# Computational modelling of the coastal Mesolithic in south-eastern Norway

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# Chapter 1

# Introduction

One way to conceive of scientific inquiry is as a form of strategy by which we try to confront theoretical constructs with empirical observation, aimed at aligning our beliefs as reliably as possible with what is true (Godfrey-Smith 2003, 161). A lot remains to be unpacked from this sentence. However, for now it is enough to note that the empirical side of this equation is a critical point for archaeology, as the fragmented and uncertain nature of the archaeological record means that there will always be a multitude of possible explanations that could account for any observed empirical pattern. Reducing this number of candidate explanations is first and foremost dependent on data, which in the case of archaeology are scarce.

Establishing true explanations of a past social reality is at best exceedingly difficult, perhaps impossible, and must be the result of cumulative and recursive efforts from entire research communities over time – it is not achieved by individual researchers. Accepting this social and cumulative nature of archaeological inquiry means that one can adopt a strategy to try to make one's research as open and amenable to scrutiny, extension, criticism and alternative approaches as possible. While easier said than done, an attempt at adopting such a strategy is done here.

To accommodate the above points, this thesis aims to stringently explore and contrast empirical trends that have been deemed of importance for understanding past hunter-fisher-gatherer societies, drawing on the extensive material from the coastal Mesolithic of south-eastern Norway. Based on this, the project aims to culminate with the generation and presentation of some hypotheses concerning possible casual drivers behind the observed patterns. Exploration and stringency are explicitly voiced here for a couple of reasons. Exploration concerns an aspiration to approach the material with a degree of secession from previous hypotheses concerning the societal developments in the period, and to instead have observed empirical trends dictate the hypotheses presented. This can facilitate a freer investigation, transformation and combination of empirical patterns, as it reduces the risk of forcing the treatment of empirical patterns, consciously or not, towards a single explanation or end-goal. However, a complete break with previous beliefs is clearly neither possible nor desirable. For one these will in part have dictated how the material under study has been retrieved and recorded, how I will approach it, and is necessary for it be possible to contextualise and make sense of any observed patterns. Stringency, and with it transparency of the analytical choices made, will make it easier for both me and others to follow the logic of the arguments presented, and make it easier to identify when and in what ways prior beliefs might have led an interpretation astray.

As in many other areas of the world, the last few decades have seen a dramatic increase in the material generated by Norwegian archaeology. In terms of sheer number of sites and associated data, this is most marked for the coastal Stone Age material (e.g. Bergsvik et al. 2020; Damlien et al. 2021). Given that this increase in material is achieved on the back of public spending, it is arguably a disciplinary obligation to utilise this data for research purposes. While there are many possible arguments for why archaeology is worthwhile at all, some more vague than others, the economic burden of archaeological practice is clearly easier to justify if the data we generate also informs the research we do. However, getting even a basic overview of this now vast material necessitates the use of quantitative and computational methods designed to handle, describe, explore, present, summarise and infer from such quantities of data. Following some early optimism in the 60s and 70s, such methods have, until recently, seen sporadic and relatively limited application for research purposes in Norwegian archaeology.

Quantification offers standardisation and simplification, and by extension scalability and comparability. As with all disciplines concerned with the complexity of social life, whether past or present, archaeology also benefits from multiple perspectives that move between the nuance of particularities and the general trends illuminated by aggregated analysis. I would argue that the latter is at present still underdeveloped in Norwegian archaeology. With renewed and ongoing enthusiasm for such approaches, it is important that this is combined with a continually critical view of the answers these approaches can provide, and those which they cannot.

The great disciplinary benefit of archaeology, as compared to other disciplines concerned with the study of human societies, is by many argued to follow from the time depth it offers (e.g. Gamble 2014). Furthermore, while there are instances where the archaeological record allows what could be called glimpses into an ethnographic past of individual lives, the vast majority of the material we have access to is hampered by a degree of temporal uncertainty and lumping of events that necessitates a perspective that is developed to meet the nature and quality of the archaeological record on its own terms (Bailey 2007; Perreault 2019). Both fully utilising the archaeological material and playing to the strengths of the discipline is thus dependent on knowledge of the quality of the material available to us, while also being dependent on developing methodologies fit for treating the material, given the empirical resolution it holds.

## 1.1 Aims and research questions

The overarching goal of this thesis is to contribute to answering the following:

i) What characterises the extent and quality of the archaeological record from Mesolithic Norway?
ii) What consequences for our disciplinary agenda and potential for understanding the Norwegian Mesolithic does this hold?

The answer to these questions is a disciplinary-wide undertaking, and no single thesis can hope to arrive at a final answer. However, to contribute to their elucidation, the thesis is centred on three more specific research questions, derived from these overarching goals. The first of these can be viewed as largely instrumental in that it pertains to the degree and certainty with which we can fix the occurrence of our data on the calendar scale:

1) What chronological control do we have of the occupation of coastal Stone Age sites within the study area?

As (**vankilde?**) has put it: 'Chronology is the backbone of social interpretation'. Following from an answer to this first question, the following two questions can therefore be explored:

2) What general patterns characterises the lithic inventories of the sites over time?
3) How is the frequency of sites distributed across time?

These latter questions have direct substantive implications, as their answer can be expected to be directly related to cultural developments. It should be noted that these are stated in an open and exploratory manner. What patterns are explored in the lithic assemblages and how any variation in the frequency of sites over time is interpreted in substantive terms are not done following some pre-defined framework but is rather approached in an inductive and exploratory manner.

## 1.2 Study area

The study area of this thesis is delineated to coastal south-eastern Norway. No strict cultural-historical demarcation to surrounding regions is assumed, nor does that appear to be a have been the case throughout the Mesolithic.

Furthermore, what is termed the coastal Mesolithic naturally didn't exist in isolation from inland regions. While the Mesolithic sites in Norway are concentrated to the coast (e.g. Bjerck 2008), the reason behind the geographical limiting of the study is mainly analytical. First, while Mesolithic data is available from a wider region of south-eastern Norway, including inland areas, the last few decades have seen an virtual explosion of investigations in the coastal region between Horten municipality in the north east to Arendal in the south west. This has also been accompanied by geological studies to map the dramatic sea-level change that has impacted the Norwegian coast through the Holocene. The region thus represents an archaeologically well-sampled area where we also have good control of the trajectory of shoreline displacement. While the relevance of the findings in the study can be assumed to have relevance for surrounding areas, this subsection of south-eastern Norway therefore limits the spatial extent of the considered data. Furthermore, while this region holds high-quality archaeological material, investigated and recorded using modern methods, there is also an abundance of legacy data from the region, especially in the form of comparatively low-resolution and low-quality survey data. This constrained region thus also offers an excellent case-study for exploring the implications of dealing with data of wildly varying quality.

Finally, methods and approaches developed for the coastal sites are not necessarily directly transferable to inland areas. This pertains most clearly to the concept of shoreline dating, which is based on dating sites with reference to their present altitude and the relative sea-level fall that characterises the region (see Paper 1). This offers a degree of large scale temporal control that is independent of the preservation of organic material for $^{14}$C-dates that is unique to the coast.

## 1.3 Model-based archaeology

Moving on from establishing a firmer grasp on the temporal dimension of the archaeological record in the two first papers, the final two papers of the thesis are more directly aimed at elucidating past cultural historical dimensions by tracking developments in empirical trends that have been linked to the understanding of past hunter-gatherer societies. This will be done within a framework of model-based archaeology. Models can be seen as partially independent representations of theory and data (Morrison and Morgan 1999). By being a concrete realisation of an abstract theory in which its claims and conditions holds true, the model allows for a transfer of the logic of the theory to the modelled data, and a subsequent evaluation and manipulation of the fit between the two. Models are thus both descriptive and analytical, and can be seen as mechanisms or mediators allowing for the coupling of the two dimensions (e.g. D. L. Clarke 2015[1972]; Kohler and van der Leeuw 2007; Lake 2015). The inferential modesty called for above follows from the defining characteristic that 'All models are wrong, but some are useful', as Box (1979, 202) famously put it.

Barton (2013) proposes a conscious and explicit modelling practice in archaeology for the same reasons. Traditionally, archaeological explanation is based on inductive and informal construction of narratives based on the inferential strategy of including as much data as possible and then arriving at a single explanation that is perceived to be the best fit in a *post-hoc* manner. This is argued to have a tendency to result in explanatory complacency and high personal investment into the credibility of any given explanation. By embracing the explicit uncertainty and fallibility that is a defining part of model-based approaches, this will therefore increase disciplinary progress, as it will lower the threshold for probing, adjusting and discarding one's own models.

## 1.4 The hunter-gatherer model and the coastal Mesolithic of south-eastern Norway

The concept of hunter-fisher-gatherers will function as a foundational model from which to derive empirical avenues to be explored, and to propose possible causal drivers behind any observed patterns (cf. Warren 2022, 29). An example of a source from where this will be derived is the seminal work *The Lifeways of Hunter-Gatherers: The Foraging Spectrum* (Kelly 2013). In the introduction of the book, Kelly (2013, 4) states that it is aimed at providing its readers with 'some knowledge of the variation that exist among foragers and some idea of what accounts for it'. Thus, while comprehensive in scope, Kelly (2013) is also very explicit in the limitations of his review and cautions against. The societal variation among more recent hunter-gatherer societies is immense, and as foraging has constituted the predominant life-way for humanity for as much as, the variation that can be expected among past hunter-gatherer societies is comparatively vast (Singh and Glowacki 2022). Consequently, this thesis attempts to balance insights from hunter-fisher-gatherer studies more widely with an open and exploratory perspective. While preconceptions of hunter-fisher-gatherer societies necessarily dictates some of the analytical avenues taken and influence the type of questions that are asked, the aim is to have idiosyncrasies of the archaeological record in the context of coastal south-eastern Norway dictate the conclusions that are reached.

Another challenge in determining what empirical trends are of interest and how these are to be understood follow from the explicitly coastal setting of the study. Historically, both work on world prehistory and ethnography has focused on terrestrial contexts (e.g. Bailey 2004; Yesner et al. 1980). This issue also extends to the methodological realm, where for example the use of Geographical Information Systems (GIS) in archaeology has predominantly been used in terrestrial contexts (see e.g. Conolly and Lake 2006). This is especially pertinent for the study of the coastal Mesolithic of Scandinavia, which is characterised by dramatic sea-level change throughout the period (e.g. Bjerck 2008; Astrup 2018). Out-of-the-box procedures are therefore not necessarily directly applicable and have to be adjusted to meet the demands of a geologically dynamic coastal context.

## 1.5 Open research and reproducibility

In making the case for open sharing practices in archaeological research, Marwick (2017, 426) compares the principle of artefact provenancing with dissemination of raw data and methods that underlie a study. Without knowing the origin and find-context of an artefact, it's archaeological value is practically none. Comparatively, by openly sharing data and programming code that underlies a study, other researchers can assess the procedures that have led to the results. Apart from facilitating an evaluation of its reliability, this allows others to extend on the analysis and the employed data, to learn and reconstruct how methods are implemented, and to attempt to repeat all or parts of the analysis themselves. Open research is thus beneficial to archaeology as a cumulative research endeavour as it will both increase the frequency of model rejection and adjustment, by allowing others to explore their foundations and inner workings, and because it will increase the pace of method sharing, evaluation and adjustment.

This thesis has been written in its entirety using the R programming language (R Core Team 2021). Unlike for example mouse-driven computational analyses, this means that an unambiguous record of the entire analytical pipeline is recorded in the form of programming scripts, moving from the initial loading and cleaning of raw data, through to analysis, visualisation and final reporting of results. Given the large amount of analytical choices that have to made in the course of any analysis, this can never be adequately presented in prose. Furthermore, what a researcher believes they have done need not correspond with what they have actually done. The high-resolution analytical record that is the programming script makes this entirely transparent. All data, programming code, figures and text used in this thesis is freely available in version-controlled online repositories on GitHub (https://github.com/isakro) and on persistent archiving services where the repository is provided a digital object identifier (DOI). A complete overview with links to the various online archives associated with the individual papers and this synopsis is provided in Table 1.1.

Table 1.1: Overview of repositories and preprints.

| Text | Preprint | GitHub repository | OSF repository |
|---|---|---|---|
| Synopsis | NA | github.com/isakro/thesis | osf.io/h3jfd |
| Paper 1 | osf.io/cqaps | github.com/isakro/exploring-assemblages-se-norway | osf.io/7f9su |
| Paper 2 | osf.io/3x7ju | github.com/isakro/assessing.sealevel.dating | |
| Paper 3 | | github.com/isakro/shoredate | osf.io/ehjfc |
| Paper 4 | | | |

## 1.6 Overview of papers

### 1.6.1 Paper 1: *A simulation-based assessment of the relation between Stone Age sites and relative sea-level change along the Norwegian Skagerrak coast*

The first paper of the thesis offers an approach for integrating the various sources of uncertainty associated with reconstructing the relationship between $^{14}$C-dated archaeological phenomena and past sea-level change. This is used to quantify the distance between Stone Age sites and the prehistoric shoreline within the study area. That coastal sites would have been located on or close to the prehistoric shoreline is a fundamental premise in Norwegian Stone Age archaeology. In combination with reconstructions of past shoreline displacement, this is frequently used to date the sites based on their altitude relative to the present day sea-level—a method known as shoreline dating. The findings of the paper largely reflect the development proposed in the literature, with a predominantly shorebound coastal settlement in the Mesolithic, followed by a few sites being located some distance from the shoreline at the transition to the Early Neolithic (c. 3900 BCE) and a more decisive shift with the Late Neolithic (c. 2400 BCE). The result of this analysis is used to propose a formalised method for shoreline dating sites older than the Late Neolithic. This takes into account uncertainty as related to the displacement of the shoreline and the likely distance between sites and the shoreline when they were occupied.

### 1.6.2 Paper 2: *shoredate: An R package for shoreline dating Stone Age sites on the coast of south-eastern Norway*

Based on the findings from the first paper, the second paper of the thesis is a presentation of the R package *shoredate*, which provides tools for performing and handling shoreline dates along the Norwegian Skagerrak coast. This is freely available for anyone to install from the Comprehensive R Archive Network

(CRAN): https://cran.r-project.org/package=shoredate. The paper itself gives a brief presentation of the package, but the publication of software with the *Journal of Open Source Software* also involves a useful review process of the software itself. Having published the package and released it as open source software on CRAN means that the method for shoreline dating is now available for researchers and student to employ, and that underlying code is available for anyone to explore, evaluate, criticise or extend upon.

### 1.6.3 Paper 3: *Exploring the composition of lithic assemblages in Mesolithic south-eastern Norway*

The second part of the thesis is aimed more squarely on elucidating past cultural history, as opposed to the more instrumental focus of the first part on establishing tools for . The third paper of the thesis is an exploratory study aimed at identifying variability in the contents of a set of lithic assemblages. The main goals of the paper is to evaluate the typo-technological framework currently in use in Norwegian Mesolithic research, and to assess the temporal development for variables that have been linked to variation in land-use and mobility patterns. It is demonstrated that elements of the so-called Whole Assemblage Behavioural Indicators (WABI, e.g. Clark and Barton 2017) align with previous research into developments of mobility patterns in Mesolithic Norway, suggesting that the WABI could be a relevant framework also in this context. This is specifically reflected in a negative relationship between density of lithics, and the proportion of secondarily worked lithics in the assemblages over time, which is taken to reflect a transition from a more curated towards a expedient technological organisation with the transition from the Early Mesolithic (c. 8200 BCE). This is in turn argued to follow from a shift in land-use patterns and a overall reduction in mobility.

### 1.6.4 Paper 4: *Comparing summed probability distributions of shoreline- and radiocarbon dates on the Norwegian Skagerrak coast*

Unpacking the complex interplay between environmental conditions, settlement patterns and population density has been deemed of fundamental importance to archaeological inquiry (e.g. Shennan 2000; French 2016). The fourth and final paper of the thesis is aimed at combining findings from the previous papers to evaluate the interplay between some empirical indicators suggested in the literature to be related to these dimensions. Concretely, the paper aims at elucidating the relationship between variation in relative population size as potentially reflected in the density of shoreline dated sites and the intensity of radiocarbon dates over time.

# Chapter 2

# The Norwegian Mesolithic

This chapter presents the context of the study. This starts by laying out the general environmental developments in the period, before the general archaeological discussions and understandings of the period is laid out. While insights from studies undertaken in Norway and Sweden will be drawn on at times, the general focus is that of developments in south-eastern Norway.

## 2.1 Environmental setting

The environmental setting for the Mesolithic in Scandinavia is first and foremost defined by the end of the Weichselian and the retreat of the Fennoscandian Ice Sheet (see **skar2018?**; **skar2022:106?**). Most pronounced of the resulting environmental impacts is the melting of the ice sheet itself, corresponding eustatic sea-level change and isostatic rebound, changes in the ocean currents, as well as the developments of the Baltic Sea, which transitioned between being open and closed off from the North Sea. These developments form a backdrop to which human societies developed.

The global climate reconstructions based on oxygen isotopes from the Greenland ice cores gradual heating interspersed with cold events c. 10 300 cal BP, c. 9200 cal BP and 8200 cal BP. However, how these events played out locally and what consequences this development had for local ecosystems and human populations appears to vary quite dramatically across Northern Europe and thus cannot be assumed a priori to have been of relevance or concern for the societies dealt with in this thesis.

### 2.1.1 Climactic oscilations

Regional increase in temperature and perticipation rates from the early Preboreal, followed by mild winters and humid conditions in the Boreal with the Holocene Thermal Maximum, locally dated to c. 6400–2400 BCE.

At the transition to the Boreal, the vegetation in the region goes from arctic vegetation characterised by to

### 2.1.2 Sea-level change

Following the retreat of the ice

Depending on the scale of, sea-level change can impact not only the habitational suitability of any individual location, but can have far-reaching effects that impact shoreline morphology, drainage systems and (e.g. Groß et al. 2018; Astrup 2018). While the at times dramatic magnitude of these changes would have required a response from human populations inhabiting the impacted areas, it is by no means given that these would have negative consequences for these societies. In fact, areas impacted by sea-level change can be attractive for hunting, fishing and gathering precisely because of these factors. The study of human response to sea-level change should therefore not assume that this would be the result of. How sea-level change impacts the population of any given area will thus depend on a wide range of factors pertaining to the amplitude of shoreline displacement, the topographic setting, and the nature of the societal systems which respond to these changes.

## 2.2 Archaeological background

Having presented the environmental developments of the period, the following section gives a general outline of how chronological and societal developments in the Mesolithic have been characterised and understood archaeologically. The first focused research on the Norwegian Mesolithic is ascribed (**hansen1904?**), who studied the material that (**brøgger1905?**) later saw as a defining element of the Nøstvet culture (Bjerck 2008, 61). In 1909, Nummedal made discoveries of flint artefacts in western Norway that were deemed likely to have an earlier date than the Nøstvet material (**rygh1911?**; **nummedal1912?**), and which led to the subsequent definition of another cultural unit termed Fosna (**nummedal1924?**). Nummedal later also discovered material in northern Norway that had parallels with, but was considered distinct from Fosna and which was given the label Komsa (**nummedal1927?**). While the geographical and temporal relationship between these cultural units were recognised as unresolved and was subject to much debate, the common understanding was for many decades that southern Norway was defined by the chronologically sequential phases Fosna and Nøstvet, while Komsa was seen as defining of the entire Mesolithic period in northern Norway (see e.g. **indrelid?**). With renewed debates in the 1970s, debates that were arguably founded on a better understanding of lithic technology than much of the preceding discussions (**bjerck1996?**), significant . Mikkelsen (1975) suggested a tripartie division of the Mesolithic in south-eastern Norway by dividing the period into the Early, Middle and Late Mesolithic. This was in turn also subdivided

The traditional chronological framework for the Mesolithic of south-eastern Norway has followed along the general lines of that presented by Glørstad (2010, 23, see table). Recently this has been. The degree to which the chronological shifts are. While they form a natural frame for the narrative structure of this text, these should not been seen as the impact of strict periodisation is to some extent counteracted by treating. This will be treated in more detail in the next chapter

In a comprehensive reassessment that also includes results from the last decades of excavations, (**reitan2022?**) has recently suggested a new chronological framework for the Mesolithic period in south-eastern Norway (also **reitan2016?**). As his focus has mainly been on technological and typo-chronologcal developments, and given their recent date of publication, this chronological framework has yet to be comprehensively evualuated in terms of correspondence with other societal developments. The presentation of major chronological trends in the sections below thus follows the traditional periodisation. However, in the discussion chapter, the results of this thesis will also be seen in relation to the framwework suggested by Reitan.

The following sections will outline general societal developments believed to characterise the Norwegian Mesolithic, divided into the periods Early Mesolithic (EM; 9300–8300 BCE), Middle Mesolithic (MM; ) and Late Mesolithc (LM; . While focus is on south-eastern Norway, reference to a wider context and neighbouring regions in Norway and Sweden when perspectives

Following the excavation of the site Tørkop in Østfold in the mid-1970s (**mikkelsen1999?**), a few decades would pass until sites from the period were excavated in south-eastern Norway. Through the 90s and early 2000s sites were excavated as part of larger projects in Akershus [] and Østfold [and five unpublished sites from Halden @]. From the mid-2000s onwards larger projects in Vestfold and Telemark (Solheim 2017; Melvold and Persson 2014) as well as Aust-Agder (Reitan and Sundström 2018) The excavation of sites such as Rødbøl 54 in and Anvik in marks the start of this, with larger projects such as E18 Bommestad-Sky, Vestfoldbaneprosjektet and E18 Tvedestrand-Arendal subsequently increasing the data on the MM substantially. (Berg-Hansen et al. 2022; Rosenvinge et al. 2022; see Damlien et al. 2021 for a recent overview).

### 2.2.1 The Early Mesolithic (9300–8300 BCE)

The first human presence in Norway is recorded from around 9300 BCE, which marks the start of the EM. A central discussion has concerned whether people first migrated into the area of present-day Norway from a route along the coast of western Sweden, from the north-east along the northern-Norwegian coast (**bjørn1929?**), or across the Norwegian trench to south-western Norway from the the North Sea Plain (Doggerbank, **odner1966?**). The most recent evidence suggest that a crossing from Doggerbank would not have been feasible due to the distances involved at the time (Glørstad 2016; Glørstad, Gundersen, and Kvalø 2017). The present consensus is therefore that the earliest human dispersal into present-day Norway is likely to have originated on the coast of western Sweden around 9500–9300 BCE (e.g. Glørstad et al. 2020; Bjerck 2008). From here, human occupation is believed to have rapidly extended along most of the Norwegian coastline, while a north-eastern migration reached Kola and northern Norway some time before 9000 BCE (Manninen et al. 2021). These two routes are associated with the genetically defined 'western' hunter-gatherers that migrated from the south, and 'eastern' hunter-gatherers migrating from the north (**gunther2018?**), each identifiable also in terms of distinct material culture and technological traits (Manninen et al. 2021).

Pioneer sites in Norway and Western Sweden have traditionally been ascribed the archaeological cultures or techno-complexes Fosna and Hensbacka, respectively. Today these are seen as representing the same phenomena (e.g. Bjerck 2008, 75). Fosna/Hensbacka sites are to hold fairly homogeneous lithic inventories, and are held by many as having a common origin tracing back to South-Scandinavian and North-European Palaeolithic Ahrensburg groups (e.g. Bjerck 2008; Schmitt et al. 2009; Bang-Andersen 2012; Fuglestvedt 2012; **riede2014?**). The analyses of artefact inventories, the presence of high-quality South-Scandinavian flint, and the chronological support of radiocarbon dates and shoreline dates have in sum led to the consensus on this continental connection (see Fischer 1996; Schmitt et al. 2006; Fuglestvedt (2007); Fuglestvedt (2009); Bang-Andersen (2012); Glørstad (2016)).

The similarities has led Fuglestvedt (2012, 8) to propose that the terms Fosna/Hensbacka be abandoned altogether, in favour of Ahrensburg, as this would accentuate the continental elements that appear so defining for these pioneer sites. Although this has been met with varying degrees of enthusiasm (e.g. Bjerck 2008, 73; Åstveit 2014, with comments), particularly by those emphasising the strong marine orientation of the Fosna/Hensbacka, there has also been the occasional use of variations such as 'coastal Ahrensburg' to denote the Fosna/Hensbacka (Prøsch-Danielsen and Høgestøl 1995). Following a recent analysis of lithic inventories from the transition from the Palaeolithic to the Mesolithic in Northern Europe, and comparison with Fosna/Hensbacka sites in Norway and Sweden, Berg-Hansen (2017) has argued that the Fonsa/Hensbacka sites have a clear similarity with EM Maglemose sites in Denmark. While there are elements of this technology that point back to the Ahrensburg, there is also a clear break with the Palaeolithic technology. She therefore argues that these societies should, as with the Maglemose, be considered Mesolithic and not a northern continuation of Palaeolithic life-ways. At any rate, these discussions do go to show that there is a clear affinity between the first human population on the Scandinavian Peninsula and continental hunter-gatherer groups.

To account for this apparent pan-regional homogeneity, a central question is by what process Norway and western Sweden were initially colonised (e.g. Bjerck 2009; Schmitt et al. 2009; Bang-Andersen 2012; Fuglestvedt 2012; Glørstad 2016; Berg-Hansen 2017). An important aspect in this regard is the fact that the coastal areas in western Sweden and Norway were largely ice-free early on, and must have been rich and desirable areas in terms of resources (**bjerck1996?**; **bjerck?**; Bang-Andersen 2012; **glorstad?**). Bjerck Bjerck (2017) has explained the fact that people did not start exploiting these regions until around 9300 BC with reference to less developed marine subsistence strategies among North-European hunter-gatherer groups. The coastal location of the majority of Fosna/Hensbacka sites will undoubtedly have necessitated extensive adjustment to marine environments, including by the use of boats. Following the delay induced by this, the hunting of seal, conceptually not that different from the hunting of large terrestrial mammals, might have spurred an increased development of boating technology, while at the same time lending itself to the continued use of a continental artefact inventory (Bjerck 2009; **bjerck2016?**). Proficient and effective use of boats might in turn have resulted in a relatively rapid colonisation of Fosna/Hensbacka areas, possibly as fast as over a period of only 200–300 years (Bjerck 1987a; Bang-Andersen 2012), providing a possible explanation for the homogeneous assemblages. These could reflect mobility of a kind not allowing for familiarisation with local resources for tool production, nor the development of distinct inventories adjusted to various geographical settings. The assemblages might therefore represent a sort of catch-all tool-kit, suitable to meet the variable demands of a 'pioneer condition' (Bjerck 2017; see also Breivik and Callanan 2016). Furthermore, Berg-Hansen (2017, 232) has argued that the homogeneity in the lithic inventories could be related to relatively high population numbers with closely knit social ties, which in combination has enabled this technological conservatism. Homogeneity and continuity in lithic technology over vast expanses of Scandinavia would be difficult to envision with a thinly spread population consisting of more isolated groups.

In general, the Early Mesolithic in Norway is understood as consisting of highly mobile groups (**bjerck1996?**; Bjerck 2008; Fuglestvedt 2012). As there is little organic material on which to base inferences on subsistence, there is little direct evidence to go on when attempting to determine what available resources made up the diet of people in Norway in the first centuries of human occupation. The EM sites are mainly found along the coast, and so there is little doubt that aquatic resources have played an important role (Bjerck 2008; Bang-Andersen 2012; Fuglestvedt 2014). It can, however, not be excluded that land mammals, and various species of fowl and flora have also been important constituents of the diet (Åstveit 2014; Fuglestvedt 2014). While the settlement is focused on the coast, there is also a marked presence in the mountain regions of south- and north-western Norway from a very early stage (Hagen 1963; Bang-Andersen 2012), which are believed to have been related to the hunting of reindeer. Given the lack of direct evidence for subsistence, an analysis settlement patterns has often been drawn on to make general inferences to subsistence strategies.

The coastal EM sites have been characterised as typically situated on small islands and been exposed to

As a counterpoint, some authors have argued that the differences between settlement patterns in the EM and later periods have been exaggerated or are not properly understood (see Glørstad 2013; Åstveit 2014), and in a recent study I found that the settlement patterns in a subregion of south-eastern Norway was fairly similar across the EM, MM and LM (Roalkvam 2020). Of the considered variables, the most important driver of settlement patterns was found to be degree of exposure, where the sites were found to be located with relatively open immediate surroundings, while at the same time being sheltered from larger stretches of open sea. However, one of the locational patterns that was not considered was the location of the sites within the wider landscape. The term exposure has been used to denote both how commanding the view would have been from the sites and how exposed the sites would have been to wind and wave-action (Svendsen 2014), but has also pertained to their location relative to deeper fjords and outermost coast (e.g. **lindblom1984?**; Jaksland 2001; **bergsvik2001?**; Bjerck 2008). It has been argued that EM sites would have mainly been located on the outer coast, and that the fjords were not utilised until a later stage. This is argued to reflect a focus on the hunting of marine mammals, especially seal, in the EM.

Coupled to this discussion is also what motivated the initial human dispersion into these territories.

The blade technology of the Early Mesolithic is based on single- and dual-platform one-sided cores, typically by means of direct percussion (Solheim 2020).

Some central discussion that linger in the literature concerned with the period pertains to the nature of the first colonisation of Norway. Did the coastal areas represent an unprecedented opportunity in an extended Garden of Eden, or an obstacle to be overcome. Furthermore, discussions pertaining to the degree of mobility and unified nature of lithic assemblages

### 2.2.2 The Middle Mesolithic (8300–5600 BCE)

While the Middle Mesolithic (MM) was defined as a separate typo-chronological phase in the 1970s, Bjerck (2008, 92–98) stated as late as 2008 that the period was associated with a limited archaeological material, thus posing an analytical challenge. This is in part related to sea-level transgressions in this period along the coast of southern and western Norway. In south-eastern Norway, which has not been subject to sea-level transgression, the lack of MM material can in part be related to the fact that MM sites are located at elevations that are typically not impacted by the expansion of infrastructure, and thus not targeted by archaeological investigations (Jaksland 2001, 27).

Discussion pertaining to the MM have in part been concerned with whether the period has more in common with the highly mobile societies of the EM or with what was is typically seen as more semi-sedentary LM societies (e.g. Mansrud 2014; Solheim and Persson 2016; Berg-Hansen et al. 2022). While there is a clear shift in the lithic technology around 8300 BCE (Damlien 2016; Eymundsson et al. 2018; Solheim, Damlien, and Fossum 2020), which coincides with the genetic, several aspects of these societies are still believed to show similarities with the EM. For one, MM sites have traditionally been seen as remnants of shorter stays. Furthermore, based on a from Northern Vestfold, Berg-Hansen et al. (2022, 662) have argued that coastal settlement patterns in the start of the MM appear to be a continuation of those from the EM, characterised by a site location concentrated to the outer coast. However, as with the lithic technology, several aspects of the MM show a clearer break with the preceding EM. Excavations of more substantial sunken dwelling-structures dated to the MM, as opposed lighter tent-like constructions characteristic of the EM, has been excavated over the last decade (Solheim and Olsen 2013; Mjærum 2018; Berg-Hansen et al. 2022). The higher investment in time and resources that these dwelling structures represent has been taken by many to indicate an increased attachment to the area in which they were built []. It has been suggested that the semi-sedentary traits of the LM can be traced back to the MM (Glørstad 2010; Solheim and Persson 2016; see also the third paper of this thesis: Roalkvam 2022). This also follows from the investigation of what has been interpretad as integrated settlement systems consisting of sites with different functions, as well as an increased use of local. As pointed out by Berg-Hansen et al. (2022), continuation in some aspects of the. (**bjerkc2008?**) thus notes that the MM is characterised by a degree of regionalisation that is not evident in the EM material.

In the Middle Mesolithic blades are produced from conical and sub-conical cores with a single platform using pressure and indirect percussion [].

### 2.2.3 The Late Mesolithic (5600–3900 BCE)

Blade production in the Late Mesolithic is characterised by the introduction of handle cores (**eigeland2015?**). As pointed out by Solheim (2020, 4), this involves a shift from cores that will result in blades of gradually diminishing size as the core is exhausted in the EM and MM, to cores that result in blades of a uniform and comparatively small size (i.e. microblades).

# Chapter 3

# Analytical background

This chapter presents underlying analytical concepts that have motivated the studies undertaken for this thesis.

## 3.1 The quality of the archaeological record

Two central concepts in the philosophy of science is that of equifinality and underdetermination. Equifinality pertains to cases where similar processes result in the same empirical output, while underdetermination pertains to situations where quality or resolution of the empirical data is too low for it to be possible to choose between competing explanations.

The quality of the data available to us is therefore fundamental for knowing what questions we can and cannot hope to answer about the past (Perreault 2019). Lower quality data will lead to averaging and smoothing, where for example a reduced temporal resolution can lead to chronological smearing that hides smaller scale oscillations and variability (Bailey 2007). The same principle extends to the spatial scale, where a lower empirical coverage The same principle extends to the dimensionality of the data, where loss will result in a reduction of variability and richness, for example in the composition of artefact assemblages. Loss and mixing are consequently more subtle effects than complete absence of data, which can be more easily recognised. Furthermore, effects such as loss, mixing of past events and analytical lumping will most likely not impact the quality of the data in a uniform way. Taphonomic loss is likely to be more severe the further back in time one moves (Surovell et al. 2009), and analytical bias following from disciplinary interests or what geographical areas have been subjected to archaeological investigation will also skew our impression of the past (Binford 1964). Mapping the spatial and temporal quality of the archaeological record is thus critical for knowing what past processes we would be able to discern, and by extension what explanations we can hope to reject and what questions we can hope to answer.

## 3.2 Chronology, archaeological cultures and modifiable analytical units

The study area of this thesis, as situated firmly within south-eastern Norway, is for the most part believed to have followed the same unified trajectory in terms of overall cultural-historical developments and expressions of material culture through the Mesolithic. At least within the analytical detection limit that has characterised the field thus far. What has been of greater concern is the temporal transition between the occurrence of cultural taxonomic elements within the region. In Norwegian Mesolithic research more widely, however, the question of both regional variation in material culture and its timing has led to discussions of whether a concept known as 'chronozones' has any merit as a framework for systematising the archaeological material. While this is a concept that has remained marginal in archaeology as a whole, as it is mainly used by some practitioners in Norwegian Stone Age archaeology (e.g. Bjerck 2008; Nyland 2016; Breivik, Fossum, and Solheim 2018), it is related to fundamental issues faced within archaeology in general.

The concept originates in a paper by Bjerck (1986), in which he attempts to tackle the distinction between the archaeologically defined cultures of Fosna and Nøstvet in the context of Western Norway. Instead of an ever-continued nuancing of these terms as more variation and idiosyncrasies are encountered in the archaeological record, Bjerck (1986, 117–19) instead suggests a division of the Mesolithic period into a series of time-intervals denoted chronozones, originally at a resolution of 500 years, which he argues could facilitate a Pan-Scandinavian framework for approaching the Mesolithic (Bjerck 2008, 72–73). The concept of chronozones is taken from geology, where the term is used to denote stratigraphic layers that formed over the same specific time-span on a regional or world-wide scale, known from geochronological terms such as. Bjerck's motivation for adapting this to archaeology is to form a framework that is neutral with respect to cultural variation across space and time. He argues that traditional archaeological units of analysis, typically denoted by terms such as cultures or techno-complexes that are discretely delineated in time and space, has led to an artificial partitioning of the archaeological material that is less open to gradual temporal change and spatial variation – an issue that has been conteded with by many archaeologists through the years (e.g. Childe 1956, 121–25; Smith 1992; Clark 2009; Roberts and Vander Linden 2011; Reynolds and Riede 2017). Bjerck argues that the use of neutral 500-year time-intervals will reduce the degree to which analyses will overemphasise homogeneity within, and exaggerate differences between such analytical units. To further illustrate the issues Bjerck attempted to tackle, it is useful with a detour via the concepts of the Modifiable

Areal Unit Problem (MAUP), as taken from the field of geography (e.g. Harris 2006), and its recently coined temporal equivalent, the Modifiable Temporal Unit Problem (MTUP, Bevan and Crema 2021).

### 3.2.1 Modifiable analytical units

The MAUP is defined by (**heywood2002?**) follows from the necessary procedure of aggregating continuous data within spatial units. Typical examples from archaeology is the definition of the site, based for example on the distribution of individual artefacts.

Moving on to the temporal dimension, Bevan and Crema (2021) have recently given a demonstration of how archaeological periodisation involving lumping and splitting of phenomena within disjoint time-intervals have analytical consequences that remain under-appreciated within archaeology. First, employing strict cut-offs between temporal units – units that often also vary in their duration – has major implications for comparison between these units, and can, as (**bjerck?**) also notes, lead to an artificial analytical overemphasis of the transition between these. Basic operations such as comparing counts will be skewed by variable duration of these units, and the position of breaks between them can be highly influential to the appearance of the frequency distribution of events over time.

Building on Crema and Kobayashi (2020), Bevan and Crema (2021) also further define three types of uncertainty associated with this archaeological practice. The first is *phase-assignment uncertainty* – how certain can we be that a given phenomena can be ascribed to a given phase. The degree to which this varies between different material categories means that it can be difficult to compare their frequency across archaeological phases. As was found in the third paper for this thesis (Roalkvam 2022), the occurrence of formal tool types at the Mesolithic sites in the study appears to be greater further back in time. This as opposed to younger sites, which are to a larger extent dominated by generic debitage that can be more difficult to assign to a phase. This can have implications for how many sites are ascribed earlier periods, and, by extension, could impact a comparison between phases. The second pertains to the *within-phase uncertainty*, the degree to which the occurrence of a various phenomena have an equal likelihood of occurrence throughout an archaeological phase. For example, it is typically assumed that the occurrence of the handle-core and the Nøstvet axe is uniform across the Nøstvet period. The final dimensions Bevan and Crema (2021) highlight is the *phase boundary uncertainty*, which pertains to the start and end points of the archaeological phases themselves. As these are typically defined by a complex interplay of multiple cultural phenomena, they are seldom meant to operate on the scale of individual years, but will in practice often be operationalised as such.

### 3.2.2 Archaeological chronozones

While possible methodological ways around these issues are presented in the references above, neither the MAUP nor the MTUP have any clear solutions, and the magnitude of their impact will depend on the given research question and accompanying analytical scale. However, their formulation arguably form a better frame for understanding these issues than the concept of chronozones. I believe chronozones can instead obfuscate the distinction between the temporal and the spatial scale, and that it can lead to a conflation of typology – understood in its widest possible sense – as a methodology for systematising archaeological material and its potential use as a dating method. As Bjerck (1987b) states, typology obviously has its place as a culturally responsive framework in both time and space. Chronozones, on the other hand, are supposed to make comparisons across typologically inferred boundaries in space and time tractable.

In her comment to Bjerck's paper, Skar (1987, 35) notes that geological chronozones couple pan-regional stratigraphic layers with the calendar scale, but that there is no equivalent pan-regional archaeological phenomena that equally consistently correspond to a section of the calendar scale. As Bjerck (1987b, 40) further underscores in his response, archaeological chronozones are therefore not, unlike typological frameworks, meant to be culturally responsive, but are to represent a neutral temporal scale, typically instantiated as 500-year intervals. However, as Østmo (1987) and Mikkelsen (1987) note in their comments to Bjerck's original paper, this purpose is already fulfilled by the calendar. If the stratigraphic information related to a specific time-interval is removed from the geological chronozone, only 'chrono' remains. Similarly, if the archaeological chronozone is not meant to hold any culturally responsive component, only the time-scale remains.

As a culturally independent scale, the calendar scale will always be preferable to the that of chronozones. Not only because it is firmly established, but also because it already allows for more variation in the temporal resolution associated with different phenomena to be systematised, and allows for their duration and uncertainty of occurrence to span a wider range of aggregative time-units. The ability to shift and readjust the temporal resolution depending on the phenomena one is attempting to align and contrast is important as 'different timescales bring into focus different sorts of processes, requiring different concepts and different sorts of explanatory variables' (**bailey1987?**; Bailey 2007).

In replying to this critique, Bjerck (1987b, 40) states that questioning the need of chronozones when we already have the calendar is like asking 'Do we need the term "month" when we have numbered days?'. This would seem to imply that chronozones, like the month, is to be of a predefined duration, at least within any individual study, and that variable duration and uncertainty of phenomena to be analysed has to be collapsed into these aggregative units. Another question is also if the terminology used with chronozones is better than simply stating what time-intervals we are dealing with. For example, Bjerck (2008) uses the term Middle Mesolithic (MM) to denote the three chronozones MM1, 8000–7500 BCE; MM2, 7500–7000 BCE; and MM3, 7000–6500 BCE. A reader coming across 'MM3' instead of '7000-6500 BCE' therefore has to keep in mind that this simply refers to this specific time-interval. The term Middle Mesolithic should be entirely disregarded, as it is in this use meant to be devoid of any cultural meaning. One could perhaps change terminology to something that doesn't have as many cultural and research historical connotations as the Middle Mesolithic, but it strikes me as altogether unnecessary to do this via the chronozone, as this is simply solved by establishing a reference frame only using the calendar scale in the first place. If one wants to use a time-scale of 500-year intervals it would in my mind be better to simply define this independently of the now inflated discussion of chronozones, not least because I believe the discussions of the concept demonstrates that its use can lead to unnecessary confusion – if not for practitioners, then likely for readers.

In their comments to the original paper by Bjerck (1986), Østmo (1987) and Mikkelsen (1987) deem chronozones an unnecessary and complicating concept. Commenting on these critiques, Nyland (2016, 53–56) states that both of the authors make their comments in light of typological frameworks for Mesolithic south-eastern Norway, but that neither address the issue of the geographical coverage that these have. However, this is first and foremost an empirical issue rather than something to be solved by new terminology, and clearly not by the chronozone, which is a concept meant to be culturally unresponsive.

Drawing on an example given by Nyland (2016, 55); if the question is if central Norway falls within the same cultural sphere as south-eastern Norway, understood to be determined by comparable material culture, then this is dependent on two dimensions, assuming the problem of the initial delineation of these two regions has been resolved. The first is an evaluation of the degree to which characteristics of archaeological material in the two regions is considered to be similar, according some criteria. The second pertains to the timing of the occurrence of this material. To establish this necessarily demands temporal data that is independent of the typological framework itself, or possibly by reference to some principle of seriation with the uncertainty that this entails (e.g. Dunnell 1970). If a set of artefact types occurs in both central Norway and south-eastern Norway, this could lead one to suggest that a similar kind of cultural expression is common to the two regions. If independent temporal data associated with this material, such as radiocarbon dates, additionally indicates that there is a temporal synchroneity between their occurrence, then this would lead one to conclude that this cultural expression appears to occur simultaneously – within some level of temporal certainty. Depending on the magnitude of artefactual and temporal evidence for this coincidence, this could then lead one to apply this typological framework as a dating method in the case that one excavates a site in either region and discover material of the type in question. A continuous adjustment and evaluation of the reliability of the identified cultural affinity and the derived typological dating frame will of course be necessary, but will have to be founded on material culture and the position of their occurrence on the calendar scale. This also pertains to the co-occurrence of various archaeological evidence and their wider cultural implications, for example whether or not some artefact type tends to be associated with agricultural activity. If either region lack artefactual or temporal data, then either the nature or the timing of cultural affinity cannot be resolved. The concept of chronozones cannot overcome these issues, and, I think, is more likely to confuse them.

In conclusion, I therefore agree with Østmo (1987) and Mikkelsen (1987) in that the concept of chronozones represents an unnecessary complication. Although some of these complications follow from misunderstandings of the original proposition (as pointed out by **bjerck?**; Nyland 2016, 55), it nonetheless appears to sometimes lead to the muddling of several spatial, temporal and culturally taxonomic issues. These are therefore arguably best handled by reference to already well-established terminology, and by the use of modern methods that allows for the formal definition and handling of fuzzy and uncertain categorisation in the aggregation of data, both on the scale of material culture, time and space (e.g. Bayliss et al. 2007; Crema 2012; Fusco and Runz 2020; Bevan and Crema 2021; Shennan, Crema, and Kerig 2015).

The amount of ink now spent discussing chronozones also means that invoking archaeological chronozones carries with it the necessity to clarify how one intends to use it, which can be circumvented by avoiding the term altogether. Furthermore, as a term suggested for use in Pan-Scandinavian Mesolithic research, I believe this idiosyncratic terminology will also unnecessarily divorce the field from discussions of the same issues within archaeology more widely, while also making the field less accessible to outsiders, more difficult to couple with adjacent disciplines, and possibly lead to confusion with the geological chronozone. It is therefore unclear what the concept now provides beyond what well-established archaeological and colloquial terminology already covers, perhaps apart from making us aware of these universal archaeological issues.

### 3.2.3 Radiometric and archaeological dating methods

Rather than being based on predefined discrete time-intervals beyond the calendar scale, the analyses undertaken in the papers for this thesis largely rely on absolute dates from radiocarbon- and shoreline dating. These two methods can be denoted radiometric and archaeological dating methods, where the first category is based on a process of radioactive decay and the second is based on some regularity in human behaviour leading to predictable variation over time. In the case of shoreline dating, this follows from the proclivity of people to have continuously settled close to the shoreline throughout the Norwegian Mesolithic, where the coupling of this with the calendar scale follows from the timing of shifts in the relative sea-level. However, a note should also be made on the fact that radiometric dating is never purely based on a steady process of radioactive decay. Apart from interpretations to do with the calibration, reliability and sampling context of for example a $^{14}$C-date, this also has to be seen in light of other chronological information and the wider archaeological context (Wylie 2017). As with the process of shoreline displacement, the cessation of radiocarbon uptake in an organism also requires an interpretative step to be meaningfully associated with a cultural event of archaeological interest.

It is important to underscore that given the scarcity of radiocarbon dates, and the relative low resolution of shoreline dates, typological frameworks responsive to variation in material culture can most decidedly offer valuable chronological insights, even though this is not directly integrated in the studies undertaken for this thesis. Furthermore, while the analyses are done here using dating methods that largely operate irrespective of archaeological periodisation, the results are frequently narratively and informally associated with general cultural developments believed to characterise the Stone Age of south-eastern Norway, as roughly outlined in Chapter 2. This is predominately done in an approximate manner with reference to what are best viewed as temporally and spatially fuzzy frameworks, and is based on the underlying logic that frequent co-occurrence of a range of material expressions in time and space, as suggested by others, reflects some level of meaningful cultural cohesion. This also means that the term culture is used in a loose archaeological sense and is not presumed to equate to a people or a unified unit in terms of language, genetics, or social structures (see e.g. **robert2011?** for a thorough discussion of the culture term as used in archaeology). While it appears reasonable to assume that such cohesion has largely resulted from the same cultural factors within the geographically limited area of south-eastern Norway through the Mesolithic, it is also worth noting that empirical correspondence can be driven by other factors. This includes cases where people have arrived at the same technology in disjoint regions of time and space, known as convergence, as well as cases where the same technological expression has occurred across a range of different cultural and environmental settings (which for example has been argued to be the case with slotted bone tool technology in Northern Europe, Manninen et al. 2021) – effectively an example of equifinality.

## 3.3 Hunter-fisher-gatherers

Hunter-gatherers or foragers are useful but fuzzy and not unproblematic synonyms that are typically, but not exclusively, used to denote societies that have a diet based on non-agricultural produce (Kelly 2013). One implication of this defining characteristic appears to be a case where environmental variation represents a considerable constraining back-drop against which cultural variation can occur. As these dimension can have a fundamental impact on the economic basis of these societies, environmental variability has been frequently shown to impact central aspects of forager societies, such as population dynamics and mobility patterns, which in turn are interrelated with virtually all dimensions of these societies (e.g. Kelly 2013; C. Morgan 2009; Bird and Codding 2008; Hoebe, Peeters, and Arnoldussen 2023; Ordonez and Riede 2022).

However, while it appears to be the case that variables such as temperature and precipitation dictate general variability among prehistoric and historic hunter-gatherers, this has to be severely qualified. First of all, while this appears to be the case *in general*, it can hardly be assumed to apply to any individual case. Secondly, understanding precisely what consequences variation in any single environmental variable has in any given case, if any, will be severely complicated by both the societal and environmental systemic wholes that respond to this variation, with some systems being more or less robust to such perturbations, depending also on the time-scale over which it operates and can be recognised archaeologically. While for example drought could intuitively be taken to represent a societal challenge, this certainly need not be the case. Drought might very well present more opportunities for resource exploitation than it eliminates (**arponen2019?** with further references), illustrating the point that any systemic variation, environmental or societal, can represent both threat and opportunity to systemic wholes, and simultaneously represent threat and opportunity to different parts of the system.

What role environmental variability has for hunter-gatherer societies is also likely to be impacted by research-historical trends and

The further delineation of this thesis to focus on coastal societies has some further implications for what ethnographic data can be reasonably drawn on, and some of the implications this can be expected to have for the structure of the societies in questions. First of all, the exploitation of marine resources is found to, in general, have been associated with a higher net productivity. The term 'fishers' is included the header for this section to underscore this coastal setting and underline the likely role fish played for these societies. However, while marine resources can reasonably be assumed to have made up a large of the diet of these societies, this likely to a varying degree involved the catching of fish.

As representing a considerable stage in the human adaptation to diverse environments, marine habitats are deemed an essential component in the evolution of modern humans and determining for the spread of the species across the globe. In Palaeolithic archaeology, the coast has traditionally been seen as a hostile environment for early hominins – a challenge to be overcome. Marine habitats and resource-use has also been characterised as a central for the evolution and

A note should be made on the fact that in the literature the terms *coastal adaptation* and *coastal resource use* are sometimes taken to imply different and quite specific things (see e.g. Faulkner et al. 2021; Marean 2014; Will, Kandel, and Conard 2019). Coastal resource use is in this understanding seen as something that is conducted sporadically or occasionally, and that has limited transformative feedback effects on the life-ways of the societies in question. Conversely, coastal adaptation involves a degree of coastal engagement and commitment that has an altering effect on these societies. I do not use the terms in this manner here. The conceptual distinction is certainly an important one, especially as marine exploitation is believed to potentially, but not necessarily (e.g. Erlandson 2001), lead to technological ratcheting and increased societal complexity. However, these quite specific connotations of the terms stand in danger of leading to misunderstandings for readers that have another understanding of adaptation, which need not be defined by some threshold in the intensity of coastal engagement. One response to the specifics of a given marine habitat might for example be movement and extended use of terrestrial resources, which would fall within a more inclusive definition of adaptation to a given coastal environment. While the above division might have merit in some analytical settings, the dependence on marine resources is arguably better understood along a continuum that I believe this distinction might unnecessarily dichotomise.

## 3.4 Palaeodemography

Palaeodemography or the study of temporal and spatial variation in the size and structure of past populations is a fundamental problem for archaeology (e.g. Shennan 2000; French et al. 2021). This follows from the fact that demography is a determining factor in processes such as genetic diversification, social network structure and scaling, technological innovation and accumulation, as well as. As human culture is in large part determined by human interaction,

One Known as the forager paradox. (French et al. 2021, 4) One of the implications of these findings is that the interpretation of archaeological proxies for population size is not as straightforward as one might immediately think. This in turn, needs to be kept in mind both when devising and comparing multiple population proxies, and in the construction of the narrative that builds on any numerical results.

Demographic modelling in early Holocene Fennoscandia has taken on a few different. In most recent years this is the SPD approach (Solheim and Persson 2018; Solheim 2020; **nielsen20?**; Jørgensen, Pesonen, and Tallavaara 2020). The SPD approach is based on summing the probability distribution associated with $^{14}$C-dates. However, there are a series of methodological and conceptual issues with this procedure, some of which have been dealt with and others which are integrated into the methodology and needs to be accounted for when interpreting any results.

Preconceived notions of what has characterised hunter-fisher-gatherer societies will at some level necessarily impact how the material under study is interrogated. This follows both from what research questions are deemed central, and because these notions will have impacted how the material has been retrieved and recorded. These notions are therefore to some degree inescapable influences on any research into the Norwegian Mesolithic. However, in the next chapter I will argue how this does not, and should not, lead to epistemic despair.

# Chapter 4

# Model-based archaeology

Over the years, several works have purported the benefits of a model-based archaeology (e.g. D. L. Clarke 2015[1972]; Wylie 2002, 91–96), which has especially gained a footing within the sub-field of computational archaeology (e.g. Kohler and van der Leeuw 2007; Lake 2015; Barton 2013; Romanwoska 2015; Brughmans 2021). The goal of the next two chapters is two-fold. First to elucidate what defines or can define a model-based scientific approach, and in the next chapter to demonstrate how this can form a useful framework for archaeological inquiry by drawing on examples from the papers of this thesis.

Central to the present chapter are four problem areas in the understanding scientific models, as identified by Frigg and Hartmann (2018): 1) The ontological: what are models? 2) The semantic: what do models represent? 3) The epistemological: how do we learn with models? And 4) what consequences do the use of models have for overarching principles such as scientific realism, reductionism and explanation?

One fairly common understanding of models simply entail seeing them as a set of simplifications or assumptions concerning real-world phenomena (e.g. Barton 2013, 154). Any representation could thus be considered a model whether it is generated physically, digitally, verbally, simply imagined, or is construed in a natural or formal language. Scholars arguing the case for model-based archaeology often start out by making the point that whether we acknowledge it or not, we always employ such abstractions when attempting to understand past reality (Kohler and van der Leeuw 2007, 4; Lake 2015, 7). The infinite complexity of reality means that any description of it has to be a simplification, and even if we were able to, a complete rendition of reality would not be a worthwhile endeavour in its own right. A perfect reconstruction of reality would be a tautology, which without perspective offers neither insight nor understanding (Yarrow 2006, 77; Slingerland and Collard 2012, 14–19). Put differently, whether we understand archaeology as tasked with providing explanation, understanding, or interesting narratives about the past, any demand for a higher empirical resolution, for its own sake, would be a refutation of theory (see Healy 2017).

These are, however, universal scientific points, variations of which have been made under diverse headings of archaeological theory (e.g. **johnson2010?**). It would thus follow that if the term model is taken to denote all generalisations or abstractions of reality, which in its ubiquity would include any description or explanation, it is not given why this would have to be dealt with within a comprehensive model-based archaeology. The arguments in favour of a distinct model-based archaeology tend to follow from *how* this necessary simplification should be made, and in turn handled. What this entails can be foreshadowed here by invoking the classic quote from Box (1979, 2): 'All models are wrong but some are useful'. But if all models are wrong, what is their epistemic value? To begin to answer this question the above view of models, simply seeing them as abstractions, will be accepted for now without regard for their demarcation to data, theory and hypotheses.

## 4.1 Confronting beliefs with data

The explicit testing of archaeological explanations was assertively introduced to the discipline with processualism, which argued that archaeology should adopt the explanatory goals of positivist social sciences. How this was to be done first follows from the standard processual view on what the archaeological material represents. Here, material culture was seen as an integrated part of – and the result of – total, multidimensional cultural systems (e.g. Binford 1962). As such, theories concerning how all aspects of cultural systems would influence and manifest in the material record should be conceived. Central to this is that the archaeological material represents an objective, albeit complex empirical record that reflects empirical causes, irrespective of our beliefs about what these causes are. The empirical material will in this processual understanding therefore offer a direct link back to this systemic whole. Archaeological material is therefore representative of the multidimensional causal chain from cultural system to the archaeological record. Theories concerning the prehistoric systemic whole and what processes have influenced the remnants available to us must therefore come prior to an archaeological investigation for it to be possible to evaluate their veracity. As complex integrated wholes, such models of entire systems were then to be tested by drawing on the hypothetico-deductive approach. Furthermore, drawing on the deductive-nomological or covering-law framework as taken from Hempel's logical positivism/empiricism, the ultimate goal was to establish laws pertaining to the conjunct occurrence of certain types of material remains with certain types of societal systems, irrespective of time and place.

It should be noted here that the programme of logical positivism, and the more mature logical empiricism (although see Uebel 2013 on this distinction), were far more nuanced that what they are often given credit for in the archaeological literature concerned with establishing why these views were misguided (Gibbon 1989, 8–60). This is equally true for the over-simplifying presentation that is given here. However, this can in part be justified with reference to the naive versions of these programmes that were adopted by positivist social science and archaeology at the time (Gibbon 1989, 91–117).

According to Hempel (1965, 231–43), the goal of science is to establish laws that are deductively valid, of the kind given by the classic example *All men are mortal / Socrates is a man / Therefore, Socrates is mortal*. If the premises are true, then the conclusion will always be true. However, when adapted to archaeology, the proposed laws were so banal that (**flannery1973?**) stated that attempts at adopting Hempelian empiricism 'has produced some of the worst archaeology on record'. The search for covering laws was therefore quickly abandoned by most practitioners. Furthermore, whether an argument is deductively valid or not is not dependent on whether the premises are true. If it happens to be true that all men are mortal and Socrates is a man, then the deductively valid argument is said to be sound. Determining whether the premises are true of the world depends on non-deductive reasoning. A deductively derived test that successfully corresponds with data only supports the hypothesis inductively (Chapman and Wylie 2016, 27). However, giving up on the search for covering laws and deductive certainty need not entail that hypothetico-deductive testing is misguided, and more modest goals of confronting beliefs pertaining to specific contexts or research questions with data is, I will argue, still very much a viable goal.

Smith (2015, 2017) has stated that one of the most central questions we can ask about our archaeological explanations is 'How would you know if you are wrong?'. Archaeological explanation often take the form of what Binford (1981) termed a *post hoc* accommodative argument. This involves first gathering and categorising the data of interest, often using variables chosen by convention and convenience, and then building an explanation around any discerned patterns (Clark 2009, 29). This data-dredging or pattern-searching approach is argued to constitute a limited inferential framework for a couple of reasons.

First, what among the virtually infinite aspects of the material available to us is considered interesting will always be determined by our beliefs concerning the processes that have resulted in their manifestation. What characteristics of the material is recorded and drawn on to organise it will dictate what patterns one can hope to reveal. As Popper (1989, 46) framed it, without an underlying theory, how would we know what to look for? If one follows what has been done conventionally, without taking any explicit stance towards this, one will be dependent on how others have conceived of what questions are of interest and how these can be answered. Furthermore, at no point in this process can our argument be falsified, and Smith (2015, 19) likens this exercise with 'the farmer who paints bulls-eyes around the bullet holes in his barn in order to show his superior shooting skills.' *Post hoc* accommodative arguments can provide the identification of empirical patterns with respects to the employed units of analysis, which in turn can form the basis for social and behavioural hypotheses. But Binford (1981, 85) has argued that such arguments can at best be 'treated as provocative ideas in need of evaluation'. Clark (2009, 29) states that a necessary next step is to derive empirical implications of this hypothesis, which can be evaluated against a part of the archaeological record that is independent from the material originally used to derive it (also Barton 2013). Subsequent testing thereby provides an opportunity to reveal if one's accommodative belief is wrong.

### 4.1.1 Confirmation

Within a classic hypothetico-deductive system, an initial goal is to derive as many empirical implications of an explanation as possible. These implications are then to be tested by comparing these implications to actual observed data. Drawing on Carnap's (1936, 425) 'gradually increasing confirmation', this entails that each time a model matches the data, the confidence that the model is true is increased. If, on the other hand, the model fails, it can be discarded as untrue. This should thus lead to the continual rejection of false models, and move us ever closer to, but not necessarily to, the actual model of reality. Although certainly an enticing prospect, there are problems related to this approach, irrespective of any goals of establishing covering laws.

A fundamental issue for hypothetico-deductivism, and scientific inference as a whole, follows from Hume's problem of induction (e.g. Ladyman 2002, 31–61). As an empiricist, all knowledge about the world was for Hume derived from sensory perception. Any reasoning that extend beyond observation, past or present, is based on cause and effect. However, since we can never observe a causal connection between events, the conjoined occurrence of observations is all we have to draw on. As there is no logical necessity for regularity in patterns to hold beyond what we can observe, there is no logical foundation for inductive reasoning – there is no logical connection between the observable and unobservable. While we might observe the sun rise every day, there is no logical contradiction in believing it will not rise tomorrow. Hume held that while inductive reasoning will continue to be fundamental to science, and our every-day lives, it has no logical justification. Following from the problem of induction, an issue for hypothetico-deductivism therefore pertains to the value of testing an hypothesis, and whether with successful tests our belief in the hypothesis should increase.

The logical empiricist attempts at working around the problem of induction and establishing a logical justification for confirmation was never successful, and a move to stating our beliefs in probabilistic terms never dissolved this fundamental issue. Central here is what is known as the paradoxes of confirmation (e.g. Sprenger 2023), of which Hempel's (1965, 12–20) own raven paradox is a classic example (see also Goodman 1984, 59–83). If the hypothesis is that all ravens are black, this is logically equivalent to the statement that if something is not black it is not a raven. If we were to observe a black raven, this is evidence in support of the hypothesis. The paradox follows from the second statement: Given their logical equivalence, the observation of a green apple would be evidence in support of the hypothesis. Paradoxically then, we can study ravens by looking at apples. While problems of confirmation such as this are simple, they have proven difficult to resolve and a logically sound justification for confirmation is yet to be found (e.g. Godfrey-Smith 2003, 39–56).

### 4.1.2 Falsification

One of the most influential contentions with the issue of testing is found with Popper and his concept of falsificationism. Popper, also a sceptic of induction, held that the problems of induction cannot be resolved. However, this is not of concern, as science in fact progresses not with confirmation but with falsification. In an attempt at demarcating science from non-science, Popper (e.g. 1989, 33–66) stated that a theory can only be considered scientific if it has the potential to be refuted by observation. A theory that is compatible with all empirical variation is unscientific. The test of an hypothesis should be aimed at falsifying it, not confirming it, and an hypothesis that is not proven false should simply be subjected to even more stringent and elaborate tests. It is with each new rejection of an hypothesis that science progresses and we learn something about the world. Although it will inevitably be falsified, a good theory for Popper is therefore one that is bold, risky and corresponds with the world in surprising ways. There are, however, further issues related to the fundamental prospect of confronting our beliefs with data.

As insight from complex systems theory demonstrates, sensitivity to initial conditions can lead both different causes to produce similar empirical results, and similar causes to produce different empirical results (van der Leeuw 2004:121; Premo 2010). This reflects the problems of equifinality and underdetermination, as presented above, where several models can agree on the empirical data, but disagree on the underlying causal mechanisms. This follows from the ubiquity of measuring error and the sensitivity of complex systems to minute variation. One classic example in this regard is the complex system of the weather, which can only be reliably predicted a few days into the future. Human systems are far more complex than that of the weather. Consequently, this renders the prospects of empirical confirmation or falsifiability weakened, and preference among different, even contradictory explanations, can often not hope to be based on observable data. In the case of archaeology, explanatory models are additionally faced with our generalisations of an already sparse and fragmented archaeological record, further increasing the likelihood that several explanations account equally well for the data at hand. However, this sensitivity to initial conditions can also impact the assumptions underlying an explanation. To show how this is an issue we can draw on what is known as the Duhem problem (after Duhem 1954[1914]), which states that nothing is necessarily learned from rejecting an hypothesis on the grounds of a test.

Drawing on Hvidsten (2014, 184–87), we may postulate a simple model holding that mechanism A, under assumption B, implies C. In a test in which A occurs, it would in a hypothetico-deductive understanding increase our belief in the model if we could then reliably measure that C is true. In the case of Popper, the model is simply yet to be falsified. If, on the other hand, C is not true, this would imply that either A or B are untrue. We would not, however, be able to derive logically which of A and B are untrue. This would perhaps not appear to be an immediate reason for concern. As long as one aspect of the model is untrue, the model is untrue, and should be rejected. The problem is that we know that models always contain a multitude of untrue assumptions. Drawing on the classic quote from Box above and the earlier discussion on abstraction, all models involve subsuming the virtual infinite complexity of reality and thus cannot work without an equal amount of untrue assumptions that could impact a test (a point made in the context of archaeology by Salmon 1975).

In exemplifying the Duhem problem, Ladyman (2002, 77–78) gives the example of testing Newtonian gravitational theory by observing the travel of a comet. The theory of gravity alone does not provide a prediction for this path. It also depends on factors such as the mass of the comet, the mass of other objects in the solar system, and their relative positions, velocities and initial positions, as well as Newton's other laws of motion. If the test was to fail, this failure can follow from an untrue hypothesis, but also from a misspecification of an assumption that is subsumed in the test – such as background conditions, measurement error, and initial conditions of the system. At some level a decision of whether the explanation has in fact been interfaced with observation is needed. As stated by Ladyman (2002, 80), 'falsification is only possible in science if there is intersubjective agreement among scientists about what is being tested.' While a severely complicating issue for falsificationism, as Popper also recognised, his proposition still holds if it is qualified by stating that for a hypothesis to be scientific, it has to have the potential to be refuted by some kind of observation. The challenge is determining what kind of observations this is (Godfrey-Smith 2003, 66).

Drawing on this issue with testing and falsificationism, several authors have argued that these factors alone do not determine the progression of science. A concern with understanding by what processes science has arrived at its current beliefs about the world is known as a naturalistic perspective. Philosophy of science should on this view not be concerned with establishing logically justified formalistic schemas for how research is to be conducted, but is itself best conceived of as an empirical study where scientific reasoning and argumentation is it's source material. While formalistic logic can provide important insights on what *can* constitute good components of strategies for scientific inquiry, such as aspects of Poppers falsificiationism, the scientific undertaking has been argued to be a far more messy enterprise. For example, the orbit of Mercury was not properly accounted for by Newtonian gravitational theory. It was not until decades later with Einstein's theory of gravity that this orbit was correctly predicted (Ladyman 2002, 89). Despite being unable to predict the orbit of Mercury, and thus being falsified, this did not cause the abandonment of Newton's theory of gravity.

Examples illustrating this point can be found in archaeology as well. For example, when new dates that dramatically push back the earliest human occupation in the Americas have been presented over the years (e.g. Holen et al. 2017; Parenti, Mercier, and Valladas 1991; Parenti et al. 2018), these have often been met with scepticism as related to their veracity, and geological and other non-anthropogenic alternative explanations have been proposed (e.g. Braje et al. 2017; Magnani et al. 2019; Agnolín and Agnolín 2023). How convincing an explanation is and what causes it to be abandoned thus clearly depends on more than data alone, not least because data is more than a simple binary category that is either observed/not observed. What data is accepted, what it is understood to represent, and if it is adequately confronted with a hypothesis is in part dependent on a decision by the person who observes and the wider research community. What we observe should to some degree dictate what we believe about the world. However, the examples above demonstrate that stringent empiricism is untenable and is not in fact how scientific insight is achieved.

## 4.2 Instrumentalism and scientific realism

Arguments such as those presented above have in sum rendered suspect an absolute demand and adherence to observable data, and presents a significant challenge to the prospect of testing our beliefs about the world. Following from (**levins1966?**), a defining element of modern MBA is consequently the realisation that models cannot at the same time maximise generality, realism and precision, which means models cannot and will never be constructed or evaluated purely on the grounds of observable data (Kohler and van der Leeuw 2007, 7; Lake 2015, 26). However, if we were to concede to the fact that all models are wrong, how can we ever trust model-based inference?

In a classical instrumental understanding, the goal of science should be the prediction of phenomena that matter (e.g. Hausman 1998, 187–90), a view famously forwarded by Friedman (2008[1953]). Whether prediction is achieved through the use of models that build on true causal mechanisms or not is irrelevant. As long as the predictions of the model has a satisfactory correspondence with the empirical variation of interest, it is deemed a success. This view is therefore compatible with the constraining realisation that all models are wrong, both because the truth of postulated causal mechanisms in and of itself does not matter, and because of the resulting relaxed demand for accordance with total empirical variation – degree of empirical correspondence determines the choice between models.

Related views have also been advanced within archaeology. The most clear example can be found in the domain of archaeological locational (often also termed 'predictive') modelling, concerned with understanding where archaeological sites are located in the landscape (e.g. Verhagen and Whitley 2012). These studies have sometimes focused on identifying where sites are located in the present-day landscape, irrespective of past motivations, so as to potentially reduce costs of land-development, or to help guide archaeological surveys in large areas where a complete coverage of the landscape is not possible. The concern then is knowing where sites are and are not located, not why.

However, one of the criticisms forwarded towards instrumentalism is that if the ultimate goal is manipulation of relevant variables for the improvement of society, this will depend on uncovering true causal mechanisms. While mere prediction depends on stable correlation, control necessitates causality (Hausman 1998, 190). As Elster (2015, 18) puts it, explanation demands causation, and causation can never be revealed solely through prediction (see also Gibbon 1989, 49). One way to conceive of causality is as dependent on a counter-factual condition (e.g. Lewis 1974; S. L. Morgan and Winship 2015, 4–6), simply stated as A causes C if when A occurs then C occurs, and if A does not occur then neither does C. Instrumentalism and a focus on prediction can therefore never hope to explain social phenomena (see also Lake 2015, 23–24). Of course, causal explanation does not necessarily have to be the main concern for archaeology. One could argue that academic interest in causal explanation should not always be the guiding principle behind archaeological inquiry but rather, for example, that mitigating costs associated with land-development or assembling interesting, albeit more speculative narratives about the past can be more important goals. My view in this context, as stated in the introduction to the thesis, follows from a form of realist understanding, where scientific inquiry is as a strategy by which we try to confront theoretical constructs with empirical observation, aimed at aligning our beliefs as reliably as possible with what is true (Godfrey-Smith 2003, 161), where the ultimate aim is to answer why something we believe to be true has occurred.

Scientific realism entails the philosophical position that there exist real observable and unobservable entities and properties, and that claims concerning the veracity of either dimension cannot be set apart (**psillos1999?**; Gibbon 1989, 142–72; Wylie 2002, 97–105). The goal is to reveal these truths, where truth typically follows a commonsensical definition of being determined by what is the case, and not, for example, what we believe to be true or what is most beneficial (Ladyman 2002, 157–58; see also Malnes 2012, 19–30). Regardless of whether or not it is possible to ever achieve, the goal of the realist is to reveal true, yet unobservable causal mechanisms that generate and shape the flux of observable phenomena. Scientific realism thus combines causal explanatory goals with ontological theses concerning the existence of observables and unobservables, and epistemological postulates on the possibility of gaining evidence for unobservables (Hausman 1998, 191).

In a realist view, even the most careful empirical approach depends on theoretical assumptions that will determine what hypotheses are deemed relevant, what evidence empirical data are believed to represent, and how these are evaluated against hypotheses (Wylie 2002, 100). With the early post-processual critique of processualism, Hodder (1984) argued that objective data is never tested against separate independent theories. These theories already underlie and determine how the archaeological material is recorded – there is no theory-free data. To the realist, however, the realisation that we might view the world differently does not take away from the belief that we inhabit a common reality that exists and is true independently of what we think about it (Godfrey-Smith 2003, 174). Shapin and Schaffer (1985, 355) stated that 'it is ourselves and not reality that is responsible for what we know.' This, however, is a false dichotomy. As human knowledge is a part of reality, not something outside of it, it is better to understand human knowledge as the result of both ourselves and the world (Godfrey-Smith 2003, 132). By extension, and by drawing on Fodor (1984), Godfrey-Smith (2003, 158–62) states that it is not enough to say that observation is theory-laden. The challenge is determining what theories influence observation, how they do so, and how reality manifests in observation.

As an extension of this view, the form of feminist empiricist perspective advocated by Longino (e.g. 1990) through her 'contextual empiricism' follows from treating the social group as the foundational scientific unit. What constitutes a good explanation in a field of research is determined by the varying views and non-coercive consensus that is reached on these issues at the level of the research community. As we view the world differently, what ideas are brought to bear on an issue, and a decision of whether a theory has been adequately interfaced with data will thus follow from the diversity of that community. Ultimately, this thus extends on aspects of for example Mill's (**mill?**) 'marketplace of ideas' (e.g. Gordon 1997) and Feyerabend's (Feyerabend 1970) 'proliferation of ideas' as scientific virtues (Godfrey-Smith 2003, 114). While there is a danger of simplistic generalisations of how for example sex differences influences how one views the world (Longino 1990,), a healthy state for a research community would thus be one where a multiplicity of marginalised and privileged groups are represented. For Longino (1990), this does not entail a retreat to relativism, but rather a pragmatic view on objectivity where claims are not (see also Chapman and Wylie 2016, 11).

## 4.3 What are models?

Building on the above, we can return to the issue of scientific models. While the classic hypothetico-deductive framework in a sense sees every model as a truth-candidate, they are for advocates of a model-based archaeology instead often understood as 'pieces of machinery that relate observations to theoretical ideas' (D. L. Clarke 2015[1972], 1). A similar view can be found with Morrison and Morgan's (1999) view of 'models as mediators', where a model is a concrete or explicit representation of observables and theoretical beliefs, and allows for a confrontation between these two dimensions. This is very much in line with the model as envisaged by Kohler and van der Leeuw (2007), who sees them as constructions that have similarities with, but exist independently of the target systems that they are to represent. Models are constructions used to draw further inferences about the reality they are to represent, and are construed on the basis of what mechanisms we believe shaped the observables available to us. What is studied directly is the model, in the hope that when confronted with the world, the mechanisms of the model that the researcher is interested in correspond with those of the target system. This is how models have often been cast in a realist understanding, and variations on this are sometimes termed credible worlds, or idealised or isolating models (Gilbert 2008; Frigg & Hartmann 2018). These entail the inclusion of boundary conditions or assumptions considered essential for the model to function, the explicit or silent omission of aspects deemed unessential, and can involve an exaggeration of the characteristics of interest (**maki2009?**).

To explicate the concept of models as mediators, it can be useful to think in terms of an epistemological hierarchy, extending from observations to high-level theory (see Smith 2011, 2015). In a Mertonian view (Merton 1968), this extends from day-to-day working hypothesis of what data represents, to middle-range theories that act as bridging concept for casting these as a more comprehensive high-level social theories (Raab and Goodyear 1984; Smith 2015, 22; see also Lucas 2015 for nuances on this). High-level theories can in this view be understood as 'overall perspectives from which one sees and interprets the world' (Abend 2008, 179), with examples frequently encountered in archaeology being practice theory, cultural evolutionary theory, and so on. Popper was concerned with establishing how Marxism and Freudian psychoanalysis were unscientific, as they are compatible will all empirical variation. However, it would seem misguided to try to falsify (Godfrey-Smith 2003, 71). Marxism cannot be falsified, but a given instantiation of Marxism – a Marxist model on the view taken here – should risk exposure to observation and have the potential to be falsified. One way to see models is thus as bridging concepts representing concrete instantiations of abstract theories, and as machinery for casting data as evidence to be confronted with these theoretical constructs.

In a realist conception of models, these can thus be seen as analytical tools, the purpose of which is to provide a concrete representation of the researchers beliefs, used to isolate or create a closed and credible surrogate system where causal mechanisms are allowed to work without impediment from surrounding noise (see Sugden (2000); Sugden (2009); Cartwright (2009); (**mäki2009?**) for variations on this). The aim, according to Cartwright (2009), is to reveal the capacities and differential contributions of unimpeded causal effects within such an idealised structure. However, this does not mean that the causal contribution is necessarily stable outside the surrogate system. In an open target system, the complex interplay of several causal mechanism can render the contribution from the modelled causal effects completely transformed, compared to their role in an idealised surrogate system (Gibbon 1989, 150). Although stable correlations can point to the possible existence of a causal relationship, the relevance of the realist study of capacities, unlike positivist regularities, does not presuppose closed target systems (Groff 2004:12–16). Positivism necessitates a closed system with regular conjunctions between events, such that an event of type A is always followed by an event of type B (Gibbon 1989, 149). Cartwright (2009) contends that even though the realist surrogate system is credible, in the sense that the mechanisms could conceivably occur and result in the phenomena in question, the system is almost always different from all real cases in ways that matter. Drawing on the oft-invoked *ceteris paribus* statement – all other things are in fact not equal (cf. Cartwright 2003[1983]:44–47) – all models are wrong. The confrontation of model and data can therefore never avoid the problems of induction, and the question of interest then is not whether the model is true or false in its entirety, but if the model resembles the world in the relevant dimensions, given its purpose (K. A. Clarke and Primo 2007, 747; Kohler and van der Leeuw 2007, 3).

For all the ambiguities nested in the above account of what can be taken to constitute models, a central element is the view that they are constructed and explicit representations of our beliefs. Precisely this is also central to the contention that one of the most important aspect of model-based approaches follow from their explorative side (**hausman1992?**; Aydinonat 2007; **premo2010?**). This results both from the assembly process itself, and from subsequent probing and manipulation of the model (Morrison and Morgan 1999). In the initial construction of a representation of theory and data, the researcher is forced to concretise their assumptions and beliefs. This will likely lead to the adjustment of inconsistencies, the discovery of additional theoretical implications or relevant empirical patterns, and increase the opportunity for explicit handling and reporting of uncertainty. Through stringent and explicit aggregation of model features, further theoretical and empirical consequences are also likely to be revealed. Thus, in its construction, the model will already have provided valuable insights, regardless of its future archaeological life-span. Even so-called caricature models that are wildly unrealistic, extreme distortions have been argued to generate such insights (Gibbard & Varian 1978).

Following its construction, further insight can be achieved through direct manipulation of model parameters and assumptions (Morrison and Morgan 1999, 32–35). This holds the potential of revealing additional causal propensities and limitations that are difficult to reveal by passive study of the model, and can reveal how sensitive it is to such adjustments (**premo2010?**). It has been argued that the potential of mathematical and computational models to stringently and coherently aggregate a multitude of mechanisms and allow these to interact over time, means that these can reveal unnoticed or counter-intuitive aggregate effects (Aydinonat 2007), in effect generating new evidence (Wylie 2017). The same exploratory potential is then extended by any attempts at evaluating the correspondence between model and target system, and by the involvement of an audience that comments, criticises, dismisses or helps align model and target system (**mäki2009?**).

## 4.4 Inference to the best explanation

So far induction has here been used to denote all non-deductive reasoning, and been exemplified by what is sometimes termed its enumerative or statistical form. That is, induction as the repeated observation of conjoined phenomena. However, other forms of non-deductive inference exist. Archaeology is often, if not most often, concerned with explaining singular or infrequent events, and not generalisations where an appeal to enumerative induction is possible. Clearly then, other lines of reasoning can be drawn on to arrive at and choose between alternative explanations. One such form of inference has been variably labelled abduction, explanatory inference or inference to the best explanation (Godfrey-Smith 2003, 39–44; Harman 1965; Lipton 1991). Lipton (1991, 58) formulates this mode of inference simply as 'Given our data and background beliefs, we infer what would, if true, provide the best of the competing explanations we can generate of those data.' (Fogelin 2007, 604). Scientific realist often lean on this mode of inference to provide a way around the problems of induction and underdetermination, and this has been argued to constitute a good and often inadvertently employed framework for archaeological inquiry (e.g. Fogelin 2007; Campanaro 2021).

Fogelin (2007) argues that despite the theoretical differences that exists among archaeologists, inference to the best explanation is often the logic underlying their conclusions. For example, he demonstrates how when providing an explanation for smudge pits, a common archaeological feature in Eastern United States, Binford (1967) draws on ethnographic analogy to arrive at an explanation that is better than any alternative explanations he can muster (Fogelin 2007, 611–12). Despite using deductive-nomological language, Binford never independently tests any deductively derived hypothesis, and he arrives at his conclusion, Fogelin (2007, 612) argues, because it is the explanation among the alternatives that corresponds with the widest breadth of relevant empirical data. Similarly, Hodder (1991), after having abandoned his most relativistic stance, adopts what he terms a 'guarded objectivity' through an appeal to hermeneutics. This starts with the context of the archaeologists themselves and their pre-existing beliefs and underlying theories, which is opposed to the context of the people responsible for the archaeological material available to us. By moving back and forth between such context and trying to cast our data in the light of these, the goal is to adjust an interpretative whole until the two contexts coalesce. The process is thus one of iteratively fitting empirical pieces within an interpretative whole, that is at the same time adjusted by these pieces. In this framework 'We measure our success in this enmeshing of theory and data (our context and their context) in terms of how much of the data is accounted for by our hypothesis in comparison to other hypotheses.' (Hodder 1991, 8). This is arguably also an appeal to inference to the best explanation (Fogelin 2007, 612–14).

Central here is that hypotheses have been argued to be best evaluated when comparing them to the ability of substantive competing alternatives to fulfil the same purpose, and not just their negation, the null-model (e.g. Smith 2015; Wylie 2002, 95; Perreault 2019, 1–22). Pitching alternatives against each other will lead away from a pure search for corroborative evidence for a single hypothesis, and will, following from Chamberlin's (1897) 'method of multiple working hypotheses', help the researcher avoid 'a pressing of the theory to make it fit the facts and a pressing of the facts to make them fit the theory' (Chamberlin 1897, 843; see also Platt 1964). This thus avoids one of the dangers of *post hoc* accommodative arguments, which has been argued to be explanatory complacency and personal attachment to individual explanations (Smith 2015; see also Elster 2015, 12).

However, if one arrives at hypotheses that account for the data equally well, then other criteria will determine what is the best choice among them. A first criteria pertains to explanation, where a realist would hold that an hypothesis that makes claims about what has caused an empirical pattern will be given preference over an hypothesis that does not. If a locational model says that sites tend to be located close to rivers, and another explains this with reference to a specific kind of resource exploitation practice, then the second hypothesis would be given preference. This follows from the additional empirical implications this causal explanation holds, thus potentially increasing it's explanatory breadth and increasing it's falsifiability. Other virtues of a good explanation has been argued to be

## 4.5 Archaeology, evidential scaffolding and models

Theoretical discussions in archaeology have often framed the field as situated at extremes of positivism and relativism, or humanistic and scientific ideals, harking back to Snow's (1959) distinction between 'The Two Cultures' in western academia (e.g. Earle and Preucel 1987; Sørensen 2017). However, Chapman and Wylie (2016) and others (see below and Fogelin 2007, above) have argued that this perspective does not inform how archaeology has in fact progressed, nor that it constitutes a good reference frame for understanding how to do good archaeology. This is not to say that these discussions cannot hold important points for elucidating the nature of our inferential frameworks, or that theoretical stances do not influence what questions are deemed of interest and how the material record is approached. Rather, this then naturalistic argument is that these discussions are over-simplified, hyperbole and largely unrepresentative of an archaeology that generally progresses by drawing on a far more complex web of theoretical and philosophical influences (see also Johnson 2006; **pearce2011?**; **preston2014?**). Therefore, the extremes of insisting on trying to establish deductively certain knowledge or a whole-sale rejection of the possibility of ever moving beyond speculation does not represent an adequate reference frame for understanding what constitutes good archaeology, how to conduct it, nor how consensus and synthesis on claims about the past have been arrived at in the past.

As (**preston2014?**) states, archaeology 'is intellectually *distinctive*, even if not intellectually *unified*. The latter, I would argue, is an inappropriate goal for the discipline'. Considering the complexity of the questions we deal with, it is difficult to argue that this eclectic state of affairs is anything but positive. Furthermore, accepting this appears to be a far more fruitful way of trying to understand archaeological practice, rather than insisting on casting this in some dichotomous light that poorly captures how the archaeological enterprise is actually undertaken. Moreover, embracing eclecticism hopefully leads to a research-environment were practitioners are open to dialogue and are not overly committed to any single approach and dismissive of alternatives (Hegmon 2003, 233).

Given the realisation that we lack an infallible logical foundation with which to establish explanations, Chapman and Wylie (2016) speak for an iterative epistemological process where a temporary scaffolding for how data is cast as evidence by drawing on multiple methodologies and lines of reasoning is continuously adjusted, extended and reassembled. Crucially, these scaffolds are to be subjected to critical reflexivity, but be grounded in domain-specific norms of what constitutes evidence, so as to tackle what Binford (1981, 21) presented as the challenge of 'how to keep our feet on the "empirical" ground and our heads in the "theoretical" sky.' (Chapman and Wylie 2016, 8). They further draw on Norton (2003) to argue that progression in science has been achieved mainly through the domain-specific development of robust reference frames for grounding further inference, not through the development of increasingly sophisticated universal inferential schemas (Chapman and Wylie 2016, 39).

By drawing on Toulmin (1958, 213), who argues that we should 'abandon the ideal of analytic argument' and the goal of deductive certainty, a central component of Chapman and Wylie's (2016, 36–37) argument is illustrated by a quote from Toulmin (1958, 37): 'The proper course for epistemology is neither to embrace nor to armour oneself against scepticism, but to moderate one's ambitions – demanding of argument and claims of knowledge in any field not that they should measure up against analytic standards but, more realistically, that they shall achieve whatever sort of cogency or well-foundedness that can relevantly be asked for in that field.' Important here is therefore that there is no universal recipe for inferential adequacy, but that inference is domain specific. What we can hope to achieve is that our inferences are credible, but this limitation should not entail a regress into whole-sale scepticism. The goal is to arrive at beliefs that are more reasonable to trust than doubt, without demanding that they should be infallible and beyond critical scrutiny.

Building on the theoretical plurality of archaeology, and echoing the point made by Godfrey-Smith (2003) referenced above, Chapman and Wylie (2016, 41–43) argue that theory-ladenness will differentially impact what archaeologists consider evidence. Some biophysical observations will be relatively transferable between contexts, and their role as archaeological evidence less integrated with theoretical preconceptions. Inferences to do with symbolic behaviour is less transferable as they will be less secure and more contingent on the given cultural context and the evidential scaffolding surrounding them to be considered evidentially adequate. However, this does not mean that symbolic behaviour is in any sense more off-limits than for example chronological inferences that draw on radiometric dating. Neither can reach deductive certainty, and their role as evidence for past events is differentially dependent on the warrants and assumptions that underlie them (Chapman and Wylie 2016, 42).

Thus, while the inferential virtues outlined above can constitute some guiding principles for how to arrive at good explanations, I would also take Chapman and Wylie (2016) to mean that these cannot be schematically and universally brought to bear on archaeological explanation. Different questions will necessitate different evidence, and different evidence will necessitate different warrants. What Wylie (2017; Chapman and Wylie 2016) has held as a central component of evidential scaffolding is that these should be robust. In his review of Chapman and Wylie (2016), Currie (2017b) likens this with the view of Cartwright (2015) who prefers arguments that are 'short, stocky and tangled' over 'tall and skinny' arguments that are elegant and tidy. That is, at the price of complication, a diverse and broad evidential foundation is more secure than an elegant but fragile chain of evidential premises (see also Currie 2017a; Bayliss and Whittle 2015).

The understanding of archaeological inquiry outlined above need not be cast within a model-based understanding. The term model has been noted to increasingly involve aspects that were previously seen as a domain of theory (**preston?**), and their role has conceived of here also relates to other bridging concepts such as middle-range theory and evidential scaffolding. Furthermore, it has also been argued that models are best understood as a separate and distinct kind of reasoning (Godfrey-Smith 2009) and that models should not be conflated with all kinds of 'representational vehicles'(Godfrey-Smith 2003, 186–89).

However, despite its ambiguities, I still believe the model terms offers a sensible way of thinking about the issues dealt with in thesis. It forces us to see explanations as fallible explicit constructs, which are thus both more easily interrogated by others and less likely to lead to an explanatory complacency. Furthermore, this conrectisiation and fallibility is also very much compatible with the ideals of open science, concerned with

Given the case where multiple models achieve empirical adequacy, that is, they predict the empirical pattern of concern equally well, realist have argued that inference to the best explanation is the best way forward and that this represents a way to handle the unavoidable problem of induction. While these might be sensible guiding principles for arriving at good explanations. Chapman and Wylie (2016, 7) as aims at 'establishing claims that are empirically irreproachable they may foreclose (some) risk of error but at the expense of abandoning the very questions that make archaeology worth doing, and if they do not self-limit in this way they may have nothing to offer but speculation.'.

What are models? "It is […] more appropriate to describe models than to attempt a hopelessly broad or hopelessly narrow definition for them" (D. L. Clarke 2015[1972], 2)

Following Clarke, no conclusive definition of models is sought here. The concept is notoriously difficult to pin down as its use varies across different disciplines, between various authors, and a multitude of classificatory schemas of model types have been proposed (e.g. Hausman 1992; Morrison & Morgan 1999a; Gilbert 2008:5; Godfrey-Smith (2009); K. A. Clarke and Primo (2007)). Nonetheless, some possible understandings of the term are presented below, mainly in order to set up an understanding of modelling in relation to the practice of archaeological inquiry and to understand what could demarcate model-based archaeology (MBA) from other forms of archaeological inference.

Godfrey-Smith's (2009, 102) understanding of a model-based style of science sees this as a distinct approach, starting at remove from the phenomena, or target system, of interest. A model-based approach is in this understanding thus different from an analysis that starts by trying to describe and understand the system under study. Instead it begins with the exploration of a hypothetical, fictional model, simplified and largely independent of the target system, before this is ultimately interfaced with empirical data, involving thus 'a deliberate detour through fiction' (Godfrey-Smith 2009, 103). As the modeller in this instance would be in control of all parts of the artificial model and their interactions, it is in the empirical confrontation that the model can yield the most fruitful results, either by capturing the empirical variation of interest or by allowing for an exploration and understanding of where it fails.

A realist understanding holds that unobservable theoretical constructs should be conceived of as true mechanisms and events, and that these should not be distinguished from observables. In a realist view, the progression of science is not achieved by a random and undirected trial-and-error search by exploring empirical patterns, nor is it achieved by rejecting relevant hypotheses only on the basis of empirical adequacy. Our beliefs concerning unobservables shape how we observe, order and confront theoretical constructs with empirical data.

Why model?

The last chapter laid out the analytical framework that was used when thinking about these issue. The last section. This was not drawn on directly in the papers, but form a good frame both for considering their contribution and for setting up some future avenues along wich these could be explored.

# Chapter 5

# Modelling the Norwegian Mesolithic

The last chapter laid out the foundation for what can constitute components of a model-based archaeology. This chapter will explore how casting the papers of the thesis in this light can help elucidate assumptions and further lines of inquiry associated with the arguments made in the papers. Each paper is first presented using an evidential argument schema, following Toulmin (1958; see Chapman and Wylie 2016). Subsequently, a suggested causal model for the main components of each paper is presented in the form of directed acyclic graphs (e.g. S. L. Morgan and Winship 2015).

## 5.1 Evidential argument schema

In the presentation of the papers below, these schemas will not be complete, but draw on what I view as the most central components of the arguments. Further nuances and caveats can be found in prose in the papers themselves, while the data and code published with each paper also offer further sources that can be scrutinised for additional underlying assumptions and potential inconsistencies.

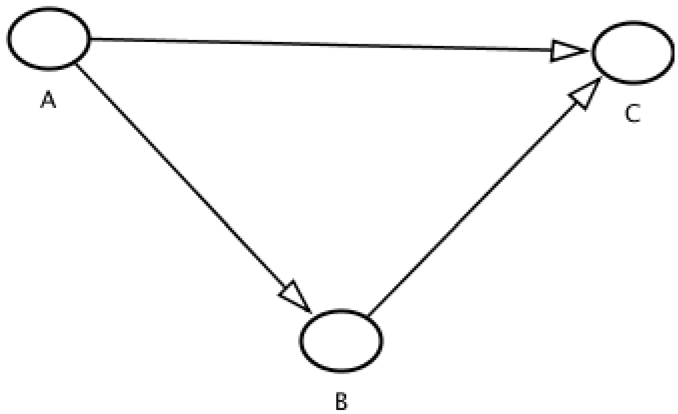


Figure 5.1: Example of a simple causal model presented as a directed acyclic graph.

## 5.2 Directed acyclic graphs

## 5.3 Modelling the relationship between Mesolithic sites and the prehistoric shoreline

In the first paper of this thesis I have proposed a method for shoreline dating Mesolithic sites on the Norwegian Skagerrak coast, based on an empirically derived model of the relationship between the sites and the prehistoric shoreline (Roalkvam 2023). This was based on simulating the distance between sites and the shoreline using 67 $^{14}$C-dated sites and local reconstructions of shoreline displacement. The study found the sites to typically be located on or close to the shoreline up until some time just after 4000 BCE, when a few sites are located further from the shoreline. At around 2500 BCE there is a clear break, and the sites are from this point on situated further from and at variable distances from the shoreline. Building on these findings, the likely elevation of sites dating to earlier than 2500 BCE were, in aggregate, found to be reasonably approximated by the gamma function given in Figure. This is the model that forms the foundation of the proposed method for shoreline dating that is released as an R package with Paper 2.

In one sense this model is instrumental as the *reason* for the location of the sites has not been considered explicitly. By combining the present altitude of a site, its likely elevation above the shoreline when it was in use, and local shoreline

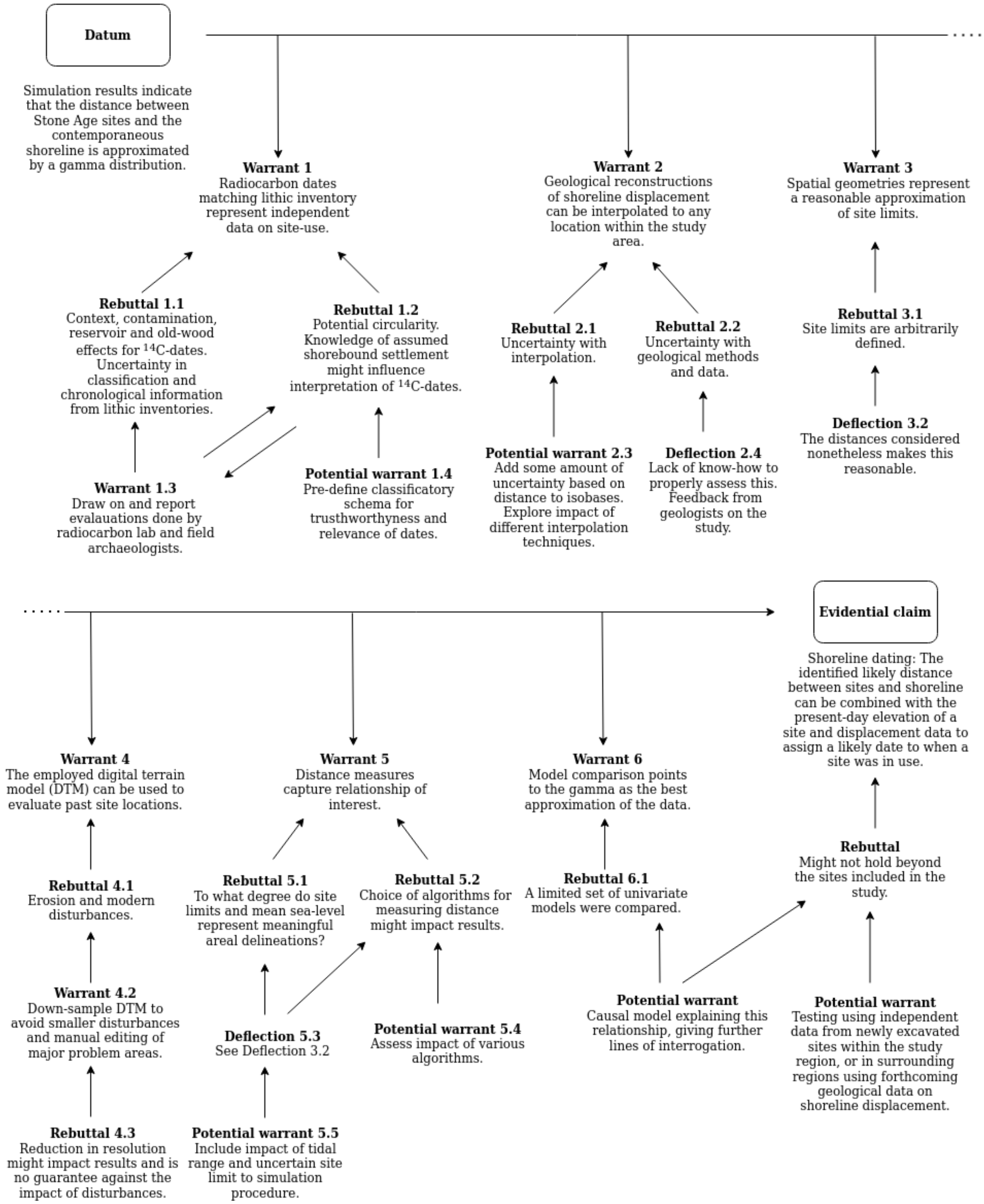


Figure 5.2: Central evidential arguments underlying Paper 1 presented as a Toulmin-like argument schema inspired by Chapman and Wylie (2016).

displacement curves, this model makes it possible to assign a probabilistic absolute shoreline date to coastal sites in the region. On a realist view, however, it is still true that the treatment of the data and the conception of the model followed from an underlying belief of what mechanisms shaped the patterns in the data deemed relevant. While the model and derived method can be viewed as a instrumental dating tool, they are determined by the proclivity for sites to be located on the shoreline. As such, they are likely to be tightly integrated with both overarching cultural developments, as well as behaviour at the site level. By extension, the multitude of factors that can have shaped the site-sea relationship on the large and small scale, both temporally and spatially, offers a challenging causal web of possible interacting effects that ultimately determine this relationship. Having first derived this largely instrumental model, it gives opportunity to further test it's correspondence with other empirical data, and explore and expound underlying theoretical assumptions and implications.

To illustrate this, below I have constructed a suggestion for a causal model concerning what determines the vertical distance between coastal Mesolithic sites and the shoreline in south-eastern Norway. The direction of the arrows in the model illustrates what variables are believed to impact other variables. An arrow going directly between two variables means that there is a direct effect. If there is a direct effect between A and B, but variable Z also impact each of these, Z is said to be confounder. An arrow from variable A to B that go through one or more other variables indicate that the effect of A on B is mediated by the intermediate variables. A central element of this model is that the effect of the other variables on the distance between site and shoreline are all mediatated through the exposure of the site to the surroundings and accessibility to and from the site.

A likely important factor for how exposed and accessible a site could be is the purpose of the visit to the site. The purpose of the visit is therefore given a direct effect on exposure and accessibility. For example, is the site meant to be used as a stop to rest and repair tools, to be used as a hunting camp or a location from where to acquire raw-materials for tool-production? Is it a base-camp for the entire residential group from where further forays are made, or is it meant to be a meeting place for several groups? The purpose of the stay is likely also to impact the length of the stay, which in turn might have implications for how close to the shoreline the site is established. A longer stay could for example mean that the site is more withdrawn from the shoreline, so as to make sure storm surges do not reach the site.

Means of travel is also included in the model. Most travel is assumed to be done by boat in this period, which means accessibility to the site from the sea is likely to be of concern, as well the ability to safely beach and store the boats. However, some travel was also likely done by foot, for example from a base-camp to a site close by for gathering and processing resources such as shellfish, where the need for the carrying capacity offered by boats might not have been necessary. Travel by sledge on the ice is also a possible alternative. Not having to land boats could

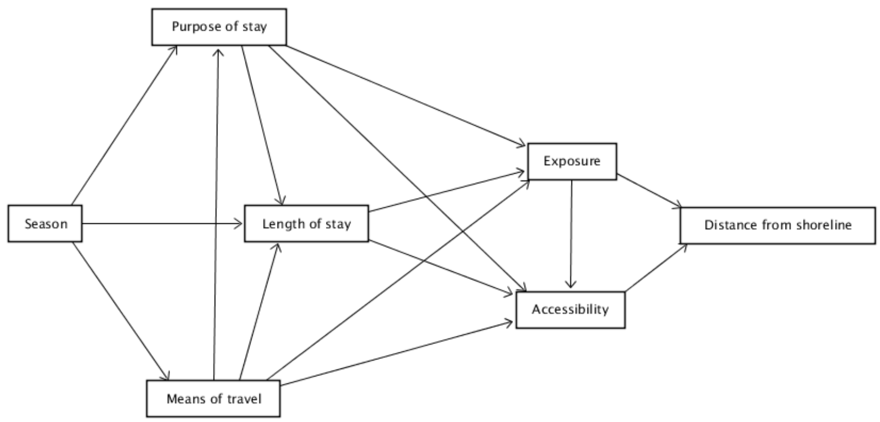


Figure 5.3: Sugggested causal model for the drivers behind the relationship between site location and the prehistoric shoreline in Mesolithic south-eastern Norway.

presumably have implications for how exposed and accessible a location could be.

The season also presumably has implications for how often one had to establish camp, possibly reducing mobility in colder periods. The season might also have implications for the kinds of dwelling structures that were necessary to erect, and likely determines the kinds of resources that were exploited, thus potentially impacting the purpose of the stay. The season is also believed to have implications for the degree of wind and wave-action at a location, thus affecting the exposure of the site to the elements, and impacting accessibility. Season is therefore given a direct effect on all of these variables.

Some variables and nuance that have been left out of the model are worth commenting on. The weather is for example likely to impact a lot of these factors, but is near if not entirely impossible to determine archaeologically. Furthermore, the purpose of the stay is here indicated using a single variable, but a stay need not, or perhaps likely did not have a single purpose. A simple example might be a case where multiple kinds of resources were to be exploited from a site. A possible alternative would be to operationalise these as individual variables, where for example the magnitude of seal-hunting and the gathering of hazelnuts to be done from the site is kept as separate variables. These would in turn likely be determined by factors such as the density of these resources in the landscape, their caloric return, their cost in terms of handling-time and -energy, and the potential prestige associated with hunting a specific species or the accruement of enough food to allow for sharing. Furthermore, the entire picture is also further complicated by other latent variables that are left of the model. Social structure, overarching mobility patterns, territoriality, group size and composition, as well as religious beliefs could all impact land-use, site-structure and ultimately how sites were positioned relative to the sea.

However, I still believe the model forms a reasonable starting point and that it has the potential to reveal some important and true causal determinants for the site-sea relationship. A central challenge is of course how these factors are to be operationalised and determined archaeologically. The exercise of setting up the causal model is nonetheless useful in its own right, if not simply by forcing me to think through and concretise what elements I believe are important and how these are related. It also forms a framework that dictates how these variables would have to be handled statistically. Furthermore, with reference to the concept of inference to the best explanation presented in the last chapter

Some assumptions concerning the directionality of influence between the variables are made here, which might be discussed. For example, it is assumed here that the length of the stay influences how exposed the location is. This in a sense places primary weight on the planning of the inhabitants. One could envisage a situation where arrival in fair weather leads to a case where a worsening of the weather could prove that the location was in fact too exposed, and the site is moved. The purpose of the visit did in this case determine what was initially an acceptable degree of exposure, but, although not modelled here, the purpose of the visit might change with the weather.

Focusing first on its instrumental value, shoreline dating will often provide the highest resolution date that one can hope to achieve for a site, given that material to radiocarbon date is quite rare due to taphonomic loss, and as established typological frameworks in the region operate on the millennial scale. By facilitating a dating method, the model can thus be drawn on to explore traditional long-term chronological questions, such as the frequency of sites throughout the period (**roalkvam?**), the assessment of typological frameworks, or the timing and spread of various cultural phenomena, to give some examples. Furthermore, the finding that sites tend to be located further from the shoreline from around 4000 BCE and 2500 BCE, correspond with major socio-economic developments, where these dates roughly correspond to the first introduction and subsequent firm establishment of agriculture in the region. Although it still remains to be tested on data independent from where it was derived, the instrumental utility of the model is therefore clear.

However, as it is difficult to determine the longevity of use and re-use of the open-air sites that dominate the Mesolithic record in the region, the number and duration of settlement events being dated in each instance is not clear. Previous consideration of these questions typically range from characterising the sites as the result of short visits of only a few hours up to a few months, and range from single visits to seasonal re-visits over a few decades—possibly centuries in the most extreme instances. However, given the resolution that shoreline dating provides, even at its best, the method does not by itself provide a precision high enough to weigh in on this issue, and can therefore not inform the number or length of stays within the determined date range. This has implications both for the questions that the method can be used to answer, and causal drivers behind site location that we can hope to disentangle using the model, given that these dimensions are likely to have been of importance for the location of the site relative to the sea. (e.g. Bailey 2007; **perrault2019?**)

Having established the model also opens up for a shifting of perspective back and forth from the large to the small scale, and from shoreline date to site location relative to the shoreline. Such an approach can illuminate implications of the model and its workings, and, in turn, potentially also feed back to and lead to a refinement of the model. One such example is now given by considering the location of the site Pauler 1, relative to sea according to the model.

The mean elevation of the site is masl. Based on the likely elevation of the site when it was in use, as informed by the shoreline model, the resulting shoreline date is. The benefit of having a model where all three parameters are clearly defined is that this allows us to shift perspective between the date of the use of a site and the implication this has for the relative position of the shoreline. Thus, looking now instead back from the calendar scale (the x-axis in Figure), to the likely elevation of the site above sea (the exponential function on the y-axis in Figure), and combining this with the elevation of the sea-level above it's present altitude at this time (represented by the displacement curve in Figure)–effectively rearranging the equation–thus allows for an instantiation of the model implications for individual sites in the spatial domain. In Figure, this is done by simulating the sea-level that the shoreline date implies.

Given the commutative nature of the relationship between shoreline date and the elevation of the site above sea-level, it is possible to translate directly between these dimensions and treat the resulting sea-level

The model, while a reasonable approximation of the relationship between sites and shoreline in aggregate, could still be substantially off when applied to individual sites, as was demonstrated here by the in-depth analysis of Pauler 1. However, this model failure has to be qualified. First, the articulation and exploration of *how* the model is wrong has allowed for a further understanding of both the site and the model. Furthermore, this can also function in a step towards generating causal models explaining why, in any given case, the site was located as it was. While an immensely challenging task, this would

The concept of shoreline dating touches upon such a wide range of issues that it can in sense be seen as a microcosm of archaeological inquiry as such. While physical sciences underlie the framework, as it is ultimately dependent on reconstruction of shoreline displacement, the question is inherently social and cultural. Furthermore, perspectives from a vantage of social and humanistic can not only be used to derive cultural significance from the observed patterns, but these can also be used to further improve the method for shoreline dating. My view is that this is defining of archaeology as a whole. While here nested in model-based archaeology, which I found a useful framework with which to think about these issues, I also think this illustrates the heterogeneous nature of archaeology and the value of drawing on multiple strains of evidence and perspectives. The iterative move between aggreagte model and individual cases could just as easily have been cast within a hermeneutic understanding of archaeological research. This also highlights the inadequacy of attempting to understand archaeological research as being situated somewhere on a scale between more or less scientific or humanistic, more or less processual or post-processual, as these are simply unable to capture the necessary nuance of archaeological inquiry.

# Chapter 6

# Conclusions and future directions


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