questions of depopulation, emigration and new settlement.
IV. Connexions with place-names.

The work was to be organized through the individual museums and carried out by individual specialists, albeit using common methods and in close cooperation. The goal was a collected, edited publication with contributions from the various archaeologists. No collected publication ever appeared but a number of major and minor individual contributions did, of which Petersen's (1933; 1936) and Grieg's (1934; 1938) are the best known. In 1943, along with Shetelig, Brøgger drew up a new programme in which, amongst other things, the farm as a sociological and economic unit would be a focus of research (Brøgger 1943; Hagen 2005). Once again there was a close relationship to the Farming Society Programme of the Institute, although the latter now had much less funds available. It ceased financing archaeological work around the same time as the archaeological farm excavations began (Kyllingstad 2008:589). The efforts of the Institute were directed more and more towards the collection of data concerning the existing cadastral

farm. At the same time, the German occupation put barriers in the way of research. The programme was taken up again after the War, but the research was then funded through Norway's General Scientific Research Council (*Norges allmenvitenskapelige forskningsråd*: NAVF). Bjørn Hougen's *Fra sæter til gård* (1947) [From Sheiling to Farm] and Anders Hagen's excavations at Sostelid are the most familiar outputs of this archaeological initiative. In 1950, it was noted in Universitetets Oldsaksamling's *Årsberetning* (Annual Report) that the topic was to be taken up again over its full range, with NAVF providing funding. The money was used, amongst other things, for recording, which was later to be followed up by excavations — but the latter did not come about to any great extent (*Universitetets Oldsaksamlings Årbok* 1951:188–9; Hagen 2005).

Anders Hagen's excavations at Sostelid, Åseral, Aust-Agder, of 1946–49 were clearly inspired by Shetelig's, Grieg's, Petersen's and Gjessing's investigations of building ruins in southern Vestlandet. Hagen discovered little new about the buildings themselves, however, and in many respects these excavations closed this phase. Those investigations of the complete farm unit with fields, walls and graves, and the doctoral thesis which followed, did point the way forward nonetheless. Hagen emphasized that a holistic and detailed understanding of the farm, covering building-types, the use of the fields and the livestock, were fundamental to understanding social and economic conditions of settlement and farm history. Perhaps his most important contribution was that he set the farm in a wider European context on a broad empirical basis, and rejected Brøgger's idea of the farm as the product of some distinctively Norwegian sequence of development (Hagen 1953:11, 113, 118, 196–7). Both Graham Clarke and Gordon Childe were referred to in the dissertation, and Hagen later emphasized (1997:229) how stimulating Childe was in a period of Norwegian archaeology when style history was more important than those social and economic matters that he was immediately interested in.

After that, there was a long period with no major, national initiatives concerned with Iron-age settlement, even though important regional research projects such as the Ullandhaug excavations, the Forsand excavations, the Åker Project and the Vestland Farm Project were carried out (Myhre 1980; Løken 2020; Pilø 2005; Øye 2012). More recently, a new national initiative has been launched. On the basis of St. meld. nr. 15 (2007–2008) Tingenes Tale—Universitetsmuseene [Parliamentary Report no. 15: What Things Say—The University Museums], in 2009 Håkon Glørstad submitted an application to the Norwegian Research Council that was subsequently successful. The application was concerned with 'Research in Partnership' (*Forskning i Felleskap*), under which the five university museums would work together (Sak nr. 2008/16036 in the Museum of Cultural History's Archive). In the application, the need for research into the mass of material that had been produced by heritage management excavations was underscored, and the dissertation which this book is based upon can consequently be seen as part of a wider national initiative.

THE HISTORICAL NATIONAL RESEARCH FRAMEWORK

The deserted farms of the Roman Iron Age in southern Vestlandet had permanent manured fields, farmsteads comprising houses and byres, droveways from the byre out to the pastures, and several other features which have led many to see similarities between the Roman Iron-age farm and the historically known cadastral farm. The similarities have made it easy to focus upon the economic aspects of the Roman Iron-age farm and the prehistoric farm generally. There are also similarities in the most basic tools of the Iron-age famer and his 19th-century counterpart (sickle, scythes, leaf knife, axe, spade, ard and subsequently the plough), domesticated livestock (cattle, sheep, goats, horse and pigs), and varieties of cereal (oats, wheat and barley). Because the farmer's working environment in these two periods had clear similarities, it has implicitly been assumed that the farmer's motivation and reasoning were also alike.

The rational farm or the ethnographic pitfall

In such a conceptual framework, the farm is treated first and foremost as rational mode of organizing agriculture. The famer is presented as a rational economic agent and the highest possible level of production is assumed to be the farmer's only, or at least definitely his most important, motive. In such a conceptual framework, which Janken Myrdal (2013) has labelled the 'ethnographic pitfall', meaning that everything resembles the 19th century, it becomes quite automatic that property relations are also like those of the 19th century. Greater population pressure, apparently an important driver of change, leads, in this perspective, not to change or restructuration of the existing settlement but to the establishment of new, marginal farms, which are abandoned when the pressure falls. It may be noted in addition that the population level, in

broad terms, is calculated on the basis of the number of deserted or newly established farms, so the likelihood of circular argumentation is high (cf. Myhre 1982; 1983). It is therefore, so the argument goes, only new technology which brings about changes in the working methods and farm structure. Since the technology is well known and changes very little after the introduction of iron, there was no reason to change the working practices or farm structure. Although it is true that the working methods of the Early Iron Age and those of the modern period have much in common, the farm structures were not necessarily so similar (Mjærum 2012b).

THE CADASTRAL FARM — UNIT OF DOCUMENTATION OR UNIT OF ANALYSIS?

I have already demonstrated that belief in the stability and continuity of the farm has been strong and pointed out that open-area excavation was introduced to Norway quite late (Ch. 2), maybe precisely because of the powerful belief in stability. Even though some works have sought to challenge the view of the farm as a stable unit, these have had little success until recently (Grønnesby 2019). In my view, one of the reasons for that is that the cadastral farm is used as a unit of documentation in Norwegian archaeology, and that the unit of documentation is rather uncritically used as a unit of analysis in research. Maps that recorded ancient monuments were not produced for a long time (Skjelsvik 1978). Mapping at a scale that supports the plotting of ancient monuments, which means at a scale of 1:5000 or even greater, was fully introduced only in 2002, even if central areas were to a great extent mapped in the course of the 1970s. As a result, the ancient monuments were located using farm and holding numbers and then according to ecclesiastical parishes, something which made it possible to gain an overview of the ancient monuments and finds, initially for researchers and gradually for heritage management too. To begin with, the connexion between the historical cadastral farm and the prehistoric remains found there was taken for granted and not subject to discussion. The cadastral farm was thus the automatic unit of analysis. When better maps became available they were used primarily to illuminate the connexion between known farm boundaries and topographical contexts (e.g. Hovstad 1979; 1980; Farbregd 1983; 1984), and so little used to challenge the perception of the cadastral farm as a geographical unit with its roots in prehistory (but cf. Myhre 1972; 1973a). Even in works with a critical stance towards continuity, the cadastral farm is used to some extent as the unit of analysis (Pilø 2005). In Skre's work (1998), where the influence from European approaches is clear, the cadastral farm continues to be the fundamental unit of analysis. In the Bergen school too, the cadastral farm was the basic unit of analysis, even though several of the works emanating from Bergen point out that an objective was to investigate the age and origins of the farm. When the cadastral farm is the primary unit of analysis it is difficult to challenge the perception of continuity. Virtually all ancient monuments associable with agriculture are sited on a cadastral farm and it appears to be an underlying and sometimes unexpressed premiss that the age of these agrarian ancient monuments determines the age of the farm.

The farm-names

Along with the revision of Norway's 'cadaster' [*matrikkel*] or property register, a Commission for the Revision of the Names in the Cadaster involving Oluf Rygh and others collected information on farm-names, first and foremost in order to standardize spellings. The Commission set about its task with thoroughness and collected not only the earliest known name-forms but also the contemporary pronunciation of the names, and thus gained commendable insight into the historical sources for each individual farm. In the context of the printing of the collected data in *Norske gaardnavne* vols. I–XVIII Rygh had also wanted to explain what the farm-names meant and to assess their age (Rygh 1898:vii–xiv, 7–10). Rygh (1898:4–8) had a clear and explicit view on the relationship between farms and farm-names, and pointed out that farm-names could disappear, new farms could emerge, and that farms could change their names. This source evaluation is much less evident in Olsen's influential Ættegård og helligdom (1926) [Ancestral Farm and Sanctuary], which in many ways set the standard for the perception of the relationship between farm-names and farms even though Olsen himself (1939) was clear about the source-critical problems.

Farm-names are still used to date cadastral farms in some cases. Certain classes of names, such as farm-names that end in *-land*, are essentially dated by means of archaeology (Brink 1984; Vikstrand 2013:28–9), while historians have also concurred in allowing the datings of toponymic types to follow suit when archaeological finds mean that the antiquity of settlement is extended (see 'Diskusjon' in Salvesen 1990:32). Archaeological finds date the

name-sets, in other words, and those in turn date the cadastral farm, and the farm-names are then used to corroborate the fact that the archaeological finds do correctly date the cadastral farm. The relationship between archaeological and place-name scholarship can thus be characterized as "a scientific circle with vague contours" (Gräslund 2010:46, translated). What is much needed at present, therefore, is a critical investigation of the relationship between farm-names, farm boundaries and archaeological finds. This is a key reason why farm-names are not included within the present study.

SETTLEMENT RESEARCH AROUND THE OSLOFJORD

I have outlined the national research history up to this point, and to conclude I shall look more closely at previous research into evidence from the specific area under scrutiny in this study. The history of settlement in Østlandet has also to a large extent been written on the basis of indirect settlement evidence (Pilø 2005) and very much in accordance with the national framework sketched above. This is, in part at least, due to the fact that until quite recently few settlements of the Iron Age had been investigated around the Oslofjord (Østmo 1991; Løken 1998a; Bårdseth 2006; 2008; Martens 2007; Gjerpe ed. 2008). However in Østlandet too, three-aisled buildings with earth-fast posts and no wall banks of the Bronze and Iron Ages had been discovered by the 1970s (Løken 1978) and the early 1980s (Haavaldsen 1983). These had little impact on settlement history, even though Trond Løken (1978), even before the end of the 1970s, suggested that settlement in Østlandet had aspects in common with the situation in Sweden and Denmark; that observation did not really leave much of an impression.

In Pedersen's works (1990a; 1990b; 1999), Lil Gustafson's (1995), Gro Jerpåsen's (1996) and Ingunn Holm's (1995; 2004), the principal sources of evidence were remains of cultivation. Their studies can be seen as the first attempts to break free of the idea of continuity in relation to Østlandet (for an introduction to the models of the 1980s and 1990s and the debate around farm continuity or mobile farmers in Østlandet, see Henriksen 1994; 1995; Løken 1998a). The methods, especially those of Pedersen and Jerpåsen, were inspired by Swedish Cultural Geography. The methodology and source material produced results which do stand apart to a certain extent, not least with the reduced attention to continuity.

Pedersen's studies concerned with the investigations of fossil remains of cultivation at Hørdalsåsen in Sandefjord, Vestfold (1990a; 1990b; 1999), are based, *inter alia*, on Stefan Höglin's thorough recording in the style of Swedish cultural geographical methods. She finds evidence of extensive farming to start with and subsequently intensive cultivation in the Early Iron Age. Her results have since been nuanced and corrected through excavations and detailed archaeometric analysis, but the main lines remain valid (Mjærum 2012a). Jerpåsen (1996) combined the landscape analysis of a major area with detailed studies of cultivation evidence in a smaller region. She perceived the landscape as a process, and emphasized that even though earlier structures often lay out a pathway for later ones, sudden events can cause breaks in the pattern. She also added subtlety in relation to several different elements of the landscape (1996:14). Thus she did not presuppose continuity, and also found a number of breaks in the use of the landscape in her landscape analysis. Both Pedersen and Jerpåsen examined areas within the central agricultural lands which, however, have not been cultivated since the mechanization of agriculture. Ingunn Holm (1995) compared the picture which is produced by studies of the traditional sources for settlement history (burial monuments, stray finds and place-/farm-names) with the picture that comes from studies of clearance-cairn fields. She found that the conventional settlement history produced too narrow a view of settlement (1995:58). Here, we see the first steps taken towards a critique of the idea of continuity.

There are also some studies which made use of the buildings which from 1990 onwards were excavated with the aid of mechanical area stripping. Skre (1998) was the first to use building ruins as a source in a Norwegian doctoral thesis. He emphasized that not all farms were run by independent farmers but often by subordinates in a sort of patron-client relationship. At the same time, he held a more traditional view of continuity, with the cadastral farm as the fundamental unit of analysis and funerary monuments as important sources. In this way, the social situations become comparable with those of Denmark and Sweden while the settlement pattern itself remains relatively distinctly Norwegian. This perspective is also maintained after a fashion in *Østfolds historie* (Stylegar and Norseng 2003). As late as the end of the 1990s the debate around stable farm settlement versus mobile farmers died away (Pedersen 1999). In its place, the discussion turned rather to the forms of settlement and the degree and form of stability. This came about, indeed, because of the large number of building ruins that

were progressively found both in what are now the central and in the marginal agrarian regions, together with the quantity of macro-fossil analyses which yield details of agriculture such as what sort of cereal was being cultivated and what weeds it was associated with. In several works, Torgrim Guttormsen has made use of buildings uncovered by open-area stripping at Moer in Ås. One of his articles was strongly inspired by English landscape studies (Guttormsen 2002). In a more popular scientific article, he pointed out how the evidence just cannot be reconciled with Trygve Vik's detailed studies representing the primeval farm tradition (Guttormsen 1998). He discussed the problems concerning the dating and interpretation of buildings, farm boundaries and farm-names, and the possibility of a central place associated with well-recorded buildings at Veien, Ringerike, Buskerud (Guttormsen 2000; 2001; 2005a; 2016). Pilø (2005) has used Iron-age building ruins primarily to show that the primary settlement-site evidence is incongruent with the dominant idea of continuity. His thesis was first and foremost a critique of the primeval farm model and the peculiarly Norwegian application of the retrogressive method. In the remainder of this work, I draw inspiration both from the alternative voices (Ch. 3.2.1) and Pilø's critique, and aim to find an approach which allows me to research both continuity and breaks in settlement — or maybe every possible grade of continuity and discontinuity.

SUMMARY

Empirical archaeology is often used by researchers long after the theoretical and methodological approaches the data originally pertained to have become obsolete. At the same time, new empirical evidence in settlement research has had extraordinarily little impact on settlement studies. It looks as if the excavation and publication of building ruins became more or less an end in itself. As we have seen, the history of settlement has commonly been written on the basis of archaeological evidence other than the primary settlement material (the buildings), and grave finds have been particularly significant. Influential Norwegian archaeologists' and historians' choices of theory and methods in settlement scholarship in the post-War period differ ever more clearly from those of other Scandinavian specialists. Simultaneously, the situation in respect of sources has been different, a state of affairs which again, in part at least, is due to the faith in continuity in Norwegian settlement scholarship. Norwegian settlement research may thus to a certain extent be said to have been trapped in a hermeneutic circle to which continuity and Norwegian distinctiveness have been key premisses. The premisses have not been affected by apparently contrary data; rather, those data have been explained away ad hoc or neglected. The result is that in certain key areas Nowegian settlement history diverges from that of the remainder of Scandinavia.

It is difficult to divide settlement scholarship up into chronological periods. The widely used partition into cultural history (1900–60), processual (1960–80) and post-processual archaeologies (1980–present) (e.g. Olsen 1997:29–72) is not, in my opinion, at all productive in understanding how continuity has held its ground as a premiss. The belief in continuity and various forms of retrospection have remained dominant since the 19th century even if the explicitly retrogressive method was first developed and clearly articulated at the end of the 1920s. It was initially rather implicitly assumed that the cadastral farm could be traced back to prehistory, and more weight was progressively attached to arguing for such continuity, as one of the bases for the specifically Norwegian application of this method. The free, independently land-holding Norwegian farmer has, as we have seen, been a crucial foundation stone of the construction of the nation, at least from the 19th century and far into the 20th. It was supposed that the owner and the operator of the farm were one and the same person, although he [*sic*] might have had slaves as part of the farm's property. In the 1990s, the view of the farmer as a free landholder was challenged, and this no longer predominates in settlement archaeology. The focus has turned instead to the residence of the lord and collections of farms (Iversen 1997; Skre 1998; Iversen 1999; 2004). The specifically Norwegian version of the retrogressive method has also been subjected to severe criticism (Pilø 2000; 2005; Widgren 2000). This criticism has been largely ignored rather than countered or considered. Parallel with this principal current of development, individual researchers have been influenced from other quarters, and particularly by social anthropological models or cultural geographical methods (Rønneseth 1966; Odner 1969; 1973; Rønneseth 1974; Myhre 1978; Ringstad 1992).

I have not done much to place settlement scholarship in the context of much more general trends in the history of ideas and philosophy. This is because for the most part such ideas reach Archaeology only via other disciplines, while in the present context it would be too much of a digression to give an account of those currents. It is nonetheless clear that certain fundamental modes of thought, such as romanticism, national romanticism, nationalism, Scandinavianism

and perhaps above all evolutionism have played crucial roles in the shaping and maturation of the Norwegian method in settlement scholarship. In this context, what is more interesting to me than how those ideas themselves emerged, or what impact they had on society more widely, is how the ideas have left their mark on Archaeology. I have shown how a peculiarly Norwegian research tradition was born of a belief in the Norwegian people's particular conservatism and the use of a distinctly Norwegian version of the retrogressive method that was developed to make use of conservatism. Alongside that, known prehistoric farmsteads were defined as marginal and so could largely be ignored in research into the settlement patterns. Although several archaeologists have identified aspects of the archaeological evidence which have a poor fit with continuity scholarship, their observations have been widely ignored.