

# **Accessing intangible technologies through experimental archaeology**

## **A methodological analysis**

Submitted by Tine Schenck, to the University of Exeter as a thesis for the degree of *Doctor of Philosophy in Archaeology*, November 2015.

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"In reconstruction after reconstruction, this same structural pattern surfaces: technology is serious material business but not necessarily social or enjoyable."

*Marcia-Anne Dobres 2000:21*



## **Abstract**

This thesis concerns the relationship between research in experimental archaeology and the intangible of the past. Only a quarter of technological experiments in a sample of 100 studies addresses the intangible of technological practice, and this project sets out to explore if there are conceptual or practical obstacles for this low rate.

The thesis begins with an in-depth examination of experimental archaeology and the criteria, paradigms and theories that determine its practice. Through this study, elements of the dichotomy positivism//postmodernism are uncovered and discussed. To resolve this dualism, a third paradigm – philosophical pragmatism – is introduced as an alternative. This conceptual debate represents Part I, and is subsequently collated into a methodological framework for the creation of a typified experiment.

Part II consists of the experimental segment of this study, in search for practical obstacles for the exploration of the intangible. Through experimenting with Iron Age Bucket-shaped pots, Mesolithic faceted pebbles and Middle Palaeolithic birch bark tar production, different components of an experiment are highlighted for investigation. An element that comes forward as problematic is the relationship between experimental archaeologists and science ideals that is underscored by experimental tradition. Conclusively, the final discussion leaves the conceptual and practical barriers that may prevent archaeologists from studying the intangible aspects of technology overturned. In sum, this may enable experimental archaeologists to take a fuller view of their own practice and that of the people of the past.



*For Kari Christine  
In Memoriam*



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## Abbreviations

AM	Abductive mode
AM/UiS	Archaeological Museum, University of Stavanger, Norway
BCE	Before Common Era [before 0]
BP	Before Present [Years before 1950]
CalBP	Calibrated BP [Years before 1950, calibrated after scientific analysis]
CE	Common Era [after 0]
GC/MS	Gas chromatography/mass spectrometry
EDX	Energy dispersive X-ray spectroscopy
HDM	Hypothetico-deductive mode
IM	Inductive mode
IMRAD	Writing outline model derived from Science: Introduction, Method, Results, Analysis, Discussion.
KHM	Museum of Cultural History, Oslo, Norway
TPQ	Terminus post quem [Occurred after]
SEM	Scanning electron microscope/microscopy
UoE	University of Exeter
UNIMUS	The collated web-portal and central database of portable antiquities of Norwegian University Museums ("Universitetsmuseenes samlingsportal")
VSM	Vikingskibsmuseet/Viking Ship Museum, Roskilde, DK
X-AT	Exeter Advanced Technologies, College of Engineering, University of Exeter
∅	Diameter

*All photos and illustrations by the author unless otherwise noted.*

# 1. The intangible, the technology, and experimental archaeology - an introduction

## 1.1. The study of the intangible in experimental archaeology

This thesis concerns intangible technologies and experimental archaeology. This combination currently seems unfashionable, as illustrated in the literature: A browse of the first 80 articles on Google Scholar with the search words “+intangible” “+technology” “+archaeology” yields 67.5 % cultural heritage protection publications and 1.25 % publications in experimental archaeology since 2011<sup>1</sup> – one article (Foulds 2013b). However, what seems less clear is what either 'intangible' or 'technology' is, whether the concepts are at all definable, and if their definitions bear any particular relevance to experimental research. The latter is the very foundation for this thesis and raises a number of issues in itself. Even so, before the analysis of these issues can begin, it is necessary to better explain and define the concepts of intangibility and technology.

Let us consider ourselves as a community that may come under archaeological scrutiny, and that an archaeologist of the future will excavate a church. In the ruins, the archaeologist finds a chalice, a screwdriver and one flower vase. The present day priest keeps the chalice on the altar, the janitor has a screwdriver lying around, and someone forgot to clear away the flower vase after a wedding service. The chalice, screwdriver and vase each have their use, although unrelated. One is a holy artefact, occasionally used for ritual drinking, one is a flower-holding vessel used when flowers adorn the church, and one is considered a basic tool for managing screws. Although they have vastly different purposes, the artefacts are in existence in the same localised community connected to the church. The screwdriver is often used to screw screws, but also to open paint cans and other odd jobs where one might need

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1. Search executed on 10/8/15.

some leverage. It is, in a way, a multifunctional tool, although it has one foundational purpose. Although it would be perfectly fine for the task, very few western citizens use it for spreading butter on a slice of bread. It is just the way it is supposed to be. Equally, the vase has its very specific purpose, and although it would be very useful as a drinking vessel, it is usually not considered right. Additionally, the vase is generally created to be a thing of beauty, whereas the screwdriver is generally not created for beauty. Also, the vase may be worth a lot more metal coins and pieces of paper (money) than the screwdriver, but this is often determined by brands. The chalice – although perfectly fine for holding flowers and it being a beautiful thing, is in addition *holy*. Exactly what that concept constitutes is hard to explain, even for archaeologists of today, who seem rather familiar with it, as represented by the extensive literature of religion and symbolism. The chalice is also likely to be older than the other items, and worth the most money, but the brand may be less important in this matter. If someone would use the flower vase to drink with, it would probably be laughed off and considered improper, but if someone puts flowers in the chalice, the paradoxical use is also improper but very possibly no longer laughed off. The principles that steer the notions of use, beauty, value and holiness, are intangible. They are real, we experience them every day, but they are not tangible – they cannot be experienced by touch, and sometimes not by any sensory input at all. The archaeologist of the future may understand some of them instantly, but the vast amount of these intangible relationships that exist within a church will need intensive research to grasp.

When something is intangible, it is bodiless. Many things can be intangible, and it is hard to generalise exactly what they have in common. Social constructs are intangible, such as purpose (Fredriksen 2006), storytelling (Comis 2010), or tradition (Haas-Lebegyev 2014). They can also be the premises we rely on for our existence in the world, such as cognition level (Wadley 2013), skill (Apel 2006), and feeling warm (Liedgren and Östlund 2011); or it can be personal qualities such as liking a taste (Šálková et al. 2011), musicality (Cross et al. 2002) and dexterity (Liardet 2013). Although it may be difficult to describe, the reality of the intangible is hard to dispute. The intangible, non-physical, parts of societal structures bear vast influence on any given community. Societies are steered by structures we cannot see, beliefs we cannot articulate or customs we

apply indifferently. Intangible cultural and social aspects have long been acknowledged in archaeology and other social sciences and humanities such as social anthropology and history. This has led to the codification by UNESCO in the Convention for the Safeguarding of Intangible Cultural Heritage as a "mainspring of cultural diversity", which also highlights the "deep-seated interdependence between the intangible cultural heritage and the tangible cultural and natural heritage" (UNESCO 2003). Although it will not be refuted that the intangible relies on the tangible for consolidation, for the purposes of this thesis, "intangible" will here be defined **precursory** as follows:

*The intangible is that which presupposes, envelops and defines the tangible.*

The definition above includes a vast universe of aspects, only bound together by the technological process or product. However, a commonality in the extreme variability that intangible aspects present within this definition, is the person – the agent of society. If an intangible concept includes, presupposes, envelops, and defines something tangible, the connection between the two aspects is usually made by someone. That someone is a person: an agent that actively applies these connections constantly, either consciously, or un-/subconsciously. This connection can for instance be between a rule and its execution in time and space ("only use the vase for flowers"), a name and its relating physical item ("screwdriver") or skill level and final product (expertly crafted chalice). Boundless other alternative connections exist, and in each case, there will be many such links that bind the tangible with the intangible.

As such, the connections between intangible and tangible are mediated by human agents. Agents are themselves a mixture of both aspects – unified minds and bodies. However, although the human must make the connection between intangible and tangible concepts, this does not necessitate that intangible and tangible must always be related to humans. Sometimes physical phenomena are intangible, such as gravity, air currents and temperature. The connection between tangible and intangible can be when water (tangible) becomes choppy due to wind (intangible), or when hot temperatures (intangible) melt fat (tangible). These phenomena happen unrelated to humans. Yet, there is much debate if they are always irrelevant to humans (see Fotiadis 1994, Peirce

1878, Rorty 1999).

Even if some intangible phenomena are always irrelevant to human lives, as archaeology is a discipline that researches humans, this thesis will remain focussed on the intangible/tangible connections mediated by human agents. In this thesis, that therefore entails that the definition of intangible will tie back to human agency. In other words, tradition or belief systems are included in the definition, whereas wind speed is not. However, if wind speed directly influences human agency, such as was the case with wind powered iron smelting furnaces on Sri Lanka (Juleff 1996), wind becomes an intangible aspect that influences on human, intangible concepts, and so becomes included in the definition.

Not all intangible aspects of a society will be investigated by this thesis. A majority of experimental studies (79 % of investigated literature) are oriented towards a technological question. To investigate a relevant problem to the wider experimental debate, this thesis will stay within this realm, and the intangible that relates to technology <sup>2</sup> directly will be the intangible aspects researched in this thesis. Broadly speaking, most intangible elements of a society can be said to influence on technological ideas and concepts. For instance, the notion of propriety that follows a religious belief may determine who performs the technological process, how it is ritualised, where it takes place, and many other important aspects of that particular technology. This is the case when the Pangwa and the Fipa in Tanzania perform their iron smelting purity rituals. The rituals amongst other consist of dancing to symbolise the sexual act between the smelters and the furnace, which is conceived as a woman that gives birth to the iron. Other aspects that are seen as crucial to the process is the exclusion of women during the smelt, and use of magical substances such as ritual water (Barndon 2012: 40). Such rituals are part of a wider cosmology that influences not only technological processes but general worldviews. It is therefore difficult to draw a definite line between which (intangible) aspects of society are significant to a technology, and which are not, nor will this be attempted in this thesis. Yet, a clear line is drawn between what is tangibly and physically observable in the objects and structures that are left to us to study today. In this

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2. For a definition of "technology", see 1.2.

study, the focus is first and foremost on intangible aspects that can be directly related to the archaeological primary reference.

As will become clear throughout this work, one of the principal foci of a united experimental archaeological discourse is that archaeological research experiments should have an archaeological primary reference. This is found in much of archaeology, but in a sub-discipline that works predominantly with things, such as experimental archaeology, an object reference becomes important. To analyse the experimental debate, the intangible aspects under study will not only be linked to the technological process per se, but also to the archaeological object directly. As became clear through the study of the discourse, intangible aspects are often given cursory mention, or discussed in a generalised fashion also in experimental archaeology. However, primary references for intangible aspects are rarely brought forward in experimental studies, and this thesis sets out to explore this problem specifically. The intangible aspects that will be sought after in this process, are the aspects that have been given an object manifestation in an archaeological primary reference. As a result, the definition of the intangible aspects investigated in this thesis becomes the following:

*The intangible aspects that presuppose, envelop and define the technological process through human agency, and which has tangible or physical observable object or structure manifestation.*

As human agents, we often move in the intangible without giving it a second thought. Although archaeologists are generally very much aware of the concept of **intangibility**, in certain archaeological research debates it is frequently left out. This is the case with experimental archaeology. and it is the main research question of this thesis if methodological barriers complicate the research of the intangible through archaeological experiments.

Experimental archaeology, as a sub-discipline to archaeology, can be widely defined as a practical approach **involving experiments with** past structures and

artefacts, often involving replication<sup>3</sup> of things or processes, for the sake of getting to know the people behind the archaeology and what led their thoughts and actions towards that particular form of material expression.<sup>4</sup> Why the intangible seems to often be left out of the experimental archaeological discourse is only partly a question for this thesis. The consideration to actively choose to not include or address intangible structures or perspectives is ultimately a matter of preference. If the cause for the exclusion is found in an active rejection of the topic, it will not be further explored. Nevertheless, through a study of 100 select experiments from the international, academic discourse, read for the purpose of this thesis,<sup>5</sup> it has become obvious that an active choice to exclude the intangible is rarely put forward as an argument.<sup>6</sup> In fact, none of the sampled publications argue this way. Additionally, even though the study sample exhibits a significant diversity between experimental set-ups, modes, environments and topical focus, only 34 % of the publications do in fact go into intangible topics, and some of them just briefly.

In the archaeological fields of for instance landscape archaeology (Juleff and Bray 2007), archaeoacoustics (Díaz-Andreu and García 2012), archaeology of art (DeMarrais and Robb 2013), ethnoarchaeology (Smith 2001), public archaeology (Holtorf 2010b) and many more sub-areas of archaeology that sometimes employ a practical approach to material culture, intangible aspects of a society is a common research interest. Questions of why something occurs now or occurred in the past are often asked and answered through results of various researches, and is particularly dominant in archaeology. Explaining why usually entails interpretation, and such interpretive explanation is often a result in research concerning, amongst other topics, subsistence, technocomplexes, conflicts, settlement layouts, mobility, identity, cultural change, monuments, gender roles, rituals and professionalisation. On the opposite side of the scale are the archaeological sciences, which are very much either modelled on or wholly consisting of non-archaeological, natural scientific disciplines. They therefore rely on scientific ideals of unbiased empiricism, which necessitate

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3. In this thesis, "replica" and "replication" is used for attempts made at copying past objects, structures, processes or other elements.

4. **For a detailed definition of experimental archaeology, see Chapter 2.**

5. In the following, these sample publications will be referenced as the "study sample."

6. See Appendix A for full list and selection criteria.



unbiased experience by sensory, rather than intellectual, input for a phenomenon to be relevant (Barrow 2005; Popper 2002). It is therefore easier to comprehend why an intangible focus is less often applied to archaeological studies in scientific archaeologies.

While a focus on tangible in the archaeological sciences can be explained through empiricism, this is not necessarily the case for experimental archaeology. For instance, quite a few experimental archaeologists apply principles such as technological *choices* (e.g. Gheorghiu 2011; Foulds 2013b; Jeffra 2015), which are social, intangible constructs in a society. The same can be said for experiments concerning expertise or aesthetics (e.g. Apel 2008; Kreiter et al. 2014), or other normative behaviour. Additionally, the 33,3 % of surveyed experiments that do in fact address the intangible often do so convincingly. Even so, the majority – 66,6 % of the experiment study sample – stop short of putting their results into an intangible context. How the results feed into the actual lives of concrete past societies and how their peoples may have behaved, related to each other and why, is rarely addressed, bar in a generalised manner.

Certain questions arise from the study of the experiment sample. Is it custom behaviour for an experimental archaeologist to not provide such interpretations? Is the research method in the majority of cases simply unable to deliver such perspectives? Since experimental archaeologists address relations between people and things in a unique, practical manner, one could expect that answers to how people made, used and in which state they abandoned their things – typical experimental questions – could disclose at a minimum certain elements of human situations in the past, not limited to what their artefacts are empirically perceived as today. When only 1 of 3 experimental archaeological projects engage in this type of discussion, this seems at odds with the remainder of the archaeological discourse, which on the whole aims to study precisely the move from known fact to unknown situation.

One aspect of experimental archaeology that does become clear through the analysis of the sample experiments, is the dominant focus on technology – 79 % of experiments concern technological aspects, typically processing,

making or using. This is a general feature across speciality, mode and technological focus, and is reflected in the notion of experimental archaeology as a techno-focussed method (e.g. Malina 1983; Johansson 1983; Comis 2010; Kelterborn 2005; Heeb and Ottaway 2014). The remaining 21 % of the study sample successfully show that technology is not an obligatory topic. Why the significant focus is the case will therefore not be explored in this thesis. However, as only 24,1 % of technological experiments explore intangible elements of the technological, it seems pertinent to ask if a focus on technology complicates a study of the intangible in academic experimental archaeology. This question will form the overall focus for this thesis. The aim is therefore to provide a thorough exploration of the technological intangible through both theoretical concepts and practical experiments.

## **1.2. Technology and experimental archaeology**

Thus far, technology has proved popular in experimental archaeology, also outside of academia. As mentioned previously, a substantial majority of the sample publications concern technology. But technology can have many definitions, differing with the criteria the researcher chooses to include. Its definition may consequently span from the ideas and notions that the technology is a result of, to something entirely tangible, such as the things involved in its execution.

The term technology stems from Greek, and originated from τέχνη (tekhnē: 'art/' 'craft') and λόγος (logos: 'word'/'learning'/'knowledge')<sup>7</sup> (Dodson 2012), which combined literally translates to the knowledge of the craft. The etymological origin of the term indicates more than just physical implements. Today, technology is sometimes defined in a broad sense, such as "a manner of accomplishing a task especially using technical processes, methods, or knowledge."<sup>8</sup> However, it can equally well be defined narrowly, as "the application of scientific knowledge for practical purposes"<sup>9</sup> This latter definition

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7. Dodson Greek-English Lexicon May 2012: /τέχνη and /λόγος

8. Merriam-Webster online dictionary: <http://www.merriam-webster.com/dictionary/technology>

9. Oxford Dictionary of English/technology

opposes an archaeological use of the term, by indicating practices that stem from scientific knowledge, a recent feat for humankind. This definition of technology is apparently exclusive of practices related to crafts in general. But, by removing "scientific" from the definition above or replace it with relevant, similar factors, it can be re-coined "the application of (e.g generational/traditional/task-specific) knowledge for practical purposes" – more in line with archaeological research practice.

Although not a relevant criterion for this thesis, certain definitions seem closer to the original, Greek meaning, and more plausible when technology is viewed holistically. Amongst philosophers of technology, there is still consensus that technology is an interplay between technological artefacts or structures and their intangible intentions, functions and goals, which makes technology as a whole a value-laden part of any society (Fransen et al. 2013). Marcia-Anne Dobres (2010b: 158) defines technology as "dynamic acts of social and material transformation." Along these lines, and especially influential, a technological technique or craft was seen as a socially constituted phenomenon by anthropologist Marcel Mauss (1973: 78). Mauss' definition is often reiterated as a definition of technology as a 'total social fact', but the totality of it was not necessarily Mauss' intention (Gofman 1998; Mestrovic 1987). Nonetheless, the definitions above are perhaps wide enough to also incorporate other social constructions, nor do they limit technology to something functional with a cause and effect purpose. However, they do highlight that within the construction and execution of a technology, there is a whole other backdrop that facilitates and is necessitated by the existence of a technology. For the purpose of this thesis, the definition of technology, and the subsequent use of the term, will include this broader scope.

The backdrops of technologies are constituted by the respective social contexts, and the intentions that lie behind it (Dobres 2000: 21; articles in Haas-Lebegyev 2014), and include for instance the aforementioned traditions, gestures and fashions, but also personal knowledge and know-how. Most people have experienced how their gestures and personal knowledge inform on how to make things, such as a pot or drawing, or how to use technology, such as preparing an advanced meal. This is the foundation for a prominent interpretational theory

called *hermeneutics*. Hermeneutic theory proposes that every one of us understand and navigate the world on the sole background of our personal knowledge and know-how, called *horizon*. Our individual horizons are influenced by the concepts and structure of the society we live in, biological factors such as ADD or other illnesses, personal likings such as taste, what we have coincidentally experienced, and countless other factors that occur to and surround a person. Every time we *experience*, such as sense input or think about something, we use our horizons to interpret that which we are processing. The interpretations that result feed back into our horizon, which is then used to interpret subsequent experiences. This cyclical tacking between horizon and experience is individual, and has traditionally been called the hermeneutic circle (Jones 2002; Ramberg and Gjesdal 2013). Today, hermeneutics are often presented as a spiral, because our horizons continually expand with experience (McKemmish et al. 2012). As technology is operated and understood through experience, the hermeneutic spiral will be considered part of the concept of technology in this thesis.

There are other ways to define technology than as a plurality of individual ideas and actions. A singular technology can be considered an entity and function as a *field* as defined by Pierre Bourdieu (1977: 184); composed of "...economic relations, the constitution of which is inseparable of a body of specialized agents, with specific interests."<sup>10</sup> A field can be considered a microcosmos connected to and existing in a macrocosmos (Jakobsen 2002: xiii). One field can involve all aspects of a communal social category, such as for instance all the ideas, traditions, rituals, people, buildings and relics belonging to the church with the chalice, flower vase and screwdriver in the initial example of this chapter.<sup>11</sup> However, that does not mean that all the things that are physically in the church space always belongs to the church, or only belongs to the church field. Whereas the chalice and flower vase may belong to the field of the church and its purpose, the screwdriver belong may to the field of building management, which the church, as a building, is part of. As well as these, all three things belong to other fields too, such as the technological fields of goldsmithing, pottery making and furniture production. Within the field of pottery

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10. "Economy" as associated with amount of social resources (*Ibid.*).

11. On p. 13.

making, the pots, ideas, knowledge, skill and traditions about making pottery such as vases are aggregated in one arena. This includes the pre- and post-production processes, for instance knowledge about raw material, where to get it and how to mix it with other substances, and how durable the pot will be after it is made. In the midst of the field are the potters, and all the other people connected to pottery production, and the interactions of these people. When a potter interacts with non-potters, they often leave the field entirely. They return the next time pottery is on their minds. Although the field has no physical expansion, it may be socially localised and connected to for instance a local community. If a potter engages with other potters or field participants from elsewhere, a field may expand to include that social interaction, as well as parallel localised fields within, e.g. a meeting of potters in a regional tradition where participants discuss local trends.

While the idea of a field can have transparent and flexible borders, fields can help define a technology into one entity that includes the things that result, and everything connected to the things as well. The parts of a field that are not participants or objects are often intangible. However, these parts remain decisive for the objects' final form and use. To understand the end results, one must often have knowledge about some of the influential, intangible aspects that surround them – for instance use. The fact that archaeologists strive to do this is illustrated by the substantial attempts at telling stories about the pots, such as provenance studies (e.g. Pollard et al. 2014), studies of social grouping through stylistic or technological features (Engevik 2008; Jones 2002), food and drink practices (e.g. Rødsrud 2012; Skibo 1992), and pots as grave goods (Østmo 2007). It therefore seems surprising that this widened, socialised view of technology is so rarely studied through archaeological experiments.

In order to investigate the broader connotations of a technology, it is clear that the problem of invisibility has to be overcome. Archaeology is built on the foundations of the tangible remains of people past and rests on the premise that the things can tell us something about the people and their community. **In one way, this can be considered** a dogmatic notion that the tangible opens the door to the intangible surrounds it was part of. In experimental archaeology, this has led many authors to highlight that the primary reference for an experiment

should be archaeological (Adams 2010; Coles 1967; Crumlin-Pedersen 1995; Rasmussen 2007a; Reynolds 1999; Whittaker 2010 and many more).<sup>12</sup>

As indicated above, to approach tangible manifestations of intangible concepts is already carried out in the archaeological discourse with regards to for instance religion, hierarchies, societal structures and politics. However, with regards to the intangible aspects of technology much less has been done that links the archaeological record directly to the field of the technology in question. Nevertheless, there are examples, reflected in the 18 experiments in the study sample that have achieved such results. An excellent example is that of Jan Apel (2008: 102) in which he acts as an apprentice to the expert flint knapper Errett Callahan in a series of production experiments, in order to estimate degrees of practical know-how in different stages of the production of Danish Late Neolithic flint daggers. The estimation is then compared with several sites with this kind of production and helps provide a solid interpretation of the skill level and the spatial distribution thereof. One of his finds is that a high number of knapping errors is found in secluded zones close to the flint source. Apel sees this as a secret transfer of a pool of knowledge and know-how concerning the flint dagger craft, which was not to be shared with the larger group because the specialisation process was considered a significant asset (*Ibid.*: 109).

In the above example, the daggers and their debitage function as the material representations of concepts, skill and learning. Such case studies display how a material, archaeological approach that refers to the archaeological record as primary reference can produce results that have meaning to the investigation of intangible parts of given technologies. Particularly when skill is concerned, as demonstrated with Apel's study, experiments have come far in producing valuable insights that surpasses the technicality of the end product, often in combination with the sequential *chaîne opératoire* concept (Apel and Knutsson 2006a; Khreisheh et al. 2013). However, this sequential concept of a chain of operations,<sup>13</sup> or gestures and actions, should not only be limited to studies of manufacture, cognition and skill generation. It can rather be said to include "all cultural transformations that a specific raw material had to go through" (Sellet

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12. For more authors, and a further discussion of this criterion, see Table 2 and section 2.3.

13. Also called 'reduction sequence' in lithic circles, for instance Eren et al. 2011.

1993: 106), which in this thesis will also incorporate use and discard operations. Within its core lies a concept of the situated embeddedness of technological fields, reliant on personal craft production and transfer, but also on the demands of the society that need or want the technology in place (Jeffra 2015: 142). When a stage, typically in, but not limited to, a manufacture sequence, is analysed and the actions of the involved individual(s) become clear, it is both possible to obtain a view of parts of their life, but also to see communal features between similar things which can provide grounds for an interpretation about their larger unit – the society. For instance: an individual potter can be glimpsed through personal, aesthetic expression, such as in the exceptional variation in decoration patterns on Bucket-shaped pots from the Norwegian Iron Age. However, these particular pots have a limited number of strict shape schemes that can be used to define for example regionality (Kristoffersen and Magnus 2010), which paints a picture of social cohesion in a society where individuality was valued. Both these elements belong to the phase concerning shaping of the pot, and provide paradoxical social aspects within the same process. Although operational steps in a sequence can sometimes be (and is traditionally) clarified through stylistic or other visual analysis, experimental archaeology is particularly suitable for identifying the practical procedures in a sequence. This is indicated by the wide application of the sequential approach, most often in, but not limited to, experiments with lithics and ceramics (e.g. Driscoll 2011; Gheorghiu 2011; Melis et al. 2011; Purri and Scarcella 2011; Santos da Rosa et al. 2014).

Given the discussion above, it seems almost paradoxical that the method that so frequently engages with the *chaîne opératoire* approach is so rarely concerned with intangible aspects; especially when considering that it is both a method and an approach especially suitable to provide insights into the technological realm. Additionally, experimental archaeology contains a large amount of people who have intimate, personal knowledge of and experience in practical production and use of specific technologies. In theory, this could lead to a perfect marriage of concepts and practice for providing both an intangible understanding of practical issues and a practical understanding of intangible issues.

Of course, if the preference for tangibility is in fact a matter of personal choice, this should not be morally judged, and it will not be in this regard. Whether or not it is a personal, customary or idealistic decision will, therefore not be the topic for this thesis. The research questions based on the already described aim for this thesis will follow two levels; based upon the practical-intangible doubleness of experimental archaeology just described:

- Are there conceptual barriers to understanding intangible aspects of technology through the experimental research methodology?
- Are there practical barriers to understanding intangible aspects of technology through the experimental research methodology?

Through asking these questions, the aspiration is to explore the academic methodology of experimental archaeology thoroughly, in order to discuss and facilitate experimental investigations into intangible aspects of technology. The project aims to do so through a combination of both theoretical and practical *modus operandi*. If this can be obtained, archaeological questions about technological issues will have the potential to vastly expand **beyond the tangible**.

### 1.3. Thesis trajectory

As may have become clear from the argumentation above, the focus terrain for this project is academic experimental archaeology.<sup>14</sup> Nevertheless, just as any technological practice, the mode of experimental archaeology is diverse, also within academia. Some archaeologists exclusively perform strictly controlled laboratory based experiments, others, and the large majority, work closely with what are called *actualistic* experiments (Harry 2010; Jeske et al. 2010; Outram 2008). While the former methodology has its ideals in the natural sciences and is usually set in a university atmosphere, the latter is in one sense closer to a re-enactment of the technological operations that are hypothesised, and is often executed outside of universities. However, the actual 'reenactment' movement is by many considered a deviation from structured experiments. Nevertheless, if viewed linearly, the field of experimental archaeology contains participants from

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14. Hereafter: experimental archaeology [without specification].



one end of the scale to the other. As this thesis is aiming to address the entire academic methodology, encompassing several or all of these ways of experimenting in a non-evolutionary and non-judgemental fashion, it is perhaps more useful to see experimental archaeology as a meta-field that contains smaller fields that share a substantial interaction through their participation in the larger unit. Each unit has blurry limits and may diffuse into the next. Encircling them lies that which determines that they belong to the meta-field, and it is these shared qualities this thesis sets out to explore.

The different experiment types, modes or foci may sometimes also share other qualities with certain other sub-fields, for instance raw material focus. Flint-knapping experiments and experiments regarding the manufacture of bone tools, can sometimes share for instance parts of a toolkit, or they may result in similar objects (e.g. Buc 2011; Pétilion et al. 2011). Other experiments share raw material types ((Lobisser 2004; Nielsen 2006); other again share their public component (Bakas 2012; Crothers 2008); some the focus on reconstruction,<sup>15</sup> sometimes even to the reenacting (e.g. Edberg 2009; Magnus 2006; Narmo 2011; Liedgren and Östlund 2011). and so on.

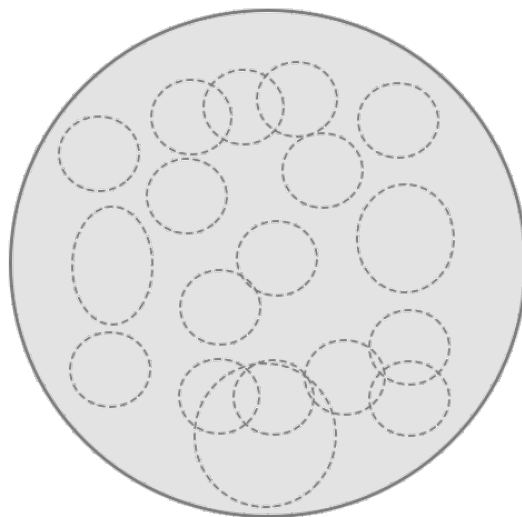


Figure 1: A metafield with subfields that share an all-encompassing practice.

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15. "Reconstruction" in this thesis will bear no specific categorical meaning. Rather it will be used for any activity where the aim is to reconstruct an action, an object, a process or other past elements.

At base, there is still something, currently somewhat disputed (Petersson and Narmo 2011a: 29f), that connects them all (Figure 1). The multiplicity is in line with certain authors' proposition of a more inclusive, humanistic and interpretive approach to experiments (*Ibid.*; Rasmussen 2001; Bánffy 2012).

In order to analyse if there are conceptual or practical obstacles that limits the use of experimental archaeology in technological, intangible topics, it is necessary to seek that which constitutes common ground. It is therefore necessary to generalise the diverse field of experiments for the theoretical evaluation and practical case experiments to be representative of the entire discourse of experimental archaeology. Therefore, some degree of typification must be sought. Nevertheless, it is important to note that although generalisation will be part of this process, in no way is this intended as a universalisation of principles for the method. The typification will pertain to the analysis of the research questions outlined above, and should not be considered a model for an idealised experiment. Rather, what is sought is the idea of a "normal" experiment, which is assumed to function as a good proxy for the method on the whole. A third research question, a vehicle for the other two main questions outlined above, therefore becomes:

- Can a *typical* experimental method be identified, and can this method function as a proxy for the method **in its entirety**?

In order to identify such typical features, a thorough examination of the methods of the experimental discourse is necessary and will be the outset of the analysis. In actuality, this is the first part of a substantial deconstruction of experimental archaeology (Part I of this thesis). On the basis of this discussion, an intimate knowledge with the reference world, notions and norms of experimental archaeology will be obtained. These will form the foundation of a debate of potential conceptual barriers to an understanding of intangible, technological aspects.

The deconstruction will be disassembled in segments that operate on different levels. The first deconstructive chapter (Chapter 2) will tackle an analysis of theoretical literature on experimental methodology, in order to approach discourse-specific, shared criteria for what constitutes the common

methodological ground. However, not only literature, but also hands-on experimental practice was analysed for this investigation, and perspectives on experimental archaeology obtained, through results from anthropological fieldwork at the Viking Ship Museum in Roskilde, Denmark, will be presented to form part of the discussion.

In Chapter 3, an exploration of the ultimate level of paradigms is presented to complement the methodological discussion in Chapter 2, and to expand on the dissimilarities that exist between certain methodological ideals. Through the existence of such fundamental differences in the experimental field, paradigms that cannot meet still clash. Yet, in experimental archaeology, the widespread allowance of separate paradigms rarely causes epistemological debate. Therefore, the deconstruction goes beyond the clash and investigates a possible paradigmatic justification for the seemingly pragmatic lack of paradigm annihilation.

Following the paradigmatic analysis, Chapter 4 brings a view to theories of technology. This chapter will carefully examine and fragment the relationship between people and their technologies, and how archaeologists may apply theories to aid in their study of these relationships. With the conclusion of this chapter, the analysis will have brought a view of a coherent experimental methodology to the detail that it can be reconstructed as an actual and practical research methodology for the purpose of this thesis. This piecemeal reconstruction will result in an experiment framework that collates elements from all discussions to provide both an experimental protocol ready for use, and an evaluative framework to aid in the assessment of the method (Chapter 5).

The methodology will be assembled into case experiments in Part II. Three experiments will be presented, each pertaining to different periodical, technological and evidential categorisation. The first case experiment (Chapter 6) starts with a well-defined archaeological situation, already rigorously researched for nearly a century: aspects of production of Bucket-shaped pots from the Norwegian Migration Period. In this experiment, the methodology is put to the test through firing experiments exploring the reasons for exceptional temper proportions in a certain type of pottery, often recovered, and interpreted

as a highly symbolic and valuable artefact type. A completely different technological situation, not researched before now is introduced in Chapter 7. This chapter describes experiments with the examination of intention behind faceted, pecked stones of unknown functionality from the Norwegian Late Mesolithic. Set in a situation without backing of other research, the experiment provides the initial entry point into the entire artefact type, providing a completely different role for the experiment to fill.

The first two experiment chapters will **explore** two very different ways to research through experiments. A third, and equally different experiment is the examination of Middle Palaeolithic birch bark tar production procedures (Chapter 8). In this case, the vast gap in time has obliterated all evidence of how birch bark tar was produced, however, several Neanderthal flint artefacts have been found in Germany and Italy with remains of this substance, which must be produced in pyrolysis under controlled circumstances. The finds have sparked a lively discourse, involving both elements of the evolution of cognition, of Neanderthal lifestyle and several previous experimental attempts at producing the substance in various structure types. The present experiment sets out to test whether pyrolysis can be achieved in raised sand structures, yet the lack of evidence that even indicate such structures is almost total. The experiment does not aim to provide evidence. This is the ultimate test for experimental archaeology as a research method. Can the experiment contribute any understanding of the intangible, based on something completely non-existent? The chapter will end with a detailed discussion of the potential contribution made to the archaeological discourse.

Chapter 9 will sum up and discuss the various elements of the project: was the collated methodology successful as a representation of experimental archaeology? Can we standardise something so inherently diverse that it positions itself in several paradigms? And lastly: are there conceptual, practical or other unexpected barriers to understanding intangible aspects of archaeological fields of technology? On the way to the conclusion in Chapter 10, and irrespective of the final result to the analysis, the author will hopefully have gained enough insight into the intangible field of experimental archaeology to be able to answer all these questions.





# Part I – Deconstruction





## 2. Experimental concepts of method

Academic experimental archaeology is a small and varied discourse, consisting of different preferences for different modes, different crafts, different periods, different raw materials and different traditions (articles in Flores and Paardekooper 2014; and in Petersson and Narmo 2011b). The centre of gravity of the academic discourse is currently in Europe, with main hubs in the United Kingdom, Germany, France, and Spain, and with increasing activity in the Czech Republic, Poland, the Netherlands, Scandinavia and Italy. The rest of Europe has a more fragmented degree of participation, which varies in line with who is where. Outside of Europe, there is a steady contribution of experiments from the United States, with additional initiatives from Latin-America. Every now and again, we also see academic contributions from the rest of the world, particularly ethnographic experiments from Africa, **but it may seem that experimental archaeology as a research discipline is still in the starting pit outside of the core areas mentioned.**

As many factors of archaeology, experiments exhibit regional trends: while in Scandinavia it is generally a museum-driven method with an extensive, public component and solid backing of postmodern<sup>16</sup> thought (Beck 2011; Bánffy 2012; Gansum 2004; Holtorf 2010b; Petersson 2011), North American experimental archaeology largely rests on ideals that promote a higher degree of control (articles in Ferguson 2010). Most other regions shift somewhere in between, with substantial parts of the discourse dependent on the presence of museum venues (Flores and Paardekooper 2014; Forrest 2008; Vorlauf 2011). Some countries have a large, national discourse but barely participate in an international exchange. As there a limited amount of languages available to the author,<sup>17</sup> the majority of these internal environments do not form part of the assessment. However, for the purpose of this thesis, the international discourse does provide a sufficiently wide view of academic experimental archaeology.<sup>18</sup>

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16. Postmodernism can be considered the reaction to the modernistic; in this sense sciences based on positivism.

17. Scandinavian, English, Dutch, French and German.

18. See Appendix A.

The diversity of experiments notwithstanding, all archaeological experiments share qualities. As opposed to the archaeological sciences which intentionally perform experiments that rest on the premise of modern comprehension (Jones 2002), the common ground of archaeological experiments is usually seen in a practical approach to make and use authentic technologies to achieve an idea of the *original* comprehension. At the most fundamental level, most people who ever aimed to understand and develop traditional crafts worked in the same manner: by making, trying, testing and exploring old ways of solving a technological problem. People have wanted to understand such technologies for the sake of it for a very long time. However, experimental archaeology usually aims to understand 'dead' technologies for the purpose of learning something about the people behind the technologies **and their practical operations**. This may have only started in the time of the Grand Tour in the 1800's, when the collection of *curiosa* became a popular hobby among higher classes (Forrest 2008: 65).

Lithic artefacts have been the subject of replication since the end of the 19<sup>th</sup> century (e.g Evans 1872), mostly concerned with manufacture techniques that slowly lead into an era of archaeological experimentation dominated by archaeologists-cum-flintknappers in the 1960s (Eigeland 2011a: 101). At the same time, the positivist<sup>19</sup> approach to archaeology was initiated and applied to archaeological theory as well as practice. The thought that an experimental methodology in archaeology should coincide with the experimental methodology of the natural sciences was affirmed, and ideas of uniform research ideals and conduct were brought to the forefront in the experimental discourse, represented by publications such as Sergei Semenov's *Prehistoric Technology* (1964)<sup>20</sup> and Coles' *Archaeology by Experiment* (1973).<sup>21</sup> Other uses of the denomination 'Experimental Archaeology' are also found in this period (for example Dethlefsen and Deetz 1966), more symptomatic of the age of

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19. Positivism is the belief in universal truths/absolutes (Knowles 2000: 106).

20. Although Semenov's work, originally published in Russian 1957, was researched and authored within the typological discourse of the cultural-historical period, it still stands as a pioneering work within use-wear studies of today, which subsequently developed on experimental and scientific foundations (Kimball et al. 1995: 6; Lawn and Marshall 1979: 63).

21. Both publications still gets a high degree of citation: On Google Scholar, Semenov's book is listed with 969 citations, whereas Coles' 2nd edition from 2014 is registered with 239 citations. (As a comparison, Lewis Binford's foundational article "Archaeology as Anthropology" (1962) is listed with 1415 citations.) [Accessed 7/8/15].

positivism and the belief that 'hard science' can answer social questions. However, over time it became increasingly clear that 'experiment' referred to practical experiments with material remains, and it is as such we find the experimental method in use today.

The experimental approach to archaeological research has traditionally been used to answer questions directly linked to the technology at hand. Under the heyday of the scientifically inclined period in archaeology – the 1960's and 70's, problems related to micro-scale processes such as specific manufacture techniques (Steinbring 1966), pinpointing use tasks (Unger-Hamilton 1988: 195f), exploring implement function (Stelcl and Malina 1970) or searching for the reason behind a certain choice of raw material or form (Puleston 1971) were often isolated from general research on the society in question, and the acquired data seemingly left for other people to make use of and interpret (Rasmussen 2007b: 14). Research questions such as “How was the needle made” and “Was this flint blade hafted transversely” are still widely pursued today, but increasingly connected to macro-scale social questions such as the differences in technological strategy between Magdalenian groups (Dobres 2000: 200f) and the onset of warfare in European prehistory (**Horn and Schenck, in press**), more or less in tune with postmodern tendencies in archaeological theory. Experimental archaeology is now used as an argument in discussing movement of people (Banks 2009: 51-53; Eigeland 2011a: 115); cognitive processes of learning (Eigeland and Sternke 2011; Khreisheh 2013); rituals (Gheorghiu 2011); social backdrop to evolving processes of technology (Lage 2012), and for assessing archaeologists themselves (Driscoll 2011). The field of experimental archaeology is ever increasing, and the method is no longer a narrow discipline with its own subject and isolated argumentation, but one of a range of methods in the common discourse of archaeology. Even so, there seems to be a considerable lag in addressing the social in the experimental discourse, when compared to other parts of the archaeological discourse. This was reflected in the analysis of 100 experimental publications that an especially low proportion of intangible, social aspects of technological pasts.<sup>22</sup> Only 18 of 79 publications concerned with technological practice

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22. For a list of publications analysed, see Appendix A.

touched upon such topics, and only 10 of those went into an in-depth analysis; 12,7 %.

Some might say that there are as many approaches as there are experimenters, others may rely on one, more specific and defined approach and others again maintain that proper experimental archaeology takes place in (near proximity to) the academic sphere. At its loosest definition, it is practiced everywhere, from universities to museums to the homes of amateur experimenters who practice an ancient technology out of interest. This means that approaches to method can be anywhere on the scale from conscious to subconscious to perhaps even unconscious, and this may also be the case in experimental archaeology geared towards research, the topic of this thesis. This chapter aims to review the academic practice of experimental archaeology and its many methodologies, and the multitude of approaches will be discussed below.

Although the experimental approach to archaeology shows the high level of diversity summarised above, all archaeological experiments have foundational features in common, specifically related to the practical, physical aspect of theory testing and exploration. They start with the material record and most attempt reconstruction of artefacts or structures, or aspects of such. The reconstructed material is then habitually tested (Kreiter et al. 2014; L'Héritier et al. 2015; Willis and Boehm 2014) or explored (Driscoll and Menuge 2011; Liedgren and Östlund 2011; Pétilion et al. 2011) and results evaluated. Coles (1973: 14) suggests that all experiments proceed through the trajectory of problem ► idea ► procedure ► result ► assessment; in other words, he proposes a fixed *chaîne opératoire* of an experiment. Coles' chain of experimental operations closely resembles the hypothetico-deductive method which is the ideal of scientific experimentation. **The suitability of this trajectory** as a characterisation of archaeological experiments will be further discussed in section 3.2.1. However, Coles does not include a *test* in his trajectory, which is fitting for archaeological experiments that often go through exploratory evaluations rather than structured tests, and that includes different modes, as described in section 1.3. That not all experiments are tests in the strict sense is also reflected by Peter Kelterborn through his description of the experimental

strategy "Try now, think later" (Kelterborn 2005).

As presented in the introduction (Figure 1), experiments often share qualities, but they also share a propensity for diversification. This is further reflected in the study sample of 100 publications, where 33 % of experiments present new methodological procedures or suggestions. In the following, a variety of academic approaches to the execution of experiments will be presented and discussed on a detailed level, to reflect on the common ground of diverse experimental interpretations, and what constitutes a generally viable archaeological experiment. The notion of 'viable' is relative to the method and aim of experiment execution, and there are diverging perspectives for what must be included in an experimental procedure. Yet, at the centre of the field must be certain qualities that qualify experiments to contribute to the same discourse under a common flag. The search for the standardised concepts will be collated in a summary of what those qualities are, but only after an assessment of the written discourse. In addition, a particular case of multidisciplinary experimental archaeology will be presented and assessed. This discourse was experienced more than read, by fieldwork at the Viking Ship Museum (VSM) of Roskilde in Denmark, which included participant observation, singular and group interviews with staff and trial participants, and exhibition analysis. This museum is first and foremost a research institution devoted to experimental maritime archaeology, and a leading actor on the scene of ship reconstruction (McGrail 2006). It therefore provided a valuable addition to the evaluation of what experimental archaeology is and can be. In conjunction, the below evaluation of written and practical viewpoints should provide an in-depth description of what it means to be an experimental archaeologist in the academic discourse.

### **2.1. Academic approaches to the experimental method**

Academia is typically guided by principles and methods to carry out different procedures. Whereas these differ considerably from subject field to subject field and even from university to university, they often have in common that most people who work in that particular environment know and follow the same

guiding principles for amongst others documentation, publication and accepted jargon. However, for experimental archaeologists, an academic environment consisting of colleagues with similar experience is less often at hand. In certain environments, such as in the Department of Archaeology of the University of Exeter,<sup>23</sup> there is a high, communal awareness of experimental methodology amongst a sizeable collegiate, whereas in other universities an experimental archaeologist may work alone and follow an individual trajectory of method. However, as shown in **Table 1** below, there are a number of guidelines and defining publications at hand.

In order to investigate whether experimental archaeology is a viable method for any problem, a definition of what constitutes the experimental approach to archaeology should be sought. In recent years, some authors have voiced a claim that there is a misapprehension of the concept (Cunningham et al. 2008; Outram 2008: 3-4; Schmidt 2005a; Tichý 2005: 114), and a definition based on the multitude of experiments around the world is indeed difficult (e.g. Leineweber 2001: 16). To Coles (1983), this versatility is part of its great strength as a method, in the fact that it attracts all types of researchers and therefore results in a limitless amount of fresh ideas. However, since unified methodologies in general seems to be preferred, a number of authors do make an effort to define what they consider experimental in archaeology. An overview of 27 select explicit definitions from different discourses and regions can be seen in Table 1.

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23. The first and presently one of two universities in the world to teach a master programme in Experimental Archaeology.

Table 1: Factors in the definition of experimental archaeology

Date	Author	Science <sup>24</sup>	interpretive <sup>25</sup>	Replication/ reconstruction	Experience
1961	Ascher	1		1	
1967	Coles		1	1	
1973	Saraydar and Shimada	1		1	
1973	Woolsey			1	
1983	Malina	1			
1987	Olausson	1			
1988	Herschend		1		
1988	Beyries and Helmer	1			
1988	Villa	1			
1988	Frère-Sautot	1			
1994	Schiffer et al	1	1		
2002	LeMoine	1			
2002	Mathieu	1	1	1	
2002	Cross, Zubrow and Cowan	1	1		1
2002	Mathieu and Meyer	1			
2002	Zubrow		1		
2005	Meurers-Balke and Lüning	1			
2005	Tichý	1	1	1	1
2006	Crumlin-Petersen	1			
2006	Finderup		1		
2007a	Rasmussen	1	1		
2010	Jolie and McBrinn	1	1		
2010	Comis	1			
2011	Petersson and Narmo		1		1
2012	Bakas	1	1	1	
2014	Foulds	1	1		
2015	Grimaldi	1	1		1
Sum	27	21	14	7	3

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24. Determined by the use of the words *science/scientific, control* or *replicability/repeatability*

25. Determined by the description of a non-controllable process of interpretation

21 of the 27 publications in the table include some form of scientific aspect in their definition of what experimental archaeology includes, whereas 14 works incorporate interpretational aspects in their definition. Seven definitions bring focus to replication or reconstruction (of the past), and four articles bring up the element of experience that an experiment contributes to the interpretation of the results. The definitions that include scientific elements are well dispersed over time. The recent application of scientific ideals in settings that do not aim for exclusively scientific audiences or results (Cunningham et al. 2008), has been dubbed "the modified control theory in experimental archaeology" by Bodil Petersson and Lars Erik Narmo (2011a: 31). They highlight its rather paradoxical application in times where the postmodern paradigm is prominent, and it indeed appears from Table 1 that interpretive aspects are a more recent feature to the definitions of experimental archaeology, that can be seen in relation to the post-processual or postmodern paradigm in archaeological theory, which gained a foothold in the 80s. However, the scientific ideals are still dominant in the discourse (Bakas 2012; Callahan 1999; Cunningham et al. 2008; Richter 2005), amongst others through a frequent reference of Peter Reynolds (1999), who has figured prominently in the bibliographies of experimental archaeological research publications across Europe (e.g. Comis 2010; Cunningham et al. 2008; Paardekooper 2011; Sevilla and Taviro 2011; Petersson and Narmo 2011a). These ideals are also sometimes expressed through a lexical and scientific definition of the term "experiment" where a definition of the experimental methodology is needed (Comis 2010; Richter 2005). Nevertheless, as is also evident in Table 1, experimenters are not rejecting postmodernism entirely, most often in Europe and concerning situations when human bias is prominent (Comis 2010; Kaltsogianni 2011; Liardet 2013; Rasmussen 2007a).

In addition to its small size, the sample above is biased in favour of this theoretical paradigm because this is when theory in experimental archaeology was brought to the forefront. The bias towards more recent literature is also intended, as the current analysis tackles what experiments are, not what they were. Even so, it does provide a sense of what is generally included in the basic concept of experimental archaeology. For now, this thesis will follow the definition put forward by Marianne Rasmussen (2007a: 10):



*An archaeological experiment belongs in that part of the scientific research process where a platform is established for interpretation and hypotheses are proposed concerning the observed phenomena revealed through data collection, in order to put these data into further perspective. The archaeological experiment is, as a method, to be seen as an analogy, with the consequences this has for an evaluation of the relationship between reality and the analogy as well as for the results.*

Nevertheless, a short definition is not sufficient to describe an entire method, and numerous works have proposed theories and methodologies for the experimental approach. Over the next pages, the experimental research procedure will be investigated from several different angles to highlight what can be considered a viable standardised course of action.

### **2.1.1. Codes for conduct in academic experiments**

It follows from the introduction to this chapter that a bronze smelting experiment differs from an experiment with procedures in flintknapping, an impact strength test of tempered pottery, a roof construction at 1:1 scale or a trial voyage with a Viking ship. It is also clear that modes, venues and theoretical ideals often differ. Even so, quite a few authors have published guidelines that are common to many, if not all, experiments, and these principles can inform on what good conduct or right form is considered to be in experimental archaeology. A number of archaeologists have come forward in attempts to address this issue directly with propositions of codes for conduct when executing an experiment (Coates et al. 1995; Coles 1973; Crumlin-Pedersen 1995; 2006; Kelterborn 2005; Mathieu and Meyer 2002; Reynolds 1999; and articles in Ferguson 2010). Although they do not necessarily agree, a synthesis of suggestions will be presented below.

The guidelines for the experimental process can be broadly grouped into two categories; design-related and circumstantial. The design-related guidelines can be seen as 'recipes' for conduct, whereas the circumstantial suggestions point out elements and problems to consider in the experimental design and subsequent interpretational process.

Since a majority of the select definitions in Table 1 include some form of scientific format such as in the natural sciences, working towards the *hypothetico-deductive* ideal<sup>26</sup> of unbiased hypothesis testing can in many cases be considered a code for conduct; this is also often cited as the baseline procedure (Grimaldi 2014; Outram 2008; Reynolds 1999: 157). However, such ideals are not always applicable, especially where less control is achievable. Instead of the rigid demands that the hypothetico-deductive method requires, the codes for conduct for archaeological experiments deal with the practical and conceptual approach to the experimental execution. A range of suggestions have been assembled in Table 2. There are many more, but the main criterion for the selection below has been an independent proposal of the principle in more than one publication. The general literature studied for this purpose consists of newer contributions to the theoretical, experimental discourse, but has also included the most iconic, highly cited sources that stem from earlier trends, as these are currently still the leading references for the experimental procedure and therefore represents viewpoints of the current discourse.

Table 2: Suggested guidelines for an archaeological experiment by more than one author.

Phase	Design-related	Sources	Circumstantial	Sources
<b>1. Planning</b>	Conceive experimental plan with explicit hypothesis or research problem.	<i>(Adams 2010; Coates et al. 1995; Crumlin-Pedersen 1995; Cunningham et al. 2008; Kelterborn 1987; Lubinski and Shaffer 2010; Mathieu and Meyer 2002) (McGrail 2006; Reynolds 1998b)</i>	Careful assessment of experimental aim and purpose.	<i>(Coates et al. 1995; Coles 1967; Crumlin-Pedersen 2006; Jeske et al. 2010)</i>

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26. The ideal of natural sciences which relies on the steps hypothesis – test – deduction of results – new hypothesis – corroboration.

This principle will be further discussed section 3.2.1.

Phase	Design-related	Sources	Circumstantial	Sources
			Reference to archaeological material must form the basis of the experiment. Ethnographic indications must be treated with caution.	(Adams 2010; Adams et al. 2009; Beck 2010; Coates et al. 1995; Coles 1967; Crumlin-Pedersen 1995; Cunningham et al. 2008; Groom 2009; Jolie and McBrinn 2010; Lubinski and Shaffer 2010; Rasmussen 2007a; Reynolds 1999; Whittaker 2010)
	It should be considered whether model simulation can fulfil the research aims.	(Mathieu 2002b; McGrail 2006; Rasmussen 2011; Reynolds 1998b)		
	The level of control necessary for the purpose of the experiment must be decided upon.	(Marsh and Ferguson 2010; Mathieu 2002b; Rasmussen 2007b)		
	The experiment must be repeatable.	(Coles 1973; Cunningham et al. 2008; Kelterborn 1987; Lubinski and Shaffer 2010)(Reynolds 1999; Richter 2005; Tichý 2005; Whittaker 2010)		
	The experiment must be measurable.	(Kelterborn 2005; Reynolds 1999)		
	The experiment must be documented throughout.	(Crumlin-Pedersen 1995; Kelterborn 2005)		

Phase	Design-related	Sources	Circumstantial	Sources
	The experiment should be in collaboration with other expertise.	(Nielsen 2006; Schiffer et al. 1994)		
<b>2. Preparation and execution</b>	Authenticity of tools, materials and techniques for manufacture must be sought.	(Coates et al. 1995; Coles 1967; Crumlin-Pedersen 1995; Mathieu 2002b; Schmidt 2005a)	The experiment should be conducted with the aid of qualified people (manufacture and/ or execution).	(Crumlin-Pedersen 1995; Jolie and McBrinn 2010; Kelterborn 1987; Nielsen 2006; Outram 2008)
	Familiarity with the experimental process is necessary.	(Kelterborn 2005; Nielsen 2006)		
	(Effects of) variables should be isolated.	(Lubinski and Shaffer 2010; Mathieu and Meyer 2002; Rasmussen 2007a; Reynolds 1999)	Variables should be conceptualised.	(Cunningham et al. 2008; Englert 2006; Marsh and Ferguson 2010; Mathieu and Meyer 2002; Rasmussen 2007a; Schiffer and Skibo 1987)
	Variables should be measured.	(Kelterborn 1987; Mathieu and Meyer 2002; Reynolds 1999)		
	The experimenters should be aware of biases to the process.	(Callahan 1999; Coates et al. 1995; Coles 1967; Jolie and McBrinn 2010; Marsh and Ferguson 2010; Rasmussen 2007a; Rasmussen 2007b; Reynolds 1998b; Reynolds 1999; Tichý 2005; Whittaker 2010)	Familiarity with the experimental process is needed.	(Kelterborn 1987; Nielsen 2006)

Phase	Design-related	Sources	Circumstantial	Sources
<b>3. Post-experimental analysis</b>	The results must be directly compared with the prediction of the hypothesis.	<i>(Coates et al. 1995; Outram 2005; Rasmussen 2007b; Reynolds 1998a; Richter 2005)</i>	The experiment produces primary levels of data.	<i>(Rasmussen 2007b; Reynolds 1998a)</i>
			The experiment is an analogy to the archaeological situation and should be considered as such.	<i>(Amick et al. 1989; Lubinski and Shaffer 2010; Marsh and Ferguson 2010; Mathieu 2002b; Rasmussen 2007b; Richter 2005)</i>
	Experiments should be analysed statistically.	<i>(Lubinski and Shaffer 2010; Reynolds 1999)</i>		
	Absolute proof should not be claimed.	<i>(Coles 1973; Rasmussen 2007a; Reynolds 1998a)</i>	Results are relevant beyond issues of technology.	<i>(Crumlin-Pedersen 2006; Lubinski and Shaffer 2010; Mathieu and Meyer 2002; Nielsen 2006; Petersson and Narmo 2011a; Schiffer et al. 1994; Tichý 2005)</i>
	Future corroboration of results should be sought.	<i>(Bement 2010; Carr and Bradbury 2010; Jeske et al. 2010; Jolie and McBrinn 2010; Reynolds 1999)</i>		
	The experiment and results should be evaluated.	<i>(Adams 2010; Jolie and McBrinn 2010; Kelterborn 2005; Mathieu and Meyer 2002)</i>		

Phase	Design-related	Sources	Circumstantial	Sources
4. Dissemination	The experiment and results must be communicated and published.	(1996; Adams 2010; Callahan 1999; Coates et al. 1995; Crumlin-Pedersen 1995; Harry 2010; McGrail 2006; Rasmussen 2001; Richter 2005)	The dissemination must be directed towards a wider audience	(Coates et al. 1995; Crumlin-Pedersen 1995; Outram 2005; Schmidt 2005b)
			Publication should be in academic journal	(1996; Coates et al. 1995; Crumlin-Pedersen 1995; Mathieu and Meyer 2002; Outram 2005; Rasmussen 2001)

In Table 2, the guidelines are grouped into chronological phases of the experimental process. However, this chronology is not absolute (Kelterborn 2005), and not always explicitly stated, and it is therefore suggested that such guidelines be viewed as concerning an experiment in its entirety.

In the planning stage of any experiment, the conception of a research plan or strategy seems to be key. At this stage, the archaeological reference must be clear, and the experiment should be generally mapped with regards to method, procedure, hypothesis if using, and aims. Furthermore, the plan should concern practicalities such as materials, crew, funding, and theoretical standpoints such as the purpose of the experiment, the ethics and the relation to ethnographic finds. The level of scientific applicability, actualism<sup>27</sup> and reconstruction should be considered, together with skill levels of the experimenters and the bias that must be dealt with during the experiment. Ideally, the whole experiment from start to finish should be planned and reviewed, to in a best case scenario ensure predictable progress, or in a worst case scenario be prepared enough to effectively manoeuvre an obstacle. The majority of the guidelines put down for the experiment execution lean towards ideals of scientific experimentation, such as repeatability, measurability, and comprehensive documentation. However,

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27. 'Actualistic/actualism' is a term often found in experimental archaeology, and indicates the conceived representability of actions, structures or things.

this is procedural, and does not entail any preference towards controlled lab modes.

During the practical preparations and the actual experiment execution, the use of actualistic materials; manufacture methods and tools; and techniques for use, are given high priority. As a circumstantial guideline the aid of a skilled crew is highlighted, including archaeologists, helpers, and potential craftspeople. Throughout the execution phase, it seems important to maintain a focus on variables; isolation, measurement and which variables are decisive for the results that are sought according to the research question or hypothesis, but also to keep check on unexpected variables and their influence on the results. Although only a few authors (*Ibid.*; Reynolds 1999; Mathieu and Meyer 2002) explicitly mention measurement, this is an ideal that probably **outranks** all the other guidelines. According to discourse intelligence, education and participation in numerous experiments, the author will maintain that it is simply part of the experimental *modus operandi* to measure variables – sometimes even to seemingly no avail. The purpose is not always entirely clear. Measurements are naturally often taken due to being necessary for the purpose of the research, but sometimes measurements are taken as an emergency measure, so that it may be possible to backtrack step by step if the experiment fails. Others practice measuring out of conformity to good form, and others yet again because they were told/taught to do so. To measure variables conforms to variable isolation, variable conceptualisation and the analysis of results, but is above all considered customary on its own.

The post-experimental analysis should entail a comparison between the results and the prediction of the potential hypothesis, or evaluation of the experiment in relation to the research question, but also a conceptualisation of the results according to the archaeological reference, which is key to the entire experiment. Proof is theoretically difficult to claim, as an archaeological experiment is trying to bridge a potentially enormous gap of time, but as a justification of the results, corroborative evidence should be sought. However, it should be considered that the experiment is but an analogy to the real, archaeological situation. A final assessment of the experiment, including a situated self-reflection, is suggested as part of the post-experimental analytical part of an archaeological experiment.

Finally, almost all authors agree on the fact that results should be disseminated. Publication is given the most focus, mainly in academic journals, but some authors highlight publication to the wider audience as well. Both for the sake of repeatability, and for a general contribution to the archaeological discourse, results should be disseminated to, at a minimum, peers. A few go further and suggest that practical presentations to the public is a good way to disseminate experiments (e.g. Richter 2005: 97).

Although they may agree on certain guidelines, some authors express diametrically opposite opinions when it comes to other perspectives. For instance, a discrepancy can be seen between those who lean towards the natural sciences as ideal, here simplistically labelled as the *processual* approach (Beck 2010; Callahan 1999; Marsh and Ferguson 2010; Meurers-Balke and Lüning 2005; Outram 2008; Reynolds 1998a; Reynolds 1999; Schiffer et al. 1994); and the largely European postmodern trend, which encompasses a human focus and is reliant on post-processual influence (Busuttil 2012; Christensen 2012; Clarke and Renwick 2013; Comis 2010; Englert 2006; Kaltsogianni 2011; Liardet 2013; Nielsen 2006; Planke and Stålegård 2014).

Amongst the differing perspectives is whether or not to feature factors that can be directly linked to the human presence in an archaeological experiment. In processual experimentation, an experiment should not be directed towards human circumstances such as emotions, motive or experience (Outram 2008; Reynolds 1999). This is in line with the natural sciences and the logical principle of control (e.g. Popper 2002: 27). Nevertheless, certain experiments are directly performed as social experiments, such as ship trials that venture into chains of command, logistics and seasickness.<sup>28</sup> Also, to record time expenditure is quite common, regardless of theoretical inclination (Nielsen 2006: 20; Štěpán 2004), especially in use-wear studies (Adams 2014; Rifkin 2012; Setzer 2012), although Reynolds (1999: 157) clearly opines that this is of no value because of modern biases. Other examples of socially influenced experiments can be

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28. Interview with Anton Englert 8/5/2013, research coordinator at VSM



found in two series of indoor climatic experiments with modern humans living for short periods in iron age houses (Christensen 2012; Larsen 2007). Lately, experimenting with sensory input seems to also have become more common (Clarke and Renwick 2013; Harris 2008; Šálková et al. 2011; **Skeates 2011**).

The way in which social and personal elements are incorporated, documented and disseminated still produces results, albeit biased. This leads into another discrepancy of perspectives, namely that bias can be excluded from the experimental results (Reynolds 1999: 157). Some researchers explicitly state that bias is inherent in the researcher and cannot be escaped (Adams 2010; Rasmussen 2007a: 10). This is then translated into a principle for experimental conduct in the form of bias *awareness* instead of exclusion. Such awareness is also highly visible in the VSM research and often voiced in publication (for instance in Crumlin-Pedersen 2006: 3; Englert 2006: 37; Nielsen 2006: 20), as well as in interviews.<sup>29</sup> For instance, the VSM program dictates that their research is not produced as a reconstruction of the past but as an interpretation that makes sense in the present; typical for Scandinavian archaeology (Beck 2011).<sup>30</sup> By changing experimental aim from production of primary data to production of a meaningful interpretation in the present, recording human circumstances can be justified. This entry point into archaeology is increasingly promoted in the post-processual trend (see articles in Alzén and Aronsson 2006; and also Beck 2011; Holtorf 2010a; Petersson 2009; Kaltsogianni 2011). In some instances, biased human circumstances have been successfully translated into scientific results that feed back into the primary data production. This is the case when activity patterns from present day activities are recorded and analysed scientifically as a reference for archaeological material (Christensen 2012; Hjulström and Isaksson 2009). Such results should always be critically reviewed and a fair amount of self-reflection is needed, yet certain data production from modern activity patterns can result in viable archaeological interpretations.

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29. Interviews with research coordinator, ship reconstructor, sailor and museum inspector at the VSM

30. From the VSM Mission statement and Core Values: "The museum's basis is the present; history is related to the present, not the other way around."

### 2.1.2. Experimental circumstances

Two main ideals, lab vs field experiment circumstances, materialise in the design and performance of archaeological experiments, much resulted from the natural scientific, strict research ideals that resonate through, amongst others, Reynolds' work, and the more conceptual approach put forward by others (such as Mathieu and Meyer 2002; Rasmussen 2001). This continuous discussion of lab- versus field-based experiments is very much alive in the discourse of experimental archaeology of today (Beck 2011; and see articles in Ferguson 2010; Schiffer 2013b; Busuttill 2012). The two aspects cannot be completely united in one single approach, and there seems to be no better way of the two. Whereas the lab environment is very suitable for actual, scientific analysis in archaeology such as phytolith studies and chemical analyses, or approaches that dictate a very clean environment such as osteoarchaeology, it is less ideal for sea-trials with boat reconstructions (Englert 2006; 2012b) and animal butchering by human hands (Saladié et al. 2015). However, a lab-based experiment does not necessarily take place in an actual laboratory. Rather, the label is used to describe strictly monitored experiments with select variables that can be controlled, measured and statistically processed. Such control is grounds for an *accurate* replication of the experiment results, which is the ideal experiment form in the natural sciences. When a result can be repeatedly replicated under exactly the same circumstances, the result is corroborated under the ideal of the hypothetico-deductive method (Knowles 2000: 72; Popper 2002: 10); the model for all scientific research that rests on experimentation. Controlled, archaeological experiments are those who fulfil these criteria, for instance in the testing of select attributes of certain raw materials such as evaluating the resistance to force of minerals (Magnani et al. 2014);<sup>31</sup> or investigating the mechanical results of heat alteration in lithic material (Harry 2010: 28-32; Jeske et al. 2010); but often it is not completely achievable to fulfil all the criteria of control and measurability when trying out a technology, and only a few types of experiments can be successfully executed by this method alone.

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31. See chapter 6.X.X.

Because the lab-based experiment is quite clinical in its strictly controlled approach to a past technology, it excludes some of the elements that must have been expected to be present in (pre)historic activities. For instance, clean laboratories have only existed for a few hundred years. This means that the conditions under which most technological operations were performed in the past must have included a certain extent of dirt, dust or grit which is likely to have influenced countless procedures, either as contamination, abrasive agent or just an annoyance – or a given – to a process. Other factors that are difficult to recreate and control include levels of humidity, temperature, wind and human beings. Most experiments with actual technologies are therefore performed outside of a laboratory environment, in the 'field.' In addition, the question of scale can complicate a true lab experiment, for instance when working with large-scale experiments such as buildings that are hard to truly replicate from one to the next. Still, the lab-approach can be predominant in the level of control attempted – or achieved – in the experiment, but a true experiment after the natural science model is hard to manage with the presence of a spectrum of unknown variables. However, this has not put an end to attempts of fitting actualistic research into a frame as scientific and controlled as possible (e.g. Domínguez-Rodrigo 2008).

The opposite end of the environmental spectrum is when an experiment is taken to the 'field.' This happens when the surroundings (and the people in it) are allowed to influence the process as part of the experiment. An argument for using this specific approach is that people will always have had a certain impact on the technology in question, and it is therefore difficult to realistically investigate a human technology only in the lab. This may be where one sees the actual separation of experimental archaeology and archaeological science, which relies on a laboratory approach alone (Outram 2008: 2). Field experiments comprises a scale of representativity, from narrowly actualistic to fully reenactive modes. Examples of studies include beer brewing in hide-lined cooking pits which failed when wild, air-borne acetobacter started fermenting the beer into vinegar (Odgaard 2008); a reconstructed Viking ship that kept losing its rudder during an expedition and where the crew could not figure out what was wrong with the fastenings (Nielsen 2011); going hiking in fibre sandals

(Geib 2000); testing function and capability of spear throwers (Whittaker 2010); experimentation with flint debris (Carr and Bradbury 2010) and function testing of shaft furnaces for iron smelting (Narmo 2011). All these examples rely on human involvement for the actual execution, and not to mention for the interpretation of concepts such as 'functional.' They also have in common that they were performed outside, without attempting to control the natural environment. A field experiment is, however, not necessarily conducted in the wild. A field-based approach can be said to cover any *modus operandi* where uncontrollable factors are allowed to interfere with the experimental process. It should be remembered that the terminology of 'field' and 'lab' are only names for two categories of experimental strategies, and that experiments with one mode does not exclude the other (Comis 2010; Outram 2008).

Field investigations generally aim to provide a foundation as actualistic or representative as possible for the execution of experiments. Several problems may arise with this way of working, first and foremost that researchers lack the variable control to know exactly which factors are contributing to the results. When an experiment is taken to the field, it is also sometimes difficult to maintain control of the variable that is tested. Even if the best available measuring equipment is used, it can be hard to exclude the influence of unknown elements with certainty. This leads to difficulties in adhering to a strict hypothetico-deductive experimental method.<sup>32</sup> However, by using the field approach to archaeological experiments, the researchers may be able to discover sides to the technology that are not observable in a laboratory. An attribute may be examined and tested in a lab-based experiment, before being taken to a second, field-based test to study the practical behaviour or potential of this very attribute. Performing a second test in a different mode can function as corroboration and the combination of the two is common. By combination, it is possible to select and strictly monitor a variable before examining if this influences the practical use and performance characteristics of the technology. It is of course also possible to reverse this and conduct a broad scale exploration of the technology first, before moving on to the laboratory to look for specifically influencing variables. For example, in a hydrodynamic towing tank,

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32. Section 3.2.2.

the lab mode experiment provides isolated perspectives of how the keel, stern shape, or other ship parts work, whereas the field mode evaluates the reality of the procedure. The hydrodynamic towing tank can connect separate ship parts to functionality issues, and may therefore highlight choices. However, those choices are in reality a fictional idea and true functionality must be explored by extensive sailing, which may highlight different choices (Crumlin-Pedersen 2006).

It is important to note that a lab experiment is currently the only properly controlled mode of experimentation. 'Control' means that all variables apart from the one that is tested *must* be kept static – they are thereby truly controlled, not merely measured. Experimenters do sometimes use the label 'controlled experiment' without having controlled variables appropriately, and in this regard, as the use of the word is closely associated with the natural sciences and is generally used to describe the protocol or method in the experiments in question, the term is not precisely applied (e.g. Setzer 2012; Pétilion et al. 2011). Such use may be a result of the modified control theory mentioned above (Petersson and Narmo 2011a: 31), where science is idealised – perhaps sometimes to the excessive when control is considered. As a result of strict control in a lab mode, one drawback with the controlled lab mode is the lack of representativeness that may result from creating a fictional situation in a static environment. Therefore, if an archaeological experiment intends to create an actualistic situation, even when experimenters monitor a number of variables closely with advanced measuring equipment, it is still a field experiment, albeit a very well documented one. Nevertheless, it is important to note that the necessity of such close monitoring is dependent on the research question. It is therefore perhaps pertinent to not pass judgement on experiments that have decided to perform their experiments in a different manner, as long as the results are not weakened by their choice (Narmo 2011); especially significant in the environment of two trends, albeit the experience author is that some experimenters do display a distinct predisposition for one or the other.

The question of what remains an typical experiment in archaeology is by no means easily answered (for instance Beck 2010: 59; Harry 2010: 22-23; Whittaker 2010: 211). For the choice of modes or experimental circumstance,

the typical use of either lab- or field-based approaches, or the combination thereof, seems to come down to the research question in each experimental case. This choice will be incorporated as part of the typicality of experimental archaeology for the case studies in part II.

### 2.1.3. Categories of experimental research

There are a number of ways in which an experiment can be approached, **very often** determined by the research problem and the part of the *chaîne opératoire* it concerns. Some experiments concern the manufacture phase, whereas others analyse functionality issues or raw material potential, and yet others deal with the study of discard and abandonment of a technology. Such studies are by Caroline Jeffra coined *single-segment experiments*, whereas she labels experiments that combine chaîne opératoire stages *combined/multiple-segment experiments*. She bases her categorisation wholly on the chaîne opératoire approach, and suggests that this sequential incorporation not only helps to structure an experiment, but also yields definition for the supplementary research that should go into an experiment to provide social backing to the research into production sequences. In this way, Jeffra's (2015) categories are defined to create either an isolated scrutiny, or a way for initial phases to function as a frame of reference for the research questions of subsequent phases. This allows researchers to assess if and how the structure of the experiment and its supplementary research is pertinent to their need, and may provide a useful conceptualisation of the research design.

As Jeffra (*Ibid.*) indicates through her categorisation, some studies are directed towards testing a whole process of manufacture, use and discard, others just one variable. Reynolds (1999) has suggested a much-cited categorisation of experiments based on the technological aspect to be explored.<sup>33</sup> His first category is the *construct*<sup>34</sup> and is defined as a 1:1 exploration of a structure, mainly concerned with the feasibility of the interpretation of the structure itself.

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33. It is specified that the categories are by no means exclusive, and are only created to simplify the complexity that archaeological experiments really amount to (Reynolds 1999: 158)

34. Reynolds makes it very clear Reynolds (1999) that he consciously avoids the term "reconstruction" due to the lack of certainty of the structure itself. For instance, a number of constructions can be based on a certain amount of postholes.

The actual manufacture of the structure is incorporated in his second category, *process and function experiments*, which regards technological processes and functionality trials as a whole. Within this comes the processing of technology, either raw material or structures (Outram 2008: 3; Reynolds 1999: 159). Investigations into procurement of raw material, manufacture and use all fall into this group, which thereby generally address chaînes opératoires in the wider sense;<sup>35</sup> from manufacture through use to abandonment.

There is a sliding transition towards *simulation* experiments (Reynolds 1999: 160), where experimentation is more focussed on the artefacts and structures than their task-scape, and which includes research into material culture taphonomy in the broadest sense. Reynolds, however, stated that simulation experiments must necessarily be long-term, and put forward a focus on natural processes as central to this category by pointing to burial experiments such as Overton and Wareham Down where a variety of raw materials, such as leather, pottery and bone, were buried to observe erosion over time. It is unclear if Reynolds intended simulation experiments to include short-term taphonomy studies, such as use-wear analysis or cut-mark analysis.

Reynolds' fourth category<sup>36</sup> are the *eventuality trials* (*Ibid.*: 160-162), a blend of construct, process/function and simulation experiments where the aim is to explore "the potential product," instead of isolated variables. Precisely the large number of variables involved complicates the controlled measurement of the factors selected for experimentation, and so Reynolds proposes a strict frame within which limits the experimentation should take place. Examples include Reynolds' own agricultural experiments with ancient field systems (*Ibid.*: 161-162) and the speed testing of a Viking ship under various conditions (Englert 2012b).

Reynolds is not the only one to consider the classification of experimental archaeology. Several (2010: 137; Adams 2002: 65; Amick et al. 1989; Malina

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35. See p. 24.

36. There is also a fifth category to Reynolds' experiments, which deals with technological innovation in archaeological research. This is meant to include experiments with new equipment and methods (Crumlin-Pedersen, 1995: 305), and is rarely included in the general definition of experimental archaeology.

1983) have divided experiments into *confirmatory* and *exploratory*, depending on experimental mode. Another perspective similar to this categorisation is Rasmussen's division into *controlled* and *contextual* experiments: a controlled experiment seeks to isolate variables and provides measurable and repeatable results, whereas a contextual experiment does not aim for variable isolation but instead provides inspiration, arguments and constitutes an evaluation of relevance (Rasmussen 2001: 6). Rasmussen's contextual experiments therefore constitute a broader category than just exploratory experiments, as she does not request a specific experimental motivation such as "exploration." She involves, amongst other examples, living/social and non-replicable experiments that can be difficult to formally document but will provide a broad spectrum of interpretation and analogies. She also points out that speaking of controlled experiments in a full-scale model reconstruction is "absurd [...] although elements of these will be present" (Rasmussen 2007a: 11). This mirrors the control-issue pointed out earlier.<sup>37</sup>

James Mathieu (2002b) has constructed a system for experiments in which he classifies experiments by scale of approach. In this way, he intends his system to actively feed from one category to the next, where small scale experiments together can build up towards a large scale approach, or a large scale experiment can uncover aspects of a smaller scale that need investigation.

Mathieu's smallest scale experiment, *Object replication*, is the exploration of a replica that has characteristics in common with the original, archaeological object. He divides object replication into *visual replicas* – copies that provide visual information such as a cast of an archaeological item; *functional replicas* – copies that explore the functional aspects and where a selection of necessary, authentic variables are experimented with; and *full replicas* – copies where an accurate production of a true replica is explored where the replica can be exchanged for a real artefact (e.g. experiment reiterated in Ascher 1961: 800-801). An example of an object replication can be for instance the casting of archaeological artefacts for analysis of wear trace (Khreisheh et al. 2013: 7-8; Lawson 1999).

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37. Page 55.



One level up on the scale, *behavioural replications* engage with past behaviours and activities. This includes reproduction of techniques and usage, which requires the use of *functional* or *full replicas*. Behavioural replication is divided into *functional replication* – replication based on the generation and testing of hypotheses concerning the function of certain objects; *comparative experiments* – replication that compare the outcome of behavioural replication; and *phenomenological studies* which aim to replicate sense perception, cognition or emotion, and which are not true experiments but can be set up following experimental terms. Examples include exploration of space (Clarke and Renwick 2013; Gifford and Acuto 2002; Kaltsogianni 2011).

Mathieu's experimental category *process replications* works to reproduce both natural and cultural processes. Process replications are mainly concerned with *formation processes* – how archaeological material is produced and transformed over time, and *technological processes* – the study of technological processes such as flintknapping or tar distillation, and Mathieu includes a large proportion of the *chaîne opératoire* in this category. Significantly, he addresses the fact that many smaller scale experiments should be focussed into a broader perspective to be able to make conclusions about the overlying technological process, and a process experiment will therefore contain both object and behavioural replications. A third category is computer aided *simulation studies*, which Mathieu considers to fall in line with the definition of experimental archaeology, although they are not involved with tangibly physical phenomena. These experiments can be useful for the replication of large-scale processes that are otherwise difficult to control, and are valuable instruments for the generation and testing of hypotheses and analogies (Mathieu 2002b: 5).

The last category of Mathieu's typology is the *system replication*, which is the highest level of experiments and involves the reproduction of a variety of processes in a *social* system. Because these experiments are extremely difficult, or even impossible, to control, they are usually pursued through ethnoarchaeological or ethnographical studies. This can be justified by the existence of observable, "experimental" data in a living society that can then be used as an analogy to archaeological interpretation, much like experiments.

Examples include pottery firing experiments executed by local societies in Cameroon (Gosselain 1992a; Smith 2001) and large scale (actualistic) sea-trials (Nielsen 2011). Such experiments can open up many individual issues to explore on a lower level. This means that even if they are not necessarily strictly scientific in procedure and form, they can be sources of large-scale information concerning societal structures and technology and feed ideas back into individual, smaller scale experiments. System replications are a good example of Rasmussen's concept of a contextual experiment.

Other researchers have also been concerned with the classification of experiments in archaeology. Edward Jolie and Maxine McBrinn (Jolie and McBrinn 2010) have a technological focus, and divide experiments into four categories: *technological studies*, *performance studies*, *contextual studies*, and *ethnoarchaeological and enculturative studies*, and while they resemble those of Mathieu, the material objects, not the social level of scale, take centre stage. That means that a use task will be connected to how the object responds, whereas in Mathieu's categorisation, such experimenting also pertains to how the researcher experience functionality. Pascale Richter has grouped experiments into *experiments with the reconstruction of: natural formation processes on archaeological artefacts; anthropogenic influence on archaeological artefacts; human behaviour, and cultural development* (Richter 2005: 100).

Lastly, Seán McGrail has divided maritime experiments into 'specific' and the 'representative' approaches, referring to whether the experiment is trying to reconstruct a specific object, or a general representation of the archaeological reference (McGrail 2006). Although in total, the list of different categorisation systems is long, quite a few researchers are not at all concerned with categories and instead deal with the broad distinction between 'experience' and 'experiment', or debate the issue but rather want an all-encompassing theory for conducting experiments (Hansen 2008; Narmo 2011). Some feel that an experimental approach cannot be completely distinguished from ethnoarchaeology (Beck 2010), and quite a few integrate lab- and field-based experiments without really creating a distinction (Coles 1973: 15-18; Jeske et al. 2010). Often, experiments are simply grouped by technology or raw material

(such as articles in Ferguson 2010), and there is also a tendency to classify into method of approach such as geoscientific studies and taphonomic experiments, as could be observed in the thematic division of sessions at the 6th Experimental Archaeology Conference in York, 2012.<sup>38</sup>

Particularly in the United States, many researchers seem to request more generalised experimental programmes for the design and execution of experiments, either directed towards experimenting with a certain technology or universally applicable (e.g. Harry 2010: 15; Jolie and McBrinn 2010: 164; Marsh and Ferguson 2010: 6-7; Schiffer et al. 1994; Beck 2010). No-one has come close to successfully construct an all-encompassing framework with general consensus, and whether such a complex and broad field with so many different classifications, definitions and foci can come together in agreement for just one approach, is debatable. As it happens, the entire field of Archaeology must be said to display exactly these features, and in many ways this is what makes the discipline move forward, through the existence of a continuous discourse concerning how to approach the material culture of past societies.

## **2.2. The multidisciplinary approach: a case study**

The academic approach is constructed by academics and usually concerned with academics. Very often, archaeologists themselves perform the experiments, perhaps aided by professionals, but usually the academic is in charge. A different entry into archaeological experiments was explored through fieldwork at the Viking Ship Museum (VSM) of Roskilde in Denmark, where one of the main activities is the reconstruction and use of the five Viking ships that are on display at the museum (Nielsen 2006: 16). The fieldwork was undertaken in May 2013, and a total of 6 interviews were conducted, of research coordinator, boatbuilders, museum inspector, volunteer sailor, ship reconstructor and sail maker. Additionally, the stay included visits to the exhibition, the wharf, an international seminar, and general presence in the administration.

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38. Conference programme available here: <http://experimentalarchaeology.org.uk/archive-of-past-conferences/6th-experimental-archaeology-conference-york-2012/list-of-papers/> [Accessed 17/8/2015]

In Roskilde, 5 viking ships were recovered in 1962, and later exhibited in purpose-built museum. When the museum decided to expand, a further 9 wrecks were found and recovered. All these wrecks now form the basis of the VSM, and work continues into their reconstruction and function assessment. At the VSM, professional boat-builders carry out reconstructions in the museum's own boatyard.<sup>39</sup> The use of professional craftspeople is common in experimental archaeology, but at the VSM, boat-builders, rope makers, sail makers, and sailors far outnumber the archaeologists involved in the experimental activities of the museum. This pioneering structure (see McGrail 2006: 14) incorporates craftspeople and archaeologists in communal research teams, and findings are published by authors irrespective of their archaeological academic education (e.g. Bischoff and Jensen 1998; Finderup 2006; Magnus 2006; Nielsen 2012). Both the head of the Maritime Crafts, Reconstruction and Public Activities division, and the ship reconstructors are professional boat-builders. The coordinator of the research division at the time was an archaeologist and professional boatbuilder. In sum, this leads to a significant competence when it comes to reconstruct archaeological shipwrecks.

In terms of experimental archaeology, this is a rather unique approach. However, in terms of ship reconstructions as well as other building projects such as houses, making use of professional teams is often done during the actual construction phase (e.g. Arnold 1999: 9; Hoheisel 1994: 257; Marlier 2006),<sup>40</sup> likely because the skill level necessary to reconstruct a functional structure far exceeds that of the average archaeologist. The unique aspect of the experimental methodology of the VSM is the non-hierarchical involvement of the professionals (Nielsen 2006: 18)<sup>41</sup> and the acknowledgement that they offer an expertise that is very different from the archaeologist's own (Crumlin-Pedersen 2006: 3).

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39. From the museum's vision: "The museum must possess a specialist competency within reconstruction and traditional maritime craftsmanship. The museum shall carry out boatbuilding and rope making itself, and shall also participate in networks with craftsmen from other relevant professions." (<http://www.vikingskibsmuseet.dk/en/about-us/mission-vision-and-values/> - [accessed the 1/8/2013])

40. For instance with the *Hansekogge*, where the building process was managed by Uwe Baykowski, a boat-building-master, and scientifically surveyed by Dipl. Ing. W. D. Hoheisel of the Deutsches Schifffahrts-Museum ([http://www.hansekogge.de/?page\\_id=251](http://www.hansekogge.de/?page_id=251) [accessed 2/8/2013]).

41. Interview with boat-builders 15/5/2013

The multidisciplinary approach pursued by the VSM was first advocated by Ole Crumlin-Pedersen (1995: 303),<sup>42</sup> who offered the following definition for what constitutes the experimental process at the VSM: "...to enable the investigator to present the evidence of the find in a way that is as competent as possible, after having had to cope with the range of problems in constructing the ship and putting it through sea-trials as a fully functional unit" (*Ibid.*: 303-304). Crumlin-Pedersen considered the approach a "splendid tool" to study the complexity and context of the ship as a self-contained and functional unit (*Ibid.*: 306; 2006: 4). Although this definition is centred on ships as artefacts, it can also be applicable to other types of structures, such as houses, fields and cooking pits, and to technological processes.

### **2.2.1. Codes for conduct**

The VSM has been developing its own approach to experimental archaeology since its first ship reconstruction in 1982. As it comes forward through interviews<sup>43</sup> and publications (1996; Crumlin-Pedersen 1995; Finderup 2006; Nielsen 2006), the method has been standardised and is agreed upon across the museum. Its main aspects are described below.

Firstly, and reflecting the general experimental discourse, it has been established that a thoroughly documented archaeological find must *always* be the reference point for archaeological experimentation (Crumlin-Pedersen 1995: 304). This may sound self-evident, but has become one of the key features of the VSM approach to reconstruction. The explicit focus has largely to do with the experience and reference world of traditional craftspeople, and the fact that their present day knowledge must be conceptualised in order to be applicable to technologies that are no longer known. The same statement was independently encountered in the interviews of boat-builders, the ship reconstructor and the

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42. Crumlin-Pedersen was a key figure setting up the VSM, who also dedicated his career to the experimental research at the museum. For his obituary, see <http://www.vikingskibsmuseet.dk/en/about-us/ole-crumlin-pedersen/obituary-by-tinna-damgaard-soerensen/> [accessed 6/8/2013]

43. As part of the investigation of the definition of experimental archaeology, a research stay was undertaken at the VSM in May 2013, to observe and discuss their interpretation of experimental practice. This resulted in 6 interviews with various staff involved with experimental activities.

research coordinator, namely that the VSM tries to avoid the pitfall of thinking “this is how it *should* be done, because this is how we *always* did it [in my crafts tradition]” (and Crumlin-Pedersen 1999: 191; Finderup 2006: 23), which can appear when a craftsman does not understand why a structure has a particular form.<sup>44</sup> Loyalty towards the archaeological record is paramount, which has at times resulted in re-interpretations and dismantling and changes to a ship reconstruction.<sup>45</sup> Only after the archaeological find has been repeatedly scrutinised, other sources such as tool finds, iconography, ethnology and experience are carefully brought into play. In other words, there is an awareness of hermeneutic thought processes and of how questions are answered and new questions posed (Englert 2006: 37; Finderup 2006: 21).<sup>46</sup>

Procedural awareness becomes important when skill and experience is another prerequisite. To be familiar with a reconstruction such as a boat; to foresee and tackle problems and structural issues; and to deduce which tools to use and how, requires skill that has been built up over years (Crumlin-Pedersen 1995: 304; Finderup 2006: 26; Nielsen 2006: 17). Several professions meet in the course of each construction; boat-builders, rope-makers, smiths, sail-makers, sailors and archaeologists. The boatyard is a synthesis of relevant knowledge needed to build a boat according to the museum principles, where the trade of boat-building is seen in a broad sense, encompassing every aspect from cleaving trunks to manning and sailing the ship (Nielsen 2006: 16). The skill of each profession is drawn upon for their relevant tasks, and the wharf also has an archaeological workshop with advanced equipment for documentation, subsequently resulting in models.<sup>47</sup> Multidisciplinary is highlighted as a crucial part of any experimental project at the VSM (*Ibid.*; Finderup 2006).

The VSM maintains that, although public archaeology is important, research is

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44. For an example of the opposite, see how Richard Thér allowed "rational considerations" by modern potters who performed firings in 'natural' kilns (2004: 64).

45. Interview with 15/5/2013

46. This must be considered typical for Scandinavian archaeological thought (Jensen and Karlsson 1999).

47. The VSM employs a FaroArm coordinate measuring machine for exact documentation of components.

the base activity undertaken at the museum (Nielsen 2006)<sup>48</sup> and the experiments are aimed towards academic standards, including a call for a strict methodology (Crumlin-Pedersen 2006). The research focus becomes apparent in the claim that a research strategy must always be in place before beginning an experimental process. As part of this plan, other outcomes must also be considered, such as educational value. Partly for this reason, documentation of all stages of the process, from planning to finished product must take place (*Ibid.*; Crumlin-Pedersen 1995: 305). The importance of documentation is visible through the extensive publication series dating back to 1969 and is also included in the vision of the museum.<sup>49</sup> The boatyard has an online database of all its boats, both reconstructed and original, including exact measurements. Documentation even plays an important role in the display of the archaeological originals, where a plan drawing of the archaeological site and estimated measurements are provided for each wreck.

Documentation is also important for the subsequent dissemination and publication of the results, which is another component in the VSM methodology. The publications should be directed to both local and international audiences; academics, craftspeople and general public alike, and are always attempted in a minimum of two languages,<sup>50</sup> which is expressed as a general necessity for countries where the native language is not English (Crumlin-Pedersen 1995: 305). The museum aims to publish all their archaeological reconstructions and experiments,<sup>51</sup> and maintains a significant presence in international journals for maritime archaeology.<sup>52</sup>

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48. Interview with research coordinator 8/5/2013. Research is an important part of the museum visions: <http://www.vikingskibsmuseet.dk/en/about-us/mission-vision-and-values/> [accessed on 5/8/2013]

49. <http://www.vikingskibsmuseet.dk/en/about-us/mission-vision-and-values/> [accessed on 5/8/2013]

50. Interview with research coordinator 8/5/2013

51. Interview with research coordinator 8/5/2013

52. For an updated list of staff publications: <http://www.vikingskibsmuseet.dk/forskning/medarbejdernes-publikationer/> [accessed on 5/8/2013]

### 2.2.2. Experimental circumstances

The VSM mainly practices field-based<sup>53</sup> experiments with their reconstructed vessels, although it has been maintained that lab-like conditions can be achieved by taking certain elements into consideration (Englert 2006: 35). In specific situations, a higher level of control is attempted, for instance relating to model ability testing in hydrodynamic towing tanks at 1:10 prior to reconstruction (Crumlin-Pedersen 2006: 4), or for speed trials (Englert 2006; 2012a). However, since very few aspects of full-scale testing can be performed without humans as an active component, the level of control varies. People function as both crew and dynamic ballast,<sup>54</sup> and are also highly biased towards sailing methods and experience, which is not replicable in the scientific sense (Englert 2006: 36).<sup>55</sup> The sea and weather conditions provide other uncontrollable, but necessary, constituents. However, according to the VSM, models cannot calculate the countless variables that are involved in a functionality test of a vessel at sea, and the experiment setup is therefore also geared towards actualistic aspects specifically (Crumlin-Pedersen 2006: 3).

Since the aim of the museum is to reconstruct their own and other original Viking and Medieval ships 1:1, quite a few of the experiments are massive undertakings both in sense of production and execution, which often takes the form of an expedition. This can at time call for sponsors and some form of public outreach of the experiments, which changes the circumstances of the experiment compared to non-educational field experiments somewhat. However, the VSM sees the educational sides to an experiment as non-experimental (Crumlin-Pedersen 1995: 303-305).<sup>56</sup> A difference lies in the reconstruction process, which is presented to the museum visitors continuously, and where the craftspeople have public outreach as part of their job.<sup>57</sup> This can also alter the conditions for the reconstruction, especially with regards to man-hour calculations (e.g. Nielsen 2006: 20). However, time expenditure elements

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53. See 2.2., page 52

54. Interview with research coordinator 8/5/2013

55. Interview with sailor 19/6/2013

56. Interview with sailor 19/6/2013

57. Interview with boat-builders 15/5/2013



are not the focal points of an experiment at the VSM.

There is another aspect to the field experiments of the VSM that involves the human factor. As becomes especially visible in the experiments with the *Sea-stallion of Glendalough*,<sup>58</sup> the expedition was also concerned with more social aspects of logistics in the Viking Age, such as the amount of food necessary; and command structures and social interaction in a cramped space.<sup>59</sup> As pointed out in section 2.1.1., this may be seen as controversial from a more scientific point of view, as these are aspects that cannot easily be recreated, nor are **they** necessarily representative. However, the VSM directs a large amount of attention towards the underlying society of a vessel, and aspects of transportation and logistics, resource management and even social dynamics (Crumlin-Pedersen 1999: 141; Crumlin-Pedersen 2006: 3; Nielsen 2006: 18-19), and consequently aims to contribute to the grand-narrative of the Viking and Middle Ages. There is, however, an awareness that these analyses are biased (e.g. Finderup 2006: 24), which alleviates determinism. An example of bias is the aspect of safety (Crumlin-Pedersen 2006: 4) which trumps experimental concerns.<sup>60</sup>

### 2.2.3. Experimental categories

At the VSM, there is a general consensus from boat-builders to research coordinator that they are involved with experimental archaeology. However, they do not clearly define what an experiment is, although it is unambiguously connected to the reconstructions. Some highlight the reconstructional process itself, and the recreation of ancient building techniques as observed in the archaeological artefact, as the main activity for the experimental archaeology at the museum (Crumlin-Pedersen 1995: 303).<sup>61</sup> Others see the reconstruction as a skilful piece of craft and the experiment to begin when the build is over although some experimental aspects are involved in the reconstruction, such as choice of tools and reconstruction of elements that are not preserved.<sup>62</sup> What

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58. Reconstruction of the Skuldelev 2 wreck

59. Interviews with research coordinator 8/5/2013 and ship reconstructor 15/75/2013

60. Interview with research coordinator 8/5/2013

61. Interview with boat builders 15/5/2013

62. Interview with ship reconstructor 15/5/2013

seems clear is that experiments should be research driven (*Ibid.*; 2006)<sup>63</sup> and that classical experimental activities, such as attempts to recreate tool-marks (Finderup 2006)<sup>64</sup> and subsequent sea trials (Englert 2006) are generally considered experiments. The lack of strict definitions may originate in the notion of a sliding transition from experimental to general archaeology,<sup>65</sup> and a significant identity as actors in the maritime archaeology discourse rather than prominence in the general experimental debates.<sup>66</sup> In addition, as mentioned, Scandinavian archaeology leans heavily towards postmodern influences and does not necessarily encourage the same definitions of what an experiment is, as do classical academic definitions elsewhere (Beck 2011). The VSM therefore operates without any categorisation of their experimental activities.<sup>67</sup> The categorisations proposed by Rasmussen (Rasmussen 2001; 2007a; 2007b)<sup>68</sup> may lend some perspective to understand the Scandinavian, theoretical tradition so closely linked with interpretive aspects rather than strictly defined categories.

### **2.3. A typical experiment in archaeology**

As should be evident from this chapter, there are numerous definitions of what experimental archaeology is and of how it should be conducted. Whether an experiment should be performed as a lab- or a field-experiment, more or less actualistic, as an exploration or a test, along with a multitude of other factors, is presently up to specific experiment conductors. However, some elements come forward as near universal throughout this chapter and can function as indicators for a typified experiment. These can be summed up in the following list of experiment characteristics:

- An experimental plan must be in place, considering the entire process.

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63. Interview with research coordinator 8/5/2013

64. Interview with boat builders 15/5/2013

65. Interview with research coordinator 8/5/2013

66. This is particularly visible in the long list of staff publications: see <http://www.vikingskibsmuseet.dk/forskning/medarbejdernes-publikationer/> [accessed on 5/8/2013]

67. However, as becomes clear in both public displays and in the ship reconstructor interview, they do distinguish between *reconstruction* (recreation of something that once existed), *restoration* (reparation of existing boat) and *new construction* (a new build of a modern, traditional type).

68. See 2.1.3., page 56

- The experiment must be relevant to an archaeological primary reference.<sup>69</sup>
- Variables should be monitored and measured.
- The experimental procedure must be tightly controlled for a lab experiment or adequately actualistic and relevant for the hypothesis/research question in the field, including:
  - The experimenters should be in possession of a minimum level of relevant skills for their task.
  - The materials and techniques used should be actualistic to the necessary degree.
- The experiment should be documented honestly, and reported through formal or informal publication, or presentation.
- The experiment should enter the archaeological discourse for evaluation.

These criteria will hereafter be considered the crucial benchmarks that need to be in place in order to state viability of an academic experiment, and will therefore be a part of the experimental protocol later applied in the typified case experiments in Part II. In this blend of criteria there are both controlled and contextual factors to consider; coinciding more or less with a scientific, neutral ideal or a situated, hermeneutic outlook. As will be explored in the next chapter, these are paradigmatic differences that hardly ever explicitly coexist in a research strategy, as one aspect would argue against the application of the other. Nevertheless, the two sides both occur in the terms for what a viable archaeological experiment should be. The scientific view prevails in the focus on variable isolation, but the hermeneutic aspect can be found in the criterion of actualistic behaviour and the practice of crafts which originate from a merging of mind and bodily agency (knowledge and know-how)<sup>70</sup> in the task performance of practitioners. Hermeneutic aspects can also be seen in de facto inclusion of uncontrolled variables in the majority of experiments; directly opposed to the base criterion for experiments in the sciences, which are always controlled for, and which is the presumed reasoning behind the criterion about honest and effective documentation and dissemination.

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69. But see Bodil Petersson and Lars Erik Narmo's discussion of this criterion (Petersson and Narmo 2011a: 34f)

70. See Apel 2008

Interestingly, this mix of paradigms is described as one whole protocol by several authors (e.g. Coles 1973; 1979; Crumlin-Pedersen 2006; Cunningham et al. 2008; Kelterborn 2005) Even more so, the brief evaluation of the discourse found in Table 1 points to temporal trends: a firm focus on the scientific elements correlates with the processual or "New" archaeology paradigm, which is still strong in the archaeological sciences and in US experimental archaeology (e.g. articles in Ferguson 2010). The interpretive trend is in compliance with the last few decades' postmodern focus in the general archaeological discourse, increasingly mirrored amongst European experimental authors (Busuttil 2012; Bánffy 2012; Rasmussen 2007a). These trends show that paradigmatic criteria colour archaeological practice, but experimental archaeology still retains a little of both paradigms, and as the discourse is inconsistent on these premises it seems pertinent to go deeper into this paradox to justly evaluate the method and its conceptual terms. As concepts seemingly do play a large part of the experimental methodology, one part of the assessment is to consider the potential existence of conceptual barriers that research on intangible, technological aspects has to overcome. It is in this regard that the next chapter will look at the paradigms of experimental archaeology and what they may mean for present, experimental practice.

### 3. Paradigms of experimental archaeology

#### 3.1. Paradigmatic phrasing in experimental archaeology

The analysis of experimental discourse in the previous chapter ended with a list of criteria put forward for a viably produced, experimental result. As became visible in Table 1,<sup>71</sup> the discourse has recently moved towards a less scientific, more contextual and interpretive view of the past. Even so, elements of the guidelines listed for a viable experiment<sup>72</sup> are still largely positivist in ideal. This contrasts the seemingly fluid and embedded execution and interpretation of an experiment, as will be demonstrated below. A significant problem in this dynamic becomes visible when considering that an argument which contains two opposing paradigms would often result in an **epistemological incoherence**. This may possibly have been one of the reasons for the paradigm shift from positivist processual to postmodern post-processual archaeology that occurred in the early 1980's (Jones 2002). But before this can be demonstrated in detail, a few clarifications are needed and will be offered in the following.

A paradigm can be described as the foundational, dogmatic thought that forms the basis of all statements made under it, through making them feel natural. If the paradigm changes, generation of knowledge will occur in a profoundly new manner, and knowledge generated under the influence of the previous paradigm is often seen as deviant or uninformed, and is sometimes discredited in its entirety (Kuhn 1996). In the positivist paradigm, the foundation is the belief in the world absolute. **The core of this view is that the universal truth about the world exists as a positive, and that rational thinking<sup>73</sup> has a definition** (Bryant 1985; Friedman 1991; Johnson 2000; Knowles 2000). In science, this principle underlines all knowledge, which generation is justifiable in its own right – we are, simply put, mapping the world around us. The postmodern paradigm is

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71. See Table 1 for a summary.

72. Listed in section 2.2.3.

73. The concepts of 'rational' and 'irrational' are subject to much discussion in the philosophy of science, and this discourse will not be further explored here (for more on this debate: Johnson 2000; Knowles 2000).

generally hermeneutic<sup>74</sup> in outlook, and as such overthrows the positive existence of objective facts (Aylesworth 2013). There is no one justification for how to generate (which) knowledge under a postmodern perspective. Researchers cannot say that they are mapping the world, because hermeneutics dictates that everyone takes *differing* views of it. Under the postmodern paradigm, it is rather the human condition which is discussed (e.g. Mignolo 1999; Olsen 2012 with comments). However, elements of the knowledge-in-its-own-right morals that influence on positivist research, do remain in the entire existence of the discipline of research, which causes some logical discrepancy within the postmodern paradigm. As logic is no longer considered a universally valid explanation mode, this does not cause concern for the individualistic postmodern commonality, which is no longer seeking universality (Aylesworth 2013).

Although the word 'paradigm' rarely appears in the discourse of experimental archaeology, terminology and descriptions that have typical paradigmatic meaning do occur quite regularly. For instance, hermeneutics are sometimes discussed (Beck 2011; Bánffy 2012), and sometimes even taken for granted as a theoretical backdrop (Johansson 1987; Rasmussen 2001). In much the same way, the hypothetico-deductive method is commonly cited (Comis 2010; Domínguez-Rodrigo 2008; Nami 2010; Outram 2008). Nevertheless, although the concept of 'hypothesis' is well understood, the same cannot always be said for 'deduction,' (e.g. Coates et al. 1995: 295; Kleisariis et al. 2014: 110) which, when used about a logical thought process or experimental structure, has a specific meaning. When Reynolds claims that "Experiment is no more than the application of deductive logic reinforced by physical testing," his iconic stature in experimental archaeology necessitates an elucidation of what is meant by deduction.

The hypothetico-deductive method (HDM) is a test according to logical principles on how to infer something from a given set of data. It is based on the testing of premises in a hypothesis in order to make a generalised conclusion (Bogen 2014). According to Jonathan Knowles (2000: 72), the procedure can be

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74. See definition on p. 22.

presented as follows:

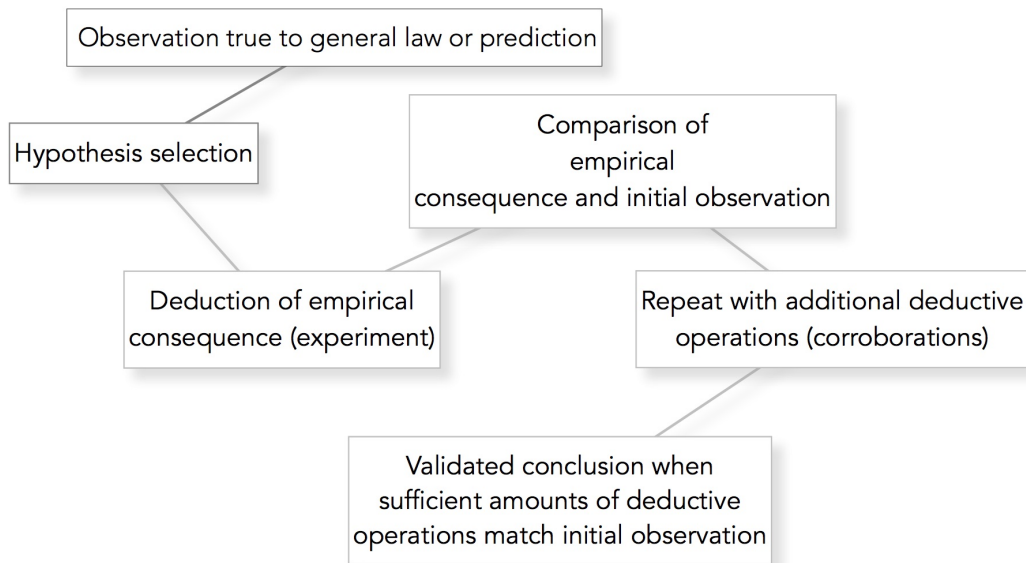


Figure 2: The principles of the hypothetico-deductive method

The HDM is based on the logical principle of deduction, a formal logic called a *necessary inference*. If the premises to which deduction is applied are true, the inference must necessarily also be true. So: if the premises are true, it is impossible for the inference to be false, which makes it a strict form of logic (Douven 2011; Johnson 2000: 58). Although the HDM is sometimes applied as an experiment template or directions for testing, in this thesis, the HDM will be used to refer to its original meaning: a test of premises reliant on necessity.

One classic, Aristotelean example that demonstrates the necessity of a deductive inference is the logical formula *modus ponens*, formulated as

$\text{If } p, \text{ then } q$	Premise
$p$	Premise
$q$	Inference

A real-life example could be:

When in sunlight, black surfaces feel warmer than white surfaces	Premise
These black surfaces are in sunlight	Premise
<hr/>	
They feel warmer than white surfaces	Inference

To use modus ponens in HDM means to use it to verify; a difficult logic under which to perform any test, which was discussed and determined by Popper in his seminal *The Logic of Scientific Discovery* (Popper 2002 [1935]). Modus ponens is only truly used in mathematical types of research (Dommasnes 1987). The fact that it is a necessary inference of verification, means that we do not actually have to be present to observe what the result is – we know what it will be from the outset, as the whole inference must necessarily be valid. This includes the hypothesis. Illustrated with the example above, this means that we do not need to feel white surfaces to know that the black surfaces are warmer. Popper's HDM uses the negative counterpart of modus ponens, *modus tollens*, to instead falsify hypotheses with a necessary inference (Bertilsson 2004: 383). One of his main statements is that we cannot logically verify hypotheses since the development of science rests on the discovery of new elements (which consequently become first premises) (Popper 2002: 10). We know that science does change/develop. Therefore, the hypothesis test should be in the falsification form to allow for new scientific discovery to be justified (*Ibid.*; Dommasnes 1987):

If $p$ , then $q$	Premise
$not\ p$	Premise
<hr/>	
$not\ q$	Inference

In this mode, when the premises are true,<sup>75</sup> the inference is not a confirmation, but a negation. Modus tollens therefore leads to falsification rather than verification of a premise (Bertilsson 2004: 383). In practice, the test could for instance be to apply something that is not  $p$  and observe the outcome. If the result is still  $q$ , then the test has proved the hypothesis (the first premise) wrong.

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75. This means that  $q$  *exclusively* occurs together with  $p$ .



For instance:

Only in sunlight (p) do black surfaces feel warmer than white surfaces (q)	Premise
Test: These black surfaces are not in sunlight (not p)	Premise
<hr/>	
Conclusion after test: They feel warmer than white surfaces (q)	Inference that falsifies hypothesis

When falsification occurs under truly controlled circumstances, the premises must change; first the test must be thoroughly investigated and repeated, and if the test is correct, then the hypothesis must necessarily change. If the black surfaces in the example feel warmer than the white surfaces, despite the fact that they are not in sunlight, then the hypothesis that this *only* occurs in sunlight must be wrong and needs to be changed. The process of change is usually achieved by further exploration (by other logical inference forms) and subsequently new HDMs to corroborate – not to verify.

One factor that should always be taken into consideration in relation to deductive inference making in experimental archaeology, is the so-called *Duhem's problem*. This problem was introduced by the philosopher and physicist Pierre Duhem (1861–1916) in his *The Aim and Structure of Physical Theory* (1954 [1914]). The problem concerns the deductive falsification mode, such as modus tollens, which Duhem claims is not feasible in actual experiments where the aim is to conclusively falsify a hypothesis – so-called crucial experiments. In his own words (*Ibid.*: 185): "The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us." Expressed differently: whether it is the main hypothesis, or an (earlier,) auxiliary hypothesis that has been falsified is not something that can be determined fully, and a hypothesis can therefore not be absolutely falsified. The reason is that auxiliary hypotheses have – in practice – always contributed to the formation of the main hypothesis, and we cannot realistically identify all of them in order to prove their falsification.

For example: if the black surfaces feel warmer than white surfaces even outside

of sunlight, we do not know if this occurs because the main hypothesis is wrong (that they should never feel warmer). The temperature difference could be due to other factors, such as if circumstances not covered by the main hypothesis have lead to the white surfaces feeling colder. Even if we *would* know that such circumstances had *not* lead to the white surfaces feeling colder in this specific case, we cannot be entirely sure that such incidences did not occur during an earlier stage of exploration of the subject.

Duhem's problem is considered the reason why it would be impracticable to falsify a hypothesis absolutely through testing, although falsification may ideally and logically be reached (Jor 1999: 108; Kourany 2010: 35; Weber 2009: 7). Because experimental archaeology is a practical discipline based on testing, Duhem's problem becomes highly relevant for the discussion of the experimental use of HDM.

Sometimes, experimental archaeologists describe experimental reasoning as inductive (Boëda 1994; Grimaldi 2014; Reich and Linder 2014: 68). Induction is a statistical prediction that is usually used either in hypothesis formation or as grounds for generalisation, such as in for instance the biological sciences, which rely heavily on observations.

Induction is a non-necessary inference with pure statistical value. It can for instance be a logical consideration of observed facts. An enumerative induction, often used as example, usually complies with the following formula (Vickers 2014):

<i>a1, a2, a3, a4...a1000</i> are <i>F</i> and also <i>G</i>	Observation
All <i>F</i> 's are <i>G</i>	Inference

As inductions are non-necessary, and, as shown through the above formula, the inference is not necessarily true. Therefore, the arguments are observations rather than premises. As an induction relies on individual occurrences, there are still many individual observations that have not been made, which is why induction is a conclusion in advance of the test – *a priori*. In the case of archaeology, this is fulfilled (Hastrup 2002: 49). Typically, we list all the

observations and make a generalisation thereof. For instance:

This male grave, and this male grave, and this male grave etc... from this period holds a battle axe	Observation
All male graves from this period hold battle axes	Inference

In this example, the hypothesis must be changed as soon as a male grave without a battle axe is found. As this can happen quite often, the inductive mode does not really function for sound predictions in archaeology, although this specific mode of inductive thinking occurs regularly in the archaeological discourse (e.g. Sarauw 2007; Marquebielle 2011). Until the mid 1900s, enumerative induction was understood as a logical procedure that generates general conclusions (law-like statements or prediction) from specific observations. This view is still mirrored in some research today (e.g. Hastrup 2002: 83; Rodrigues 2011: 131; Bertilsson 2004: 383), although it only relates to some types of induction. However, this is no longer considered to be the main characteristic of inductive reasoning. Today, it is clear that inductive reasoning is first and foremost characterised by its contingency on the premises, such as is found in weather forecasts, and its non-necessary conclusion (Vickers 2014).

There are several modes of induction, such as from general premises to particular conclusions or vice versa. A good induction can also lead from true premises to false conclusions, and the thought process can therefore be difficult to categorise (*Ibid.*). This is where the non-necessary form of inference becomes important; instead of necessary and explicative like a deduction, an induction is ampliative and broadens our knowledge. Inductive reasoning is habitual, and we do it often and automatically (Bertilsson 2004: 376). Nevertheless, the process is purely based on observation and cannot in itself be imaginative, and so perception or interpretation is not included in an inductive procedure (Vickers 2014). If a generalisation is inferred, it is today considered to only lead to estimations, as observations, one by one, can never be expected to cover all instances of the phenomenon (Gillies 1993: 8-11; Popper 2002: 4f).

Other forms of induction have different formulae; for instance, and contrary to the enumerative induction, when the observation has a specific, rather than general, conclusion (Vickers 2014):

$a1, a2, a3, a4...a1000$ are $F$ and also $G$	Observation
<hr/>	
The next $a$ will be $F$ and also $G$	Inference

This form highlights that the purpose of induction is to make a predictive inference. If this is achieved by experimentation, the experiment would formally be considered an observation. For example, when the inductive mode (IM) of reasoning is put together with an archaeological experiment, it can look much like this:

Experiment with $a$ flint axe and $B$ task induces $c$ use-wear	Observation
Archaeological $a$ flint axes have $c$ use-wear (experiment result)	Observation
<hr/>	
Archaeological $a$ flint axes with $c$ use-wear were used for $B$	Inference

The setup above can be similar to archaeological research procedures in the sense that archaeologists generally tend towards general conclusions about a moment in time, based on specific primary data; either finds distribution or experimental data. The non-necessary character of IM becomes apparent once one considers that  $c$  use-wear pattern can possibly also result from other tasks than what was tested, such as other that mimic the same use-mode but differs in actual use (e.g. Rots 2008: Fig. 7). Nevertheless, most archaeologists agree with the notion that we only produce *probable* conclusions at best, and so it is therefore likely that the principle of induction does not invoke the same controversy in archaeological discourses. However, it is important to note that an inductive process does not supply an explanation for *why* axes would have been used for a specific task, as it can only reiterate purely empirical conclusions, and so the probable prediction will be solely statistical (Vickers 2014). If there is an interpretation of the statistical data, which is usually the case in archaeology, the interpretation and its explanation are not connected to the inductive procedure.

Induction makes the prediction *before* any test has taken place, as the test will in actuality be either a) to actually observe all possible observations of the same kind or b) to observe something that opposes the prediction and therefore invalidates it (*Ibid.*). It is debatable if archaeologists are making predictions, as

there will simply never exist a way in which we can fill in the missing empirical data – they are lost in the past. The application of induction in experimental archaeology will be further discussed below.<sup>76</sup>

Both deductive and inductive reasoning, and their connection to paradigms, are key to the discussion of concepts in experimental archaeology. In the following, the disparity positivism//postmodernism, as the two paradigms currently expressed in experimental archaeology, will be presented in detail. As mentioned earlier, in archaeological thought the paradigms take the form of processual and post-processual theories, which together make up a host of theory sets. It is therefore on a fundamental level rather than in the form of archaeological theory that the two will be discussed. As a potential solution to irreconcilable epistemological problems and a resulting awkward duality in experimental archaeology, a third theory set will be explored. This third paradigm is called *philosophical pragmatism*, and it will be considered due to the allegation that pragmatism sidesteps the irreconcilable differences between positivist and postmodern epistemologies (Preucel and Bauer 2001: 93-94; Rorty 1999). As experimental archaeology works within and between currently incompatible ideals, this claim will be discussed for further use in archaeological experiments in particular. Furthermore, a third type of argument, *abduction*, was born out of this paradigm and will be presented as an alternative in the following. The purpose of this discussion of arguments and epistemology is to elucidate potential fundamental conceptual obstacles that prevent comprehensive research of the intangible in technology.

### **3.2. Positivism – what is it and how is it used?**

Positivism is the view of the world as a *positive*; an existing, present, world. With this view comes the notion that objective knowledge about the world is accessible (Loughlin 2008: 666-667). Generally speaking, positivism is *the* philosophical paradigm that defines the modern, natural sciences (Outram 2008: 1). Implicitly, this becomes clear when one scans indexes of theoretical/philosophy of science literature from the natural sciences, written well into the

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76. See section 3.4.1.

postmodern era, where the word “positivism” is rarely mentioned;<sup>77</sup> it seems that the paradigm and the viability of the methodology are mostly considered a given (e.g. Kincheloe and Tobin 2009: 522). Perhaps therefore, and possibly because positivism generally discredits meta-theories (Bryant 1985: 3), it is hard to find a definition of the paradigm originating in the natural sciences themselves. In the humanities and social sciences, on the other hand, the definitions are plentiful. For now, the one offered by sociologist Christopher Bryant will be suitable:

*Indeed positivism in philosophy has come to be associated with epistemologies which make experience the foundation of all knowledge, and also with their complementary ontologies which propose a division between objects which are accessible to observation (about which knowledge is therefore possible) and objects which are not (and about which there can therefore be no knowledge); and positivism in sociology has come to be associated with the very idea of a social science and the quest to make sociology scientific.*

*Bryant, 1985:1*

Ever since the ideas of Francis Bacon (1561-1626) and René Descartes (1596-1650) (see Descartes 1956: 111), the absence of prejudice and presupposition has been firmly established as a prerequisite for a rational scientific procedure (Knowles 2000: 23, 52). The principle of the uniformity of nature promoted by David Hume (1711-1776) made it theoretically possible to discover natural laws by inductive reasoning, together with Immanuel Kant's (1724-1804) notion of physical principles as directors of all phenomena in this world (Hume 2012 [1739]: 183; Knowles 2000: 106). What we now know as positivism was reputedly instituted by Auguste Comte (1798-1857), but with a rather different outlook than today. Comte's positivism transformed science into a philosophy<sup>78</sup> in which the sciences were structured and classified much like at present (Comte 1988). However, he also included political philosophy in his positivism, and sociology was created as a separate science; at the same time dependent on and a culmination of all the others.<sup>79</sup> Science was now considered to be sufficiently advanced to make statements about the social aspects of society (Bourdeau 2011).

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77. See for instance the indexes in Casti and Karlqvist (1996); Gillies (1993); Gross and Levitt (1998); Kincaid (1997).

78. Comte is also dubbed the father of philosophy of science (Bourdeau 2011).

79. Mathematics, Astronomy, Physics, Chemistry, and Biology. Comte is credited to be the originator of the concept of Sociology as a separate field of study (Bourdeau 2011).

Comte's positivism was transformed substantially for modern day use, a product of a branch of *neopositivism* known as *logical positivism* or *logical empiricism*, largely promoted by the politically socialist and internationalist Vienna Circle<sup>80</sup> at the turn of the century and the basis for some of the most foundational discoveries in the natural sciences of the 20th century that are still valid today (Bourdeau 2011; Friedman 1999: xi; Uebel 2012). The Circle rejected metaphysics based on the criterion of verification<sup>81</sup> of statements through cognitive significance (empiricism); and the distinction between analytic and synthetic statements – coinciding with induction or *a priori*<sup>82</sup> reasoning and deduction/*a posteriori*<sup>83</sup> reasoning (*Ibid.*: 23f; Uebel 2012). However, *a priori* statements of truth shifted from being intuitively accessible, such as in the philosophy of Kant, to be based on logical syntax derived from the 'exact sciences' themselves, thereby giving the sciences the role of shaping philosophy rather than vice versa. This was meant to be the way to neutral, objective knowledge (Carnap 1937; Friedman 1991). A few decades earlier, the notion of 'objectivity' as we know it today was discovered by physicist Arthur Worthington, who based on experiments discarded ideas of the uniformity of nature. His discovery had laid the final foundation for the key principle of honesty and neutrality in the empirical collection of data (Daston and Galison 2007: 13f). Around the same time, the principle of value-free research was also promoted by Max Weber (1864-1920) as a normative, ethical virtue for modern science to strive for (Kim 2012).

Popper, one of the leading philosophers of science in the 20th century, derived much of his philosophy from the Vienna Circle (Uebel 2012). What differed was his clear division between empiricism as a theory and empiricism as a method, and the rejection of inductive *a priori* axioms for the deductive procedure of inferring knowledge.<sup>84</sup> An axiom was by Popper seen as just another hypothesis

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80. Prominent participants include Rudolf Carnap, Moritz Schlick, Herbert Feigl and Otto Neurath. Karl Popper is often mentioned in relation to the Vienna Circle. However, Popper never saw himself as an actual associate of the circle, but did study under the influence of its discussions (Creath 2013).

81. Seemingly first formulated by Ludwig Wittgenstein who was in close discourse with the Vienna Circle (Hanfling 1981: 2)

82. Literally "beforehand" – to make a statement before the test or empirical basis/evidence.

83. Literally "after" – to make a statement after the test or empirical basis.

84. See section 3.1.

rather than a synthesis of logics derived from the sciences. Deduction after the principles of the HDM was the only way to objective knowledge generation. Even then it could not produce a truth indefinite, as any hypothesis bore the potential of falsification by a later HDM. In other words, every *a priori* axiom could potentially be discredited, as could every *a posteriori* statement (Popper 2002: 53, 55). This philosophy is the premise for most scientific activity today, although a logical consequence of Popper's epistemology is that it can be overthrown (*Ibid.*: 10). However, as will be discussed below, there are several problems with transferring Popper's theory to less demarcated knowledge systems.<sup>85</sup> Nevertheless, there was also a substantial influence from positivism in the social sciences, in which systematic and reductionist models for explanations were sought to analyse humans (Knowles 2000: 106). As brought forward in Chapter 2, this influence became foundational for the present methodology of experimental archaeology.

Neopositivist thought is particularly visible in the experiment structure that is applied throughout the sciences. Examples can be found in the writings of Barrow (1996; 2005), Gross and Levitt (1998); Hartle (1996) and Rosen (1996), who all discuss the limitations of science within the theoretical framework of positivist thought. In the natural sciences themselves, the notion of an overlying paradigm is generally not touched upon when results are presented (e.g. Michelot et al. 2012; Passarino et al. 2002), nor is this the case in archaeological science (e.g. Dickau 2010; Regert et al. 2006). Whole theories are still built on positivist epistemological philosophy, such as proof theory (see articles in Hendrick et al. 2000). In addition, positivist epistemology is also present in social science theories that are supposedly leaning away from it (Kincheloe and Tobin 2009; Manicas 2006).

Positivism and the advances of the natural sciences were catalysts and important influences on the *Processual Archaeology* that was introduced into American archaeology by Philip Phillips and Gordon Willey (1953; 1955) and promoted by Lewis Binford as a critique of the previous *Cultural Historical Archaeology* (Binford 1962: 218). Much of the systems theory presented as the

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85. See section 3.3.



new template for archaeological research was inspired by the positivistic model for natural scientific inquiry and the (predictable) uniformity of nature, visible in his statement “By such a method we may achieve our aim of expressing the laws of cultural process” (Binford 1965: 205). This was meant to attain a neutral entry point for looking into the past, while at the same time gaining a broader scope for the type of research now possible (Binford 1962: 224). In 1968, David Clarke published his book *Analytical Archaeology* in the United Kingdom in which he laid out a research model along the same lines. Clarke proposed a defined procedure for archaeological research that had been adapted from the geographical sciences, and, as Binford, he was working from the theoretical starting point of systems theory. Clarke also targeted the language archaeologists used at the time, and highlighted how terms are ambiguous – therefore the use of descriptive terms such as “typical” was instead to be substituted with the generalised results of tested hypotheses (Clarke 1977: 29-30, Fig. 2). This principle still applies in parts of experimental archaeology (e.g. Reynolds 1999: 157).

Processual archaeology was in line with the theoretical trends of philosophy of science at the time (see Johnson 2000: 63), and was well received (e.g. Chenhall 1971). The processual trend quickly developed a paradigmatic status, which has enjoyed a prolonged popularity, especially in North America (Carson 2005; Kintigh et al. 2014; Renfrew 1999: 15; Skibo 1992: 14; Watson 1991). As mentioned earlier, Reynolds’ writings in experimental archaeology (1998b; 1999) can also be classified under this research tradition due to his attendance to scientific research ideals and notion that this will lead to viable conclusions for the archaeological past. In sum, it seems positivism is firmly established in archaeological research, most specifically in the discourse that applies results from archaeological sciences, but also in experimental archaeology. However, positivist ideals have been rigorously criticised from several different angles, and this will be discussed below.

### **3.2.1. Positivist ideals and internal problems in the discourse**

Although positivism is the underlying paradigm for the natural sciences and is therefore rarely scrutinised within the sciences themselves, the philosophy of

science discourse has uncovered a number of problems that follow from the very structure of positivist thought. However well it is applied and however meticulously the research procedures are, there are some issues that cannot be easily surpassed.

Firstly, the notion of discovery and progress traditionally relies on the concept of 'truth' (Kourany 2010: 39). What 'truth' really is, is not as clear-cut as perhaps expected. For instance, Popper's falsification theory allows for *any* conclusion to be falsified, which means that at no point in time can a conclusion be stated as true. (Popper 2002: 10; Rescher 1999: 35). As the HDM is considered the best way to scientific discovery by most scientists (Knowles 2000: 72), that science is not moving towards definite truth is implicitly insinuated. Rather, the HDM leads to a validation that should not be considered true, but can be applied as such until it is falsified (Outram 2008: 1). The problem created by positivist ideals is that the epistemic virtue of objectivity, which presupposes something unprejudiced and unthinking in this world, is often associated with something true or 'the real' (Kourany 2010: 13, 39). Although 'objective' is generally not the same as 'true', the concepts create problematic debates (Daston and Galison 2007: 16f, 28, 377).

Numerous interpretations of 'objective' have brought forward the argument that this is not in fact an untainted state, but rather understood individually or contextually (Brumfiel 1996; Shanks and Tilley 1987; Shanks and Tilley 1992; Williams 2005). Examples include different linguistic interpretations of concepts (Goldberg and Rellihan 2008); different, ethical interpretations of actions (Hume 2012 [1739]: 408); and different perceptions of empirical experiences, such as visual impressions (Gillies 1993: 140f) and musical preference (Istók et al. 2013). These are statements that are hard to refute, and the principle of objectivity may be somewhat of a dogma that is more a virtue than an achievable state (Rorty 1999: xvii; Williams 2005). John Barrow has suggested that the creation of natural laws – supposedly objective – is coherent with a human desire for unity, which according to him may be an aesthetic feature (Barrow 2005: 65). If that is the case, 'objectivity' takes on a form of an *a priori* axiom, which is what Popper was trying to counter with his falsifiability approach (Popper 2002: 53-56).

Barrow spotlights another problem with generation of knowledge per positivist tradition: natural scientific descriptions of nature formed by the hypothetico-deductive procedure, such as natural laws, are general models of the world. However, in order for them to be general, they are mathematical in character and can be expressed by formulae. The question then arises whether they are in fact descriptive of the real world, which is not mathematical in character and of which they are first and foremost an analogy (Barrow 2005: 57; Casti 1996: 29; Rescher 1999: 34f). The result is that what on a day-to-day basis is considered true (the world) is instead flawed by its lack of resemblance to the formula (Rescher 1999: 36), which puts the entire notion of objectivity back on centre stage and begs the question of the nature of bias. John Casti's description of objectivity as "relatively free of investigator bias" (Casti 1996: 14), is perhaps better suited than the description of something wholly unprejudiced. Although researcher bias may generally be associated with the anti-positivist critique of positivism, this is one way in which a positivist discourse itself leads to the issue of non-neutrality.

Another form of researcher bias that may occur, is when findings are not corroborated. This will lead to a middle ground of neither justified nor falsified, and leaves scientific endeavour in limbo in terms of present-day positivist ideals as seen in the credibility issues of singular case studies (Flyvbjerg 2011). This is something positivist researchers should generally be aware of, and corroboration, in the form of repeated testing, is also fundamental to the existence of Popper's HDM and his conclusion that everything can be discredited.

However, to corroborate findings creates a logical breach for Popper and his total invalidation of induction. The corroboration of a deduction transpires through singular repetitions of HDMs identical to the original. Every inference of corroboration is actually a singular occurrence, an observation – and because it is a singular observation, each corroborative event forms part of an enumerative induction. Corroboration may therefore equate operating from an *a priori* axiom<sup>86</sup>

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86. For a more detailed description, see p. 77.

as the aim for the corroboration is to make a *prediction* of the validity of the conclusion – quite the contrary of what Popper wanted to get to grips with in the first place (Helfenbein and DeSalle 2005: 273). Following this line of argument, to believe that corroboration is the correct way forward may in this way also be described as researcher bias. This could potentially result in a non-validation on an epistemological level.

### **3.2.2. Positivist methodology and experimental archaeology**

The research ideals promoted by Reynolds are heavily influenced by the ideals of the positivist direction in archaeology. This is visible in his principle of the application of deductive, unbiased procedures, reliant on primary data, to produce reasoned conclusions, in other words the HDM. In addition, he makes an effort to remove terminology that he considers too deterministic (Reynolds 1999: 157). However, the discrepancy between an archaeological and a scientific experiment should be considered before Reynolds' theory is endorsed.

In science, the general consensus moves towards bias elimination. This is a result of the dominance of rationalist<sup>87</sup> thought through the positivistic paradigm (Knowles 2000: 53-54; Outram 2008). However, as shown in section 1.2., this thesis works from a hermeneutic perspective on technology, which is decidedly anti-positivist as it states that personal, interpretational bias cannot be eliminated. As it is our tool to make sense of the world it instead sees bias as a fundamental prerequisite for all results, as results would otherwise stay illegible to us. Even in state of the art physics experiments, performed under truly controlled experimental circumstances, hermeneutics will dictate that it is the researchers who in the end interpret what the results mean. Hermeneutic reasoning is foundational in the postmodern paradigm, which promotes situatedness and which was shown in Table 1 to also influence the experimental discourse. The list in section 2.3. contains elements of both paradigms, which will, from either side, be seen as a logical inconsistency. A result produced by the guidelines will then, on the most fundamental level, indicate that it is both

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87. Theoretical rationalism is a normative view for how we should make rational statements, and that these are better than other statements. To be rational is to perceive the world through explicit universal rules (often logical) that we have valid reasons to believe are true (Knowles 2000: 46f).

unbiased and cannot be unbiased (Shanks and Tilley 1992). This can be demonstrated by a deconstruction of the criterion that experiment operators must practice actualistically.

The background for the 'actualistic' criterion is potential disturbance of bias; a realisation that an unactualistically acting person can disturb the way the results turn out. This is for instance visible through Reynolds' statement that modern perceptions of time and labour are different to those of past operators (Reynolds 1999: 158-159). As an archaeological experiment habitually needs people to operate the technology or researched practice in order to make the experiment happen, someone often acts as a proxy for a past person. Therefore, someone that may represent the right (basic) skill level to do so is commonly preferred, so as to skew the results as little as possible.

The criterion of actualism creates few problems if researchers already consider bias as inherent in the process, as proponents of hermeneutics do. However, if Karl Popper's HDM is used as a logical baseline for an experiment, then the actualistic criterion causes a logical problem (also see Tichý 2005: 114f).<sup>88</sup>

A true deduction in a 'hard' science such as maths or physics, performed in experiment form, is considered logically valid. In these sciences, deduction can therefore be applied without modification, although Duhem's problem is always a relevant issue where deduction is concerned. In archaeological experiments, deduction is more difficult to fulfil, and can only be achieved under truly controlled lab experiments. However, to make the results applicable for archaeologists, the analogy from present to past makes the whole inference non-necessary. As this is not possible for a deduction, the archaeologically relevant conclusion is no longer deductively attained.

In uncontrolled experiments, which by far constitute the majority of archaeological experiments, it gets even more complicated. Consider for instance:

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88. Even if, as in many cases, only his falsification argument is cited, that argument decidedly rests on the premise of his logical procedure (Popper 2002).

If pots are more efficient to store foodstuff, than other vessels, they were used for storage (e.g. Schenck 2014)	Premise
Test: These [experimental] pots are less efficient for storing foodstuff	Premise
<hr/>	
The original pots were not used for food storage	Inference

Most archaeologists will understand that this inference is not *necessarily* true. There are many reasons for that. Firstly, the hypothesis may be wrong and people in the past may have used pots for foodstuff even if there were more rationally efficient storage media available. Why they would do either cannot be inferred from a deduction such as *modus tollens*, and we can also never validly judge whether or not it is a true statement.<sup>89</sup> Secondly, the experiment may *admittedly* be biased, most probably by the researcher or craftsperson. In a true HDM there will be a deduction, which results, as demonstrated above, must be true by its nature (Knowles 2000: 77). If that is not the case, it is not a necessary inference, and hence not a real HDM. The epistemological discrepancy between admitting bias in a procedure through the criterion of actualistic behaviour, and simultaneously working towards bias-freedom through the HDM, is fundamental.<sup>90</sup>

There are other complications to applying HDM in archaeological experiments. To draw a conclusion based on the HDM requires a law-like prediction as starting point, such as those found in mathematics and physics. However, archaeological reasoning would struggle to produce a law-like statement apart from statements in the present day: e.g. describing the find of a flint axe along the lines of "this is an piece of flint with x dimensions that was found in y place." Such statements can hardly be refuted, and are therefore law-like. However, such statements are not necessarily suitable for deductive testing through archeological experimentation. Furthermore, because archaeologists generally do not aim to produce such statements only, but instead statements about human behaviour, actions or motivations, applying deductive reasoning is problematic at the point of observation, which will invariably be an analogy. To

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89. This is directly oppositional of Popper's falsification theory which would entail that we could validly infer that it was not.

90. Apart from its invalidation by HDM according to Popper, induction does not immediately cause discrepant issues between paradigms. This is because it is not *necessarily* true.

observe the past application of the material remains by a human being is in reality impossible. Exemplified, this means that for instance the action of chopping wood with axes may be derive from the presence of axes now, outside of the past context of action. The experiment is therefore only an indirect observation at best, since it is possible that axes were in fact used in different manners, or that the artefacts labelled “axes” are sometimes misinterpreted.

For experimental purposes, if this analogical observation is to be tested, for instance by analysis of wear trace on axe edges, a modern reference analogy is created, most often by actualistic experiments. Usually, this takes the form of a replicated implement, structure or collection of wear patterns. The experiment is then performed in field mode with the modern replication or reconstruction, which in itself is an interpretive analogy (Pélégriin 1991: 61). *The entire test* is therefore an analogy rather than a premise. However, this setup would, by a strict application of the HDM, which is the presupposition in this thesis, in itself be a faulty experiment.<sup>91</sup> Field experiments would always lead to a biased, non-necessary **inference** instead of necessary inference as detailed above in **Section 3.1**. The necessary nature is precisely why deduction is so highly valued in the sciences, and so if the necessity is taken out of the HDM, the logical procedure loses its grounds as it can no longer be falsified (Knowles 2000; Popper 2002; Thornton 2013).

Although the HDM logic may be validly applied in controlled lab modes, it cannot lead to a *necessary* inference about the past, as the test happens in the present. The value of the HDM is therefore limited to the present. To be applicable for the *past*, an analogy has to be drawn and a modification such as “likely” must be included in the final statement. This puts inferences made per the HDM on the same logical level as any other analogy, be it ethnographical statement or inter-site interpretations. Popper himself discredited any form of probability that was not in the form of a probabilistic generalisation of deducibility (Popper 1958). As archaeological interpretation has to take probability into consideration, and since we have no means for logically predicting the deducibility of our interpretations, the *conclusion* is but an

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91. In terms of one false premise – the test itself (see p. 72f).

analogy and the HDM is not applicable for validation of archaeological conclusions concerning the past.

In sum; an archaeological experiment relies on analogies for observations/tests *and* conclusions, and therefore make non-necessary inferences. This significant problem of analogy for deductive reasoning is a prominent factor in why the hypothetico-deductive model cannot lead to statement about the past<sup>92</sup>, something that has been acknowledged by many experimental researchers (e.g. Jolie and McBrinn 2010; Mathieu and Meyer 2002; Narmo 2011; Rasmussen 2001; Richter 2005). If the hypothetico-deductive model is selected as the norm for archaeological experiments, the fulfilment of its strict methodology relies on so many analogies that the outcome can be said to be logically dubious at best. Applying HDM as the normative rule begs the question of the value of experiments as archaeological research methodology. However, by putting IM forward as the *de facto* idealised methodology for archaeological experiments, the reliance on analogy to *formally* attain the demands of the method is nullified. Nonetheless, the inductive inference will only be a numerical prediction of certain observations. As archaeologists have to transform a prediction into an interpretation of past cultural actions or ideas, archaeological reasoning and unmodified induction are two separate procedures as well. Neither induction nor deduction are therefore inference forms that describe statements about the past (and see Johnson 2000: 60f).

### **3.3. The postmodern anti-positivism**

Postmodernism is often thought of as a concept of negative relativism (Rundkvist 1997) and of logical fallibility (Gross and Levitt 1998: 73), and sometimes even as “gibberish” and “babbels” (Bruner 1994: 397). However, postmodernism is at present the most prominent paradigm in the social sciences. This means that it is considered the ‘normal science’ or “achievements that some scientific community acknowledges for a time as

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92. The archaeological sciences are considered included in the natural sciences, and can apply the hypothetico-deductive procedure in the same way it is applied in the overlying scientific discipline, such as chemistry.



supplying the foundation for its further practice”, to use the words of Kuhn (1996: 10). In a sense, to call it a paradigm is also wrong, as the only really collective feature to this multitude of theories is its ties to modernism/-ity, as a reaction and re-evaluation of the modern way of thinking (Aylesworth 2013). Modernity can be described as a distinct philosophical perspective, founded on Enlightenment ideals of progress, and manifested in positivism. Modernity has facilitated certain social behaviour (Thomas 2004: 2) and gains continuance in the antithesis of postmodernism (Friedman 2001: 496).

Where modernity has been the ideological foundation for big ideas such as democracy, capitalism, colonisation, nation states, industrialisation and urbanity (Giddens 1993; Thomas 2004: 2), postmodernism moves beyond the grand narratives into the non-narratives or small narratives (Friedman 2001). Jean-François Lyotard offers a simplified definition of postmodernism as an “incredulity toward metanarratives” and highlights the crisis of narratives (Lyotard 1984: xxii-xxiv). Where modernity has had a part in the colonised world, by bringing it under human control, postmodernity aims at the special, the unordered and the deconstructed, with a foundation corresponding to the hermeneutic paradigm in which no detached hypothesis deduction can take place (Aylesworth 2013; Friedman 2001; Giddens 1993: 289). This makes for a number of perspectives, that become evident in the abundance of archaeological theories such as contextual archaeology, phenomenology, gender archaeology, technological agency, actor-network theory, object biographies, structural Marxist archaeology, and more. Most of these play off a reaction to a modernist influence found in either the older, colonialist ideals of Cultural Historical Archaeology; and positivist theories such as functionalism, cultural ecology and middle range theory (Patterson 1990). This continuity is the reason why Anthony Giddens labels the subsequent, critical stage a “radicalized” modernity rather than a full postmodern paradigm (Giddens 1993).

Because postmodernist, archaeological theories are distinguished by their disunity in opposition of the unity of science, in other words the positivist paradigm, to give a summary such as was done with positivism above will be superfluous and incoherent. Instead, a discussion of some key concerns with elements of the discourse will be offered.

### 3.3.1. Post-processual interpretation and its internal problems

In the archaeology of today, as in general, epistemological theory, postmodernism is considered a paradigm that opposes modernity, including both cultural historical and scientific strands of the discipline. In archaeological argumentation, the postmodern takes the form of a focus on the pluralistic, individualistic and particular, and promotes multivocality of both past and present. In the past, pluralism is found in the view of individuals as active agents that (re)shape and (re)establish their own lives without being steered by an unyielding, overlying voice of reason. In the present, researchers are considered to (re)produce research within the same framework of hermeneutics (Fahlander 20012, Ravn 2011, Chilton 2014).

Several strands of archaeology are included in this anti-modernist paradigm, most notably post-processual archaeology, gender archaeology, postcolonial archaeology, and phenomenology (Blackmore 2011, Brück 2005, Fahlander 2012, Ravn 2011). Common to all of them is an emphasis on personhood, bodies, relations, sociality and non-conformity. Most also carry the notion of social and individual agency, and how agency drives the relationship between individuals and their surrounding materiality. In this regard, postmodern thought in archaeology has been focussed towards intangible facets of societies past and present to a much larger scale than archaeologies of modernity, amongst them positivist archaeology.

Postmodern thought in archaeology was largely introduced as *contextual* archaeology by Ian Hodder with his earliest works, most notably *Symbols in Action* (Hodder 1982). The earliest theories of this new discourse were concerned with the situatedness of people and material culture, and the critique of processual universalism, and ethnoarchaeological work was used to illustrate how a focus on contextuality was rightfully placed (*Ibid.*; Hodder 1986; Pader 1982; Pearson 1982). The view of an active human of the past who used material culture expressively filled a void in the processual view of the passive past humans, enslaved by their system and their ecology. Hodder has been cast as the iconic spearhead of the new paradigm, but the fact is that this

discourse was long overdue in archaeology, as the 1970s showed a spurt in poststructuralist development in the social sciences (Johnsen and Olsen 1992: 432), and a wide range of new theories of social situatedness saw their heyday outside of archaeology (e.g. Bourdieu 1977; Giddens 1979; Foucault and Bouchard 1980; Foucault 2002 [1970]).

Hodder's 1980's contextual archaeology, however, took a modernist, structuralist shape, with a focus on reading of material culture after a more or less linguistic structure with 'syntax' (see critiques in Barrett 1987; Johnsen and Olsen 1992). Still, the notion of contextuality was rapidly adapted to archaeology, **and the theory set was integrated in what was to become post-processual archaeology not long after** (Jennbert 1984; Pader 1982; Trigger 1984; Wylie 1985). **However**, the processual discourse was also gaining momentum, partly in response (Bamforth 1988; Keeley 1988; Testart et al. 1988). In addition, new debates on, amongst other topics, gender (Conkey and Spector 1984; Dommasnes 1978; Dommasnes 1982; Gero 1985), post-colonialism (Bahn 1984; Miller 1980; Schanche and Olsen 1983; Sinclair 1984) and structural marxism (Gándara et al. 1985; Paynter 1989) made their appearance, and were supplied by truly poststructuralist approaches (Shanks and Tilley 1987, 1992). The result was a multifaceted archaeological discourse that erupted onto new and abstracted topics of sociality, context, situatedness, power relations and interpretation, while methodological processualism kept producing hard data at base.

One of the key elements of the new archaeological discourse was the situation of interpretation. To be situated meant that any interpretation a past person made about their surroundings would be conditional upon unique, social premises. Nevertheless, early **archaeological postmodernism and** post-processualism – however close to social sciences and philosophy its authors were now becoming, and however difficult it was to reach the actual past meaning (e.g. Barrett 1987: 471-472) – seemed hesitant to discuss the logical consequence of a conditional situatedness for the researchers themselves (Johnsen and Olsen 1992; Shanks and Tilley 1987). Relativism was conceived as something unwanted, uncontrollable and in need of modification before it could be considered appropriate for archaeology (Fotiadis 1994; Thomas and

Tilley 1992: 108; Wylie 1992). As situatedness in reality was a product of the hermeneutic paradigm which bases all interpretation on subjective horizons,<sup>93</sup> such modification was a logical breach of the simultaneous discussion of contextuality. In this modified form, the application of a modified relativism contributed to "subordinating the contextuality of the archaeologist in achieving the meaning of an object" (Johnsen and Olsen 1992: 426) without any perceivable reason why this should be so (**but see Ravn 2011, Chilton 2014**). In other words; archaeological interpretations were not as situated as the past people subject to **the interpretation**.

However, the hermeneutic position in which the researcher is as biased by context as their study subject, eventually came to reach solid ground in theoretical post-processual archaeology (Barrett 2001; Bradley 1997; Graesch 2009; Shanks 2007), albeit somewhat later than in the aforementioned feminist (e.g. Dommasnes 1988), post-colonialist (e.g. Trigger 1984) and structural marxist (e.g. Spriggs 1984) archaeologies which were mainly concerned with just such researcher bias. Still, the positivist paradigm of objective and detached hypothesis testing and deduction is to this day still prevalent in the theoretical discourse (e.g. Walker and Schiffer 2014), possibly because it concretises archaeology and gives "a substantive knowledge of what happened in prehistory and tools [archaeologists] can use to acquire that knowledge" (Kelly 2011: 290). This may also be descriptive for the general mitigation of relativism/objectivity and why researcher bias for the most part has been left out of the discussion in the non-theoretical archaeological discourse (Dobres 2006; Fotiadis 1994).

That researcher bias has been introduced but not applied as a relative stance in archaeology, speaks against its reality as a research factor. The post-processual debate on relativism has therefore been somewhat tinged with abstracted flights of fancy, and a caricatural view of a few key researchers (Hamilakis et al. 1997: 164). Situatedness becomes problematic when a researcher is expected to apply their own situated bias to a research situation in which they deal with empirical facts and modernist methods that require

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93. See p. 22

detachment, such as systematic excavation, meticulous documentation, and what is considered honest reporting (Shanks 1992: 12f). In experimental archaeology, the problem is not only found in the research situation itself,<sup>94</sup> but is also related to the origins of the discipline, which got its firm establishment in the era of positivism. In archaeology in general, a detailed procedure on how to allow for the bias in the interpretation, and how to tackle credibility issues and consensual opinions is rarely presented, and if so, offered methodologies differ with archaeological perspective (Brück 2005) or fundamental theory applied (Spector 1993). The paradox between subjective research and tangible material culture seems hard to fully accept and integrate as grounds for research.

However, the epistemological dichotomy between theory and (research) methodology is not the only issue for the adaptation of post-processual archaeology. Another methodological call to post-processual practitioners is to provide an archaeological method to apply theories of the past in practice (Llobera 2012; Prescott 2012). Instead of one or a few epistemological operations or recommendations, the discourse spans from ethnoarchaeological transcendence (Cunningham 2003) to phenomenological fieldwalking (Brück 2005; Clarke and Renwick 2013) to experiential time-travel (Holtorf 2010b). All the while, archaeological sciences are mass-producing data at an increasing speed, which are interpreted and communicated alongside the plurality of post-processual approaches, often analysed through a more processual lens (Prescott 2012). Although contextuality is now an accepted argument for a large number of archaeologists, the discipline seems more epistemologically diverse than ever before.

Even if the archaeological discourse consists of a multiplicity of approaches, the archaeological field largely takes the same shape as it has done for most of its existence. Apart from a higher proportion of both young and female professionals and a much larger number of individuals (Aitchison 2014) archaeologists still excavate, catalogue, draw, typologise, date and muse over artefacts in museum stores. Thereafter, they form their interpretations based on this documentation and investigation. Beyond this basis, the hands-on method

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94. As presented in section 2.1. and section 2.3.

of archaeology has changed more in the direction of the natural sciences if at all, and in parts of the world, 'theory' has never been a part of archaeology as such (Gramsch 2011). The increase in the direction of natural sciences is likely the reason for the growth in neo-evolutionist theories (Prescott 2012: 8). Recently, the call for a unified theory to marry the masses of scientific data with our traditional interpretational outlook has been issued (Gramsch 2011; Kristiansen 2011), and the discourse is now seriously discussing post-post-processualism. The fact that this expression was launched as early as 1990 (Chippindale 1990) only serves to indicate the long-lasting multiplicity of approaches that moves in the archaeological world. In general, the only thing that is likely to reach any form of consensus amongst post-processually inclined archaeologists, is that consensus has stopped being the point. This leads a lot of archaeologists to ignore the entirety of the theoretical discourse in search for empirical facts about the past. It also seems to lead theoretical aficionados to not see the forest of modernity for all the postmodern trees of rejection. In fact, many post-modernists are rather perpetuating modernist concepts – not only through their theoretical dismissal, but also **through** the practical application of empirical data to their interpretation of the past.<sup>95</sup>

### 3.3.2. Post-processual archaeology and experiments

Nowadays, and as was discussed in Chapter 2, the field of experimental archaeology is as divided on the topic of science//interpretation as the rest of archaeology. Some stick to the hard science, and will probably not think twice about publishing their experiments in *Journal of Archaeological Science*, others present their research in more integrated journals such as the *EXARC Journal* and are happy to do so. Most experimental research is scattered across the global discourse, with a few gathering fora in the form of workshops, meetings or panels; a few publications;<sup>96</sup> and regular European conferences. Upon attending an experimental conference, one is usually presented with a comprehensive spread from reenactment to residue analysis, illustrative of the

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95. In fact, even through rhetoric strategies of promoting particularity, difference, and individualism, postmodernists promote ideals that are the driving principles behind Western capitalism — a modernist product (Fahlander 2012: 120).

96. Such as the *EXARC Journal*; *Ethnoarchaeology*; *Bulletin of Primitive Technology*, or *EXAR Bilanz*.

full experimental discourse.<sup>97</sup>

The conscious integration of post-processual elements has only lately become visible in the discourse of experimental archaeology. Centered in Northwestern Europe, topics such as time travel and experiential value of experiments – subjective interpretations by nature – have emerged on the scene (Cunningham et al. 2008; Hansen 2008; Holtorf 2010b; Narmo 2011; Nilsen 2011; Petersson 2003; Rasmussen 2011; Schenck 2009). At the same time, the academic debate about the definition of *experimental* vs *experiential* archaeology came and went,<sup>98</sup> exemplifying that the matter was important to at least parts of the participants, and that including experiential value was *not* considered in line with scientific thought. However, the post-processual integration of experiential and experimental has reached a seeming consensus, visible for instance in the call for papers of the 8th Experimental Conference in Oxford, where papers touching on topics situated *between* experimental and experiential were invited.<sup>99</sup> The debate is still going, but has recently been initiated from the opposite side: people involved with reenactment, frequently not archaeologists, are putting forward their contribution to the archaeological discourse (EXARC Dvoráková 2015).

The experiential/experimental discourse has resulted in an acceptance of the subjective experience as valuable also in actual archaeological experiments. This was stated by Rasmussen in her theoretical categorisation of experimental archaeology into controlled vs contextual experiments<sup>100</sup> (Rasmussen 2001; Rasmussen 2007a; Rasmussen 2007b) and has entered the field as a 'new' form of integral experiments (e.g. Christensen and Ryhl-Svendsen 2014;

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97. Recent examples include

- the *8th Experimental Archaeology Conference* in Oxford 2014:

<http://experimentalarchaeology.org.uk/2013/12/21/schedule-for-the-8th-experimental-archaeology-conference/> [accessed 2/12/14]

- the *IV Experimental Conference* in Burgos 2014:

[http://museoevolucionhumana.com/~museoevo/assets/docs/publicaciones/publicacion\\_86\\_es.pdf](http://museoevolucionhumana.com/~museoevo/assets/docs/publicaciones/publicacion_86_es.pdf) [accessed 2/12/14]

- the *EXAR conference* in Mayen, 2014:

<http://www.exar.org/voorbeeld-pagina/program-conference-2012/>

98. This debate was a repeated feature on the 3rd (Edinburgh 2008) and 4th (Aberdeen 2009) Experimental Conferences in the UK, where the author was present.

99. Conference programme available here: <http://exarc.net/event/professional/uk-experimental-archaeology-conference-2013> [Accessed 2/12/14]

100. See further elaboration on p. 58

Englert 2012b; Liedgren and Östlund 2011) where scientific equipment and methods are used to measure uncontrolled human activity. Although not a truly new feature, these experiments are now designed and presented in a format that is acceptable to international journal standards, and so have even entered the general archaeological and other discourses proper. Such experiments are different to those that measure purely mechanical, physical or chemical processes, such as smelting experiments (Juleff 1996), but also separate from those applying scientific equipment to measure conditions and results that are 'flawed' by human error but do not concern human action, such as ethnoarchaeological experiments may sometimes be designed (Gosselain 1992a; Smith 2001).

Another post-processual feature of the experimental discourse and a natural precursor to the experiential/experimental integration, has been a discussion of the terms of field experiments as a crucial perspective in experimental design (e.g. Harry 2010; Bamforth 2010; Jeske et al. 2010; Jolie and McBrinn 2010; Lubinski and Shaffer 2010). Instead of, as previously, not considering the separation field/lab explicitly in the discourse (other than as potential labels), and rather intermixing the approaches without explicit discussion (e.g. Reynolds 1999), the awareness of the experiential outlook of field experiments vs the controlled situation of lab experiments has recently increased (e.g. Whittaker and Kamp 2006; Elburg et al. 2015). This highlights that both formats have value in an experimental design, and should be considered in light of each other and the technology at hand (Outram 2008: 2-3). However, that does not entail a fundamental methodological change, as both types have been applied to experiments for a very long time, but is rather an indication of the way epistemology is now normalised as part of the discussion, as opposed to earlier takes on experimental design.

A further development of the experiential vs experimental archaeology debate is the now **legitimate** inclusion of educational motivations into the experimental discourse, which has long not been considered 'proper' experimentation. Even if it is not experimentation in its pure, scientifically defined form, educational concerns relating to heritage production is now integrated with the appeal of experimental archaeology to an audience, already demonstrated by the format



of *Sagnlandet/Land of Legends* (Lejre, DK), previously *Lejre Forsøgscenter/Historical-Archaeological Experimental Centre* since 1964 and *Butser Ancient Farm* (Hampshire, UK) from 1972. Other museums have now joined that format systematically, particularly maritime museums concerned with experimental boat and ship trials such as the *Viking Ship Museum* (Roskilde, DK). In addition, countless open-air museums around the world are setting up experiments on an ad-hoc basis. The debate on education is included in the EXARC Journal, experimental conferences and experimental archaeology volumes. It has become clear that the public is now explicitly important to experimental archaeology and one of the conditions of how we design experiments today (Crothers 2008; Schenck 2009). Experimental archaeology seems to be slowly evolving from processual via post-processual, on to post-post-processual archaeologies. Nevertheless, for the most part the discourse still requires a substantial focus on scientific methodology, but generally without an actual discussion of the real epistemological value of such in relation to a passed past, and without voicing if scientific methods such as the HDM can realistically be applied to archaeology.

### **3.3.3. The epistemological *status quo* in experimental archaeology**

The above discussion has displayed how much of both general and experimental archaeology is falling in between positivist and postmodern archaeologies. This duality leads to inconsistencies between theory and method, and results in vague and theoretically disputable interpretations. While processualists will be met with outcries of determinism – the making of statements without a consideration of context specific informations – post-processualists (and post-post-processualists) have no claim for one past. Neither do they have a given methodology, which may lead to frustration amongst many archaeologists who want resolution (Kristiansen 2014) or those who actively try to steer clear of theoretical issues.<sup>101</sup>

This clash translates to the dichotomy controlled vs contextual (Rasmussen 2007b: 16) in archaeological experiments, and the division between those who

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101. On an anecdotal note; the author has heard the statement "I don't do theory if I can avoid it" numerous times.

intend experiments to be scientific/controlled and those who intend them to be interpretive, educational or fluent in definition. The latter will critique the single-variable focus of the former, and the fact that it is too rigid for human representation. On an epistemological level, this builds on the postmodern notion of how science is delimited by its surroundings – for instance financing schemes and trends; previous discoveries in the field; political wants and needs, and simply personal comprehension, sensual perception and inspiration. Experimentalists that wish to include contextual aspects of archaeology, and who also consider the individual skill level of the participants an influence on the final outcome, tend to fall in this category (Rasmussen 2001; Rasmussen 2007a; Rasmussen 2007b). The other division has a tendency to trust natural sciences with providing the correct results, and rely on the ideal of control as a contextual approach may be considered too fluent or inconclusive, or severely flawed by human error (exemplified by Reynolds 1999). As such, postmodern contextualism is seen to fail in providing conclusive results; not only by the scientifically and scientistically inclined per se, but also by those who wish for a clear and coherent methodology to follow in order to get what they want out of an experiment.

A problem that arises from the distrustful view of contextual experiments is that a substantial amount of experimental archaeology takes on a quasi-scientific, scientistic colour, often seemingly unconsciously so. This is seldom critiqued, most likely because the discourse is too dispersed or only rarely concerned with theory. Examples are many, and can for instance be seen when an experiment concerned with refitting skill amongst archaeologists suggests a mathematical equation model, rather than human (inter)action and skill itself, to judge success in refitting studies (Laughlin and Kelly 2010). Other examples try to generate general fracture models for different fracture patterns at different impact speeds and angles, but do not use relevant substitute materials to do so (Iovita et al. 2014). However, both these studies, and numerous of similar experiments, have been published in *Journal of Archaeological Science*; which may be taken as an indicator of 'scientific' status within the archaeological field. Other non-scientific features that often appear in the scientistic experimental archaeology are: monofunctional consideration of objects, which may give a false notion of control, but may be interpretively deficient; and the non-corroboration of results

that prevails.<sup>102</sup> When studies such as these claim scientific status, it is based on fundamentally flawed quasi-scientific thinking that instead opens them for critique.

For a methodological analysis of how to apply experimental archaeology to tackle something immaterial, it is hard to see how to proceed within this conflict. In the end, it appears from conferences and publications that most experimental archaeologists simply sidestep this entire debate in order to communicate their results without lengthy philosophical discussion. As mentioned in chapter 2 where a large proportion of it is reviewed, the theoretical discourse of experimental archaeology is surmountable, and only a few sources attempt to provide an actual methodology for how to coherently move beyond the divide between positivist and postmodern (e.g. Crumlin-Pedersen 1995; Mathieu 2002a). These sources are not the ones that resound the most in general experimental literature; rather the ones that *are* cited most often are either authored before the current theoretical paradigm (Coles 1967; Coles 1973; Coles 1979) or do not provide a methodological solution to the problem at hand (Kelterborn 2005; Reynolds 1998b; Reynolds 1999). Even more so, hardly any sources attempt to provide a methodology for investigating the intangible realm of technology; the traditions, the skills and the relations (but there are exceptions, for instance Høgseth 2012). This leaves experimental archaeology in an unresolved state, between science and social science, and tends to culminate in defined results, related to object rather than human subject. The shift of object-related results into people-relevant interpretations often follow rather implicit, but deterministic routes. For example, it is common to construe the argument after statements related to functionalist approaches, such as 'ecological systems preconditioned people in the past to do this.' This type of determinism may be found in many experimental explorations of certain tasks that end with decisive conclusions, like those that generate social models (Clarkson et al. 2015). Another deterministic interpretation of experimental results appears where researchers fail to see that the procedure undertaken was only one of many technological opportunities. This can result in interpretations that imply that 'past people did the same procedure we did

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102. A discussion of this premise is found on page 85.

[because we are unable to list/do/consider more alternatives],' such as can be the conclusion after exploratory field experiments or ethnoarchaeological experimental work (e.g. Gur-Arieh et al. 2013). Many experiments are presented as *if* they are scientific in outlook, and therefore carry the underlying notion that they are producing a validated conclusion. However, even if a result is presented with scientific colour, it is not automatically intrinsically scientific, nor it is a validated conclusion necessarily valid for the past.

The determinism outlined above has a long tradition which has not changed radically with the advent of post-processualism, and as consequence, the majority of the results in the discourse seem to remain object- rather than subject-related. But, as archaeology has as proclaimed goal to study the subjects of the past, however much through their objects, this discrepancy should at least be investigated more closely. This especially relates to the disparity between scientific, and scientific, but not scientific, experiments that adhere to positivism without necessarily having familiarised themselves with its contents. Also the contextual approaches that are easy to discredit because they concern themselves with experience reports rather than experiments in the traditional sense need more scrutiny. As it stands, experimental archaeology is too diverse to claim one epistemology. This could be one of the factors that result in the problematic nature of exploring the technologically intangible, indicated in the analysis of 100 experiments.<sup>103</sup> In the following, the discussion will turn to a third paradigm and a potential way out of the predicament for experimental archaeology, both in relation to science, the general discourse of archaeology and intangible aspects of technologies.

### **3.4. The epistemological convergence on consequence in pragmatism**

In the archaeology of two paradigms, technology is still largely seen as something coherent and is first and foremost related to performing a practical procedure, as defined in section 1.2. Even on the postmodern side of the debate, the social *practice* aspect tends to fall behind, and rather contributes more as a nuance to than a motivation for technologies (but note Høgseth 2012;

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103. For results of the analysis, see Appendix A.2.

Liardet 2013). That is not to say that a technology is a procedure more than a social practice, but as such, it has proven hard to get to grips with, and is often severely influenced by generous analogies crossing tremendous time/space gaps that may seem imaginative at times (Haaland 2004; Hardy 2007). What seems to rarely be recognised is that the Western construction of "technology" as a concept, implying the practical production and products for functional purposes, is a purely categorical class. As analytical category, it is clear that technology is still treated as a material set of procedures by most archaeologists, where the *object* is the main focus rather than a by-product (Dobres 2000: 21).

Notwithstanding categories, the object is the *consequence* of technology, regardless of which form the technology takes. If a technology does not have a manifest outcome, it is unlikely to be included in the modern notion of technology to begin with. However, sometimes the object is not the intended consequence, and the technology is performed with the social practice as its goal. One example can be found in Scandinavia and multiple other cultures, where present-day knitting is a technology practiced for recreation, not for production. The products of knitting are readily available in shops, often at a cheaper price and perhaps also better quality. However, the value of 1) knitting itself as a personal hobby practice and 2) a home-made knitted product when given to someone, are often the main motivations behind taking up the needles. In Scandinavia, a beaming child presents their first "scarf" to a beaming parent who will cherish it, although the product may have none of the functionality of a scarf: prestige is often the consequence of a child's knitting today, as it may have been for various other technologies in the past and present (Hayden 2011). Nevertheless, this may not have been the case a few generations ago, when shops were limited, sheep abounded on the farm, and Scandinavian winters were just as harsh – and a scarf was expected to be more than a glorified piece of string.

The example demonstrates how social relations or practice can also be the consequence of a technology. This is acknowledged by many archaeologists, but less frequently studied than the manifest part of technology. Other intended, intangible consequences can be the enabling of a relation, such as reciprocal

gifts (Mauss 2002; Weiner 1992); procurement of relations or other goods via trade (Hansen 2013: 80); expression of group affinity (Gosselain 1992b); ceremonial activities (Weiner 1992); practice for learning purposes such as in apprenticeship (Apel 2008) and numerous other motives. As already indicated, a consequence does not have to be a chronological end-product of practice, but can represent a motivation for the practice itself. In fact, today's focus on research impact is a call for consequence of research practice in the form of social relevance<sup>104</sup> (e.g. Bertsimas et al. 2014; Brueton et al. 2014).

The consequence of technology is acknowledged by both archaeologists leaning towards the processual (e.g. Hayden 2011) and those that are more (post-)post-processual (e.g. González-Ruibal et al. 2011). Consequence can be archaeologically observed, as when Norwegian soapstone products show up at Viking Age settlements at the Faroe Islands (Hansen 2013). It can also be inferred, when objects that seem to have no obvious function but ritual are used as for instance grave-goods, even if they could be highly functional objects in other contexts, such as axes or shelters (e.g. Østmo 2007). Technological consequence is therefore an analytical concept that could provide useful information for archaeologists regardless of paradigmatic conviction. Consequence can be seen as the entire outcome of practice; from intention through behaviour to end-product; in other words the *why*, the *how* and the *what* of any technological practice. To observe or infer such intangible consequences of tangible technologies in practice is the aim of the experiments performed for this thesis.

In the remains of this chapter a third epistemological paradigm, philosophical pragmatism, will be presented as an alternative for positivism or postmodernism in archaeological research. As such, pragmatism presents a way to sidestep dichotomies that either lead to universals deconstruction, which is often too rigid or too flexible for archaeological purposes. As this is a debate in which

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104. See for instance research councils in the UK: <http://www.esrc.ac.uk/funding-and-guidance/impact-toolkit/what-how-and-why/what-is-research-impact.aspx> and <http://www.ahrc.ac.uk/What-We-Do/Strengthen-research-impact/Pages/Strengthen-Research-Impact.aspx>; in Ireland: <http://www.sfi.ie/funding/sfi-research-impact/what-is-meant-by-impact.html>; in

experimental archaeology is situated in the middle,<sup>105</sup> the argumentation for the application of pragmatism in the experimental discourse should be discussed further.

#### **3.4.1. Consequence and abduction**

Technological consequence can be empirically accessible to archaeologists, and in actuality we are already studying it, particularly in relation to human-made objects and structures. This is an important argument in philosophical pragmatism. The pragmatist maxim was from the beginning centred on consequence, which has remained its core (Hookway 2013; Rorty 1982), although neopragmatist thought today presents a multiplex of different strains.

Present day philosophical pragmatism can be shortly described as an antifoundationalist, fallibilistic, and pluralistic paradigm that promotes a call for social action rather than truth or absolute meaning (Baert 2005; Bauer 2012; Brown 2010; Thayer-Bacon 2003). Generally speaking, pragmatism views an agent as actively practicing and experiencing, rather than thinking. Instead of attempting to mind-read humans in the past, pragmatism allows us to investigate their empirically perceivable actions in the search for meaning (Blandhol 2005; Bogusz 2012; Brown 2010: 142; Hookway 2013; Kaag 2009; Preucel and Mrozowski 2010b: 28; Rorty 1999). Neopragmatism has been introduced as an epistemology for archaeology among certain researchers in North America (e.g. Aldenderfer 2012; Preucel and Bauer 2001; Reid and Whittlesey 1998), but, as will be demonstrated, is really nothing new when archaeological practice is taken under scrutiny.

It is the significance of consequence that makes pragmatism pragmatic (e.g. Saitta 2003). In legal theory, another discipline that for the most part tackles interpretation or transference of real-life situations (actions) into imagined situations (stylised or idealised legal descriptions of action), pragmatism is an established way of getting to grips with the abstraction of laws. Pragmatic legal theory has a substantial following, because pragmatism highlights the

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105. See section 3.3.3.

consequence: whether or not the interpretation "works" (Blandhol 2005: 369). Legal pragmatism postulates that if an interpretation works in the now, it does not matter what it was meant to mean. By approaching laws this way, legal consequence results from actual action, not abstracted laws. For instance: if a valid law from 1975 states "document," a pragmatic translation of 'document' is interpreted to also include electronic texts, even though these were not generally in use at the time when the law was written, and have only been considered 'documents' lately; such as e-mails.<sup>106</sup> A law pragmatically *works* if electronic documents are included since it would work less effectively and against its purpose if they were not, especially if the intention of the law was to include certain documents that today are largely electronically produced<sup>107</sup> (*Ibid.*: 366-367).

The outlook of "interpretations that work" could be useful for archaeological interpretation as well. For instance, if a question about the past is unresolvable, it does not work and should therefore not be pursued (Preucel and Bauer 2001). Examples would be "How did Bronze Age people think?" or "Was string-making in the Palaeolithic performed by men or women?" These are questions that we can ponder, but do not at present get us closer to the past; ultimately the purpose of archaeology for most archaeologists. If the intention is to consider for instance archaeological epistemology or philosophy, or if the evidence status changes substantially and these questions can subsequently be resolved, they are purposeful, and will therefore work. Hence, the inquiry has to match the discipline, the intended outcome and the notion of what works: the question of what the consequence will be must be asked. But how do archaeologists decide on what works and what does not?

Pragmatism considers discussion one of the key values of inquiry. The point of pragmatism is not to reach truth, but to discuss consequences of the inquiry. Inquiry is by Richard Rorty described as no more than "an attempt to serve

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106. This is a common way of interpreting laws in much of Europe.

107. See for instance the Norwegian *Lov om dokumentavgift nr 59, 12/12/1975* [The Document Tax Act]. This Act from 1975 has been repeatedly amended, lastly in 2014, but nowhere in the Act, nor its supplementary regulations, is "electronic document", "written" or "print-out" defined or determined crucial to its application for governmental tax claims. This is because electronic documents are automatically included in its interpretation in order for the law to have any effect.



transitory purposes and solve transitory problems" (Rorty 1999: xxii), and a dichotomy between true and false is therefore seen as a notional separation that bears no meaning in real life (Bogusz 2012; Rorty 1982: 1; Samuels 2000). In short; Since to know if something is *always* true or not rarely inflicts upon a human day-to-day life, but if something works does. For example: whether or not the sky must be blue *because...* or that photosynthesis must create green colours *because...* is not important to a painter's rendering of nature sceneries. What is important is how the colours are perceived by the painter at the time of painting. In the same sense; to know if Darwin's principles of evolution are in fact true rarely serves the individual beyond transitory situations such as discussions, or beyond the fact that it is the premise for modern medicine, so that when a person is ill, they can go to the doctor and get medications that work. When the patient leaves the doctor's office it does not matter whether evolution is *true* or not, only that it works for their momentary need. The focus remains on the consequence (Samuels 2000). However, another consequence of Darwin's evolution principle is the existence of a large part of the academic sector, and it creates jobs for doctors, biologists and probably archaeologists, too. In this regard, whether or not evolution is considered true or not, is irrelevant, because it is meaningful and it works.

This irrelevance of true/false is because pragmatism sees justification of concepts ("truth") as an ongoing consideration and balancing of consequential aspects rather than something that corresponds to 'always', which would be pragmatically difficult to define beyond the length of a human life (Baert 2005: 147; Rorty 1999; Williams 2003). As discussed by Susan Haack; truth is instead seen as reliant on social context:

*[S]care-quotes "truth," as distinct from truth, is what is taken to be truth; and scare-quotes "truths," as distinct from truths, are claims, propositions, or beliefs, which are taken to be truths – many of which are not really truths at all. We humans, after all, are thoroughly fallible creatures. Even with the best will in the world, finding out the truth can be hard work; and we are often willing, even eager, to take pains to avoid discovering, or to cover up, unpalatable truths."*

*Haack 2003: 18*

Because the consequence of inquiry is not seen as truth, but the contextual truth-creation of the acting agent (e.g. Bauer 2012; Aldenderfer 2012; Rorty

1999), the standardised, pragmatic logical operation is not centred on the objective test of true or false (HDM) or statistical predictions of truth (IM), but instead on what the first official pragmatist, Charles S. Peirce, coined as abduction: a non-necessary inference mode that explains an empirical phenomenon through a consideration of its consequence; the likelihood of the interpretation (Bertilsson 2004: 376-377; Douven 2011; Samuels 2000: 215). Abduction is now considered one of the three types of logical inference, together with deduction and induction, and it can be illustrated with a logical formula such as

If $p$ , then $q$ is likely	Premise
$p$	Premise
$q$	Inference

However, it can also simply be explained as a goodness-based reason for belief (Reisner 2008: 19), or philosophical considerations of concrete rather than abstracted societal conditions (Hastrup 2002: 196). All definitions entail the same: the inference of abduction is that something is more likely, or *better*, than other inferences, considering a concrete piece of information. This is why abductive reasoning is also called *inference to the best explanation* (Douven 2011). An example, following the logical structure above, would be:

If a Neolithic axe is in a hoard, it is likely to be a ritual deposition	Premise
A hoard is a systematic collection ("deposit") of 2+ objects	Premise
This Neolithic axe is found in a hoard	Premise
This Neolithic axe is likely to be a ritual deposition	Inference

It is important to note that, although the abductive method (AM) is similar to IM in that it is a non-necessary inference, it differs from the purely empirical and statistical IM in the inclusion of explanatory considerations, including perception or interpretation. Whereas IM will only state a conclusion based on empirical facts, AM will attempt to explain those facts (*Ibid.*; Rodrigues 2011: 132) As Thora Bertilsson (2004: 377) has stated, abduction adds perceiving something

wilfully, and thereby transforms seeing to a socially meaningful act. Through abductive inference, we create a meaning others can acknowledge. This can be demonstrated through the example above, where the empirical observation of an axe in a 'hoard' (a present-day category) does not equal that it was in fact put down as a ritual deposition. The axe could have been lost in a pit with other artefacts, or it could have been buried by a dog hiding its treasure. However, after a consideration of the premises (spatial context, cultural context, social theories, analogies, field-related bias etc.), archaeologists find it more likely that the axe, when found together with other artefacts in what we call a hoard, was ritualised. That the axe is part of a ritual deposition is therefore the inference that gives the best meaning or explanation, which is likely to be the best we can do in terms of finding the 'truth', as long as we do not have time machines. Certain authors have critiqued the potential to include an endless chain of arguments (Chippindale and Taçon 1998: 92). Nevertheless, it should not be claimed with any confidence that archaeologists can reason without such a connected strain of arguments, as most of what we argue today must be connected to previous discourse to be accepted by others.<sup>108</sup> It is also important to remember that arguments rarely form a single chain of un-evaluated argumentation. Rather the connection between each argument most often carries an evaluation (a formation of inference) within. For instance, the abductive example above is in reality a very long dialogue between researchers what constitutes the premises for inferences about 'Neolithic', 'hoard', 'ritual,' and 'deposition' and countless other elements.

(Neo)pragmatists generally postulate that the consequences must be considered before deciding on the best inference; present in their call for social action and meaning (Brown 2010; Rorty 1999). The consequence of AM is *likelihood* (Douven 2011), and the inference is called a *belief* (James 1907: 42; Peirce 1878; Reisner 2008; Rorty 1999; Samuels 2000), not truth. There should also be some degree of benefit to believing in what the evidence suggests as the best explanation, which will form part of the consideration of why this explanation is better than other alternatives; also labeled 'explanatory success' (Douven 2011; Reisner 2008: 20). The benefit can be that it works, as in legal

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108. And see section on the Strong Programme on p. 113.

theory, or that people agree on its success, as in much of the academic world. Nevertheless, this does not entail that explanatory success cannot change. The reason for this is the situated notion of concepts such as 'a truth'. An example of the latter becomes visible when one considers what were regarded successful explanations in earlier archaeological eras. For instance, very often an axe would be, and still is, interpreted as a "male" artefact, with premises such as "if the axe is found in a grave [that yields no human remains], the grave is an expression of masculinity" (e.g. Sarauw 2007: Fig. 4). However, today, the archaeological scene is open to re-negotiate this inference from a feminist stance, which will state that this is in fact a deterministic interpretation. As such, the explanatory success is less definite and open for reconsideration, and the statement therefore requires further exploration to be accepted. The interpretation does no longer necessarily work.

The situated negotiation of 'best inference' necessitates plurality in interpretation, both in number of contributors and in ways of knowing. Normally, such multivocality can cause concerns in the form of an anything-goes-relativism (Bauer 2012; Rorty 1999: xviiiff), but in a best-inference explanation, the decision of which is at any given time the 'best' inference, is subject to power negotiation. This means that not anything goes; rather, something goes, and something **does** not (Bauer 2012). The power to decide is subject to competition, and as seen for instance in the theories of Bourdieu, positions of power decide on what equals 'good' and 'bad', and are subject to struggle for relevant capital (1984: 183f). This is also reminiscent of the sociological-theoretical *Strong Programme*, established by Barry Barnes and David Bloor amongst others (e.g. Barnes et al. 1996). Based on sociological study of academic environments, the programme establishes that academia does not objectively produce better knowledge than non-academic knowledge-producing systems. However, the academic field carries far more prestige, or symbolic capital in the words of Bourdieu (1977: 181), than any other knowledge producing agent, in the most powerful nations on Earth. So it becomes an immensely powerful institution: one that gets to decide which is a better inference of many, effectively making it the only legitimate power for knowledge

definition in numerous societies<sup>109</sup> (Brown 2010; Haugaard 2012; Knowles 2000: 31). One significant feature in this mechanism rests upon the active agent responding to other agents' behaviour in combination to the external world (Mazzotti 2012: 7). This factor unites the Strong Programme with the active agent and the empirical nature of philosophical pragmatism, while at the same time necessitating the situated multivocality caused by the generation of knowledge by individual agents. What knowledge is, is in other words a consequence of constant negotiation, evident in the academic acceptance criterion of publication, and situated in a structuration process between one agent and many in a highly democratic way.

Peirce considered abduction the only mode of reasoning for amplification of knowledge and therefore for forming hypotheses prior to HDM and other hypothesis-demanding operations (Rodrigues 2011: 132).<sup>110</sup> This follows from the interpretation that necessarily goes into the evaluation of whether the inference is 'best' or not. In contrast, the purely empirical IM can only state conclusions and will on its own not be hypothesis forming; rather it needs to be modified to the hypothesis state by drawing the best inference of what will happen after a deductive test. Induction is considered to be an empirical prediction on its own, just as a deductive conclusion is a logical inference (Rodrigues 2011: 132); appealing only to the purely non-explanatory operation (Douven 2011) and neither including explanatory considerations or success. Abduction was by Peirce considered to bridge deductive and inductive operations through connecting empirical and logical (Bertilsson 2004: 383; Samuels 2000). If successfully applied, abductive reasoning should give us explanatory insights we cannot achieve through other means. This creates a difference between IM and HDM on one side, and AM on the other, as the former do not allow for an inclusion of explanatory considerations or abduction, but AM does allow for an inclusion of both IM and HDM as premises. This is one

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109. And see Janet Kourany's (2010: 60-62) discussion of PETERS: the "Privileged, Exclusive, Talented, Elite, Royal Society" that largely constitutes academic environments and determined the prerequisites for 'knowledge.'

110. However, Rorty (1980: 720) has voiced that he considers Peirce, the originator of abduction, highly Kantian in the sense of promoting universalism. This will be of less relevance here, but it is important to note that abduction should not be viewed as an all-encompassing solution to a logical problem: This will be exactly opposite of the neopragmatist plurality discussed later.

way dichotomies such as true//false or fact//non-fact can be disregarded: by including observations made through IM or HDM as one of several premises in a logical operation, AM can dismiss foundational paradigmatic differences that may arise if one attempts to include explanatory considerations into positivist thought modes. In logical terms, this could be expressed as:

If $p$ , then $q > p > q$	Deduction	Premise
When $q$ happens, it is likely to be due to $x$	Explanatory	Premise
$x$ is likely to be due to $y$	Explanatory	Premise
<hr/>		
$y$ is likely	Explanatory/ Amplification	Inference

In archaeology, this can be exemplified by the first premise consisting of archaeometric findings such as an analysis that yields biomarkers ( $p$ ) for certain wooden residues ( $q$ ) on an axe edge, and the second being an interpretation of the residue as a result of the axe's contact with wood ( $x$ ). The third premise is the explanation of why there would be wood residue on the axe, for instance due to wood chopping ( $y$ ):

If $p$ biomarkers are present, then it is $q$ wood residue > $p$ is present > it must be $q$ wood residue	Deduction	Premise
When $q$ wood residue occurs, it is likely to be due to wood contact on the axe edge	Explanatory	Premise
Wood chopping is likely to lead to $q$ wood residue	Explanatory	Premise
<hr/>		
Wood chopping with the axe is the likely reason for $p$ biomarkers. The axe is likely to be a wood-chopping tool.	Explanatory/ Amplification	Inference

This particular abduction would infer that the wood residue findings point to a likely use of the axe as a wood chopping tool. This could in turn form a hypothesis that would be subject to further exploration through other corroboration in accordance with this model, for instance an archaeological

experiment.<sup>111</sup> It is important to note that each premise is here reliant on other abductions, which is often the case in archaeology, but less necessary in situations where the premises are exclusively empirically observed, such as

Lisa just snuck out of the field trailer	Empirical	Premise
She is wiping her mouth	Empirical	Premise
<hr/>		
It is likely she had a secret cup of coffee	Explanatory/ Amplification	Inference

The above examples should explain how many thought processes that we may perceive as inductive reasoning or deductive hypothesis testing (e.g. Dommasnes 1987) are instead abductive. The difference between IM and AM will always be found in the explanatory character of the inference. If the inference explains rather than describes the premises, it is therefore abductive in character – but more importantly, it is an inference form that we both can and do apply on a daily basis in archaeological research; or at any other time we add "likely" to one or more of the premises or the conclusion.<sup>112</sup>

### 3.4.2. Consequence and the external

As demonstrated above, whereas deduction and induction are logical methods for reasoning on scientific problems, due to the explanatory considerations, abduction is at the same time a logical operation and an everyday thought process. Although it is no longer exclusively tied to pragmatism, its principles stem from the fact that pragmatism from the beginning did not separate between truths and thought, and that a thinking being is therefore the necessary

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111. This kind of abduction that incorporates abduction, induction and deduction, is sometimes addressed as a retroduction (e.g. Rodrigues 2011: 136), but as this definition is not without alternatives (Bertilsson 2004: 385) it will not be further explored here.

112. This means that Dommasnes' (1987: 4-5) attempt to modify the HDM by adding "probably" to the conditional statement/first premise of her deductive formula, is actually an abduction because it is both non-necessary and including explanatory considerations (in her example: "If a tool has an edge, it has probably been used for cutting").

prerequisite for what one calls "truth" – and which in pragmatism has no name because it simply does not matter. Rather than true, something is seen as justifiable, and it is only so with reference to its consequences (Peirce 1998: 346; Rorty 1991; Rorty 1999). Regardless of true, as pragmatism focuses on the observation of the actions of others (Mazzotti 2012: 7), pragmatic concepts *do* however separate between human thought and that which is empirically perceivable to others; the external. Although pragmatic thought is principally against dichotomies, it promotes flexible categorisation as long as it is situational and serves a purpose (e.g. Brown 2010: 137). Its usefulness should come from its indispensability as a tool for inquiry, but it is important that categories not be given a finite content beyond its purpose (Rorty 1999: xix).<sup>113</sup> In this sense, pragmatism can separate between internal (for instance the thought process abduction) and external (the actions of others as experienced by a person). The external does not necessarily entail static categories such as "stones" or "plants", which will instead have a definition based on people's beliefs about them. As mentioned earlier, beliefs are seen as a habit of action, or habitus in the words of Bourdieu (1977: 78). Since the label "stone" would not exist or be considered a (true) category without our habitual action regarding stones – for instance talking about them – the label 'stone', too, will be a dynamic category that changes with our consensual perception of stones within our field: a consequence of our interaction (Baert 2005: 155-156; Rorty 1999).<sup>114</sup> As an example we can move back to the flint axe. Is it a stone or an axe? And what about quartz fragments found at an archaeological site – are they geofacts or artefacts (e.g. Driscoll 2011)? An example used by Peirce was that there is no difference between soft and hard surfaces unless they are put to the test, and so categories only become categories under certain circumstances (Peirce 1878). For some purposes, a flint axe can be a stone, for other purposes an axe, and for other again, a hafted implement or a prestige symbol. The axe can be perceived differently in the archaeological and the geological field. In fact, according to William James, one of the founding fathers of pragmatic thought, static categories (established as truth) is nothing but a "strong temperamental vision" (James 1907: Lecture I). Another founding father, John Dewey, later

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113. This means the all categorisations applicable for this thesis, should be considered in light of its potential benefits, before it is used elsewhere.

114. And see Rorty's example about the categorisation of giraffes (Rorty 1999: xxvi)



introduced the notion of 'event ontology': that the world is situationally known through active manipulation by a person. The world-knowing does not occur in our minds, but in situations – for instance going to the doctor and thereby relying upon Darwin. An inquiry will arise due to incidents in which their consideration is necessary or useful, not because we seek truth in itself, separate from our situational need (and see Baert 2005: 154; Rorty 1999: xxv). This manipulation can be seen as an adaptive, human response to circumstances, rather than an intellectual achievement (Brown 2010: 141; Dewey 1938: 67; Preucel and Mrozowski 2010b: 29). External stasis or flux is irrelevant to problem-solving or inquiry, as it is our manipulation of and active engagement with such that leads to the solution. The problem is solved pragmatically. This feeds back into the discussion on technological practice, which can take a multitude of forms depending on the required purpose, even within one object and the actions relating to it. Someone can use an axe to cut trees, all the while demonstrating that their axe is fancier than all the other tools. At a later moment, it can be used to furnish a grave as a ritualistic symbol of afterlife. A function should therefore be considered a consequence of sociality, which due to this exact factor is subject to change.

The pragmatist concept of external must necessarily also consider one agent's relation to other agents. And it is precisely the other agents that help decide which inference is 'best'. From early on, the communal acceptance of an inference was the defining hallmark of pragmatic abduction, making a viewpoint democratic (Hookway 2013; Peirce 1878; Preucel and Mrozowski 2010b; Saitta 2007). Also, to accept a viewpoint of another person as a justifiable inference, abduction has to be ubiquitous and indeed is seen to be so (Douven 2011). In this way, consensus can be reached on what is "sensible," according to Peirce (1878), and "works" as an inference according to Robert W. Preucel and Stephen A. Mrozowski (2010b: 30). The transfer of meaning happens through experiencing, which was from the beginnings of pragmatism seen to hold qualitative aspects, and include purpose, outcome and memory rather than just empirical input alone (Kaag 2009: 64).

Such intersubjective content of meaning is easily transferred to reality: as agents, we generally accept others' viewpoint in conversation if we can

experience (relate to) their abduction, and agree to its purpose and consequence. We therefore coordinate our behaviour to each other, and if we do not we may not understand (Rorty 1999: xxiv). As tools for understanding, we use consensus-driven standards, such as language, concepts, benchmarks and other descriptive and normative entities. The existence of standard is in actual fact a *social* consequence. In academia, consensual ideals rule what gets published, what gets disseminated at conferences, and what gets taught in education. In the judicial sphere, democracy decides what the laws will be, and legal practice is decided by people in the legal field. In most organisations, programmes or principles are decided in plenary, not individually (Baert 2005: 156). Nevertheless, standards can be and are manipulated through power struggles or other social behaviour, as there is no intrinsic quality to 'agreement' that requires the use of a specific method to reach it. Instead, methods are defined by what we wish to achieve; also the background for standards (Baert and Turner 2004: 271-272).

The desire for a democratic acceptance of pragmatic inference has several repercussions: firstly, an inference has to be identifiable for others to accept – in line with the foundational principle of disregarding hairsplitting irrelevance (James 1907: Lecture VI; Peirce 1878). This perhaps appears undemocratic, as a democracy is formed on the principle of individual freedom, and in Western society we practice that one may think what one wants, as instituted in various conventions of human rights. Nevertheless, if a person thinks that s/he were abducted by aliens, or that blue is in fact not a colour but a hat, these are unlikely to be justifiable opinions because few people will understand and agree; rendering the statements undemocratic – and therefore not generally applied. That means, one may think it as much as one pleases, but regarding communal institutions such as ethics, logics, science, and religion one is unlikely to get the argument accepted, without imparting something acceptable to the wider community or field (Brown 2010; Hookway 2013; Peirce 1878; Rorty 1984). The dismissal of irrelevant, as defined by a community, leads to a practice where every individual outlook is subject to a recursive consideration/revision by the democracy, of which the individual is an equal part. An example is found in the principle on academic freedom: freedom of thought but not entirely, as research usually needs funding, affiliation and research partners,

and practice norms are therefore usually followed (revision). However, the entire ideal of academia is from the beginning reliant on a free flow of ideas (consideration). In this manner, consensus decides what we research, how we research it and if and how we disseminate results, and these standards are subject to power negotiations, much in line with the Strong Programme mentioned earlier (Baert 2005: 148; Niklasson 2013; Saitta 2003: 15).

Democratic, consensual knowledge-procurement and social consequence together necessitate an openness for multifarious modes of thought in order for there to be something to agree on, and in line with regular, democratic principles of freedom (Biesta 2010). As research practice goes, that means that although consensus rules what gets accepted as best inference, there is still room for individuality within and outside of those borders – indeed this is how academia works; through feeding individual input into the communal structure. As mentioned, a democratic research practice consists of a set of methodological guidelines, but importantly, they are open to modification upon new impulses. Within the guidelines/benchmarks/concepts/terminology, a free flow of ideas that comply with the standards of the time are accepted, and researchers are taught to analyse whether it is a structurally bad piece or a 'mere' disagreeable thought. In social sciences, the ability to introduce new perspectives on social circumstances, while breaking with previous notions, is considered a strength (Baert 2005: 148-151), and exactly such dissent is key to the interpretive practice in archaeology (Bauer 2012: 193). Regardless, whether or not a piece is methodologically unacceptable or notionally unpleasant is widely debated (Eren et al. 2014; O'Brien et al. 2014 and more; Stanford and Bradley 2012).

On an epistemological level, multiplicity serves two purposes: firstly, different ways to a goal can corroborate conclusions so that there is more chance of gaining acceptance for research findings. Secondly, and tied in with the first argument, corroboration increases an AM by multiple premises so that the conclusion becomes the best inference. In sum, a diversity of perspectives strengthens interpretations that cannot go beyond likelihood in any case.

### 3.4.3. Pragmatic archaeology

As mentioned, pragmatism does not promote specific methodologies as it rejects stable categorisation. Neopragmatist thought is currently turned towards multiplicity as practice norm, and focuses on how a variation of approaches can shed light on a problem from different viewpoints, and hence create new understandings of the research subject (Aldenderfer 2012; Baert 2005; Bauer 2012). For approaching technology, that would entail investigations into specific technological practices from various angles, from 'hard' archaeometric to 'soft' theoretical interpretation.

In archaeology, as we deal with a past that we cannot directly observe, we already work in much this way, for instance through including several levels of analysis in post-excavation analysis, such as spatial, geological, archaeometric, analogical, and osteological. A multitude of methods often work together to achieve different inferences on the same situation. We believe it works to add as many methods to the interpretation as possible, and there is widespread consensus about that (but see Jones 2002: chap 3; 2004). We have that consensus because we perceive increasing amounts of inference to provide increased understanding. At current, this is our habitual action in the archaeological field, witnessed for instance after an excavation. Through the norms we have set for such action, archaeologists have decided – by an understanding of what causes credibility – that this practice creates the best meaning. Thus we create social, not individual, meaning as a consequence of our actions; archaeological practice is meaningful to us, and also, we agree, to many others. At the same time, we have not unanimously decided *which* archaeological methods are better than others, and so we are open for a host of approaches, originating both within and outside of our field, as long as they follow our practice norms. Archaeology can at current be said to function via the same principles as democratic rule, and as in democracies it is not exempt of power negotiations between authorities in the field. However, once a collective of archaeologists decide to not comply with the authorities because it no longer serves the right transitory purposes, they can break the ties and create new ways to understanding, nonetheless based on a consensus between them

(Baert 2005: 149; Rorty 1999: xxii). For archaeology, this is visible through the paradigm shifts in the 1960's and 1980's.

In short, today archaeology largely relies on consensus and diversity to create meaning. This indicates how our knowledge-generation is both democratic and meaningful to a group of people in a field. Hence, archaeological research is a social consequence of a communal wish between archaeologists and others alike to answer questions about the past. This constitutes social meaning as warranted by neopragmatist thought, and Patrick Baert considers much of present archaeological activity to follow a neopragmatist epistemology (Baert 2005: 160-163). However, what was not explored by Baert in his epistemological analysis of primarily post-processual approaches, was the simultaneous existence of positivist thought and practice, and archaeological methodology in general. As has been demonstrated in section 3.4.1., abduction can unite positivist and postmodern thought as AM premises, which is in reality often done in archaeological research, for instance when scientific results are fed into a contextual interpretation (Gosselain and Livingstone-Smith 1995; Jones 2002: chap 6).

Although certain authors have discussed pragmatist archaeology for a while, the discussion has changed from being highly centred on empirical observation, seemingly very much adapted to processual archaeology (e.g. Yorston et al. 1987) to the present, neopragmatist thought outlined above. The pragmatic paradigm is slowly gaining a proper foothold, and archaeological methodology specifically is considered by Mark Aldenderfer (2012) in his multifaceted view of archaeological inference. In tune with the request for diverse archaeological research in our field, Aldenderfer promotes that archaeologists should apply several different methods to come to grips with an archaeological problem. Alexander Bauer highlights that the advocacy of plurality entered the archaeological stage with post-processualism, and that plurality is necessary for innovation and creativity in our continuous interpretations (Bauer 2012). A similar view is explored by J. Jefferson Reid and Stephanie Whittlesey (1998), who in addition also add an element of common sense and the ease in which pragmatic principles can be applied in archaeological research. According to Reid and Whittlesey, openness and anti-foundationalism are key elements for

any archaeological interpretation, and elements of pragmatism have long been present in archaeology (*Ibid.*: 276-277).

Multiplicity is important to Preucel and Mrozowski, presented through the theoretical and methodological diversity found in their edited volume on pragmatic archaeology (Preucel and Mrozowski 2010a). This particular volume is largely focussed on the humanist aspect and social, "real-world" consequence of archaeology (Preucel and Mrozowski 2010b: 24), and especially engages with situatedness of the past in the present. The editors underline that pragmatism is always in motion and open-ended. The obligation to cultural pluralism is mirrored in Dean Saitta's works on pragmatic epistemology in archaeology (2003: 12; 2007). Saitta focuses on intersubjective truths and the experimentation with method and theory in order to arrive at yet other ideas. Thus, theories are tested against other ideas – not by comparison for the purposes of falsification, but by merging them with ideas we already have. Such weaving of ideas prescribes a "measured relativism" whereby evaluation is coupled with cultural pluralism, which, according to Saitta, leads to an expanded community between cultures (Saitta 2003: 12). The weaving of ideas happens when an archaeologist builds an archaeological interpretation, initially derived from the discourse standards, on their own experiences – both from within and outside of the archaeological field.

Regardless of abstracted thought-generating procedures, archaeological pragmatism is still concrete in outlook. The idea of a living rather than knowing agent is key (*Ibid.*), and mirrored in for instance Aldenderfer's multiplicity of methods for approaching religious expression (2012); a practice that at heart is concerned with empirical observations and merged with theories of practice. He states this clearly when he argues that "our obsessive focus upon ritual as a category of analysis has led us to forget that what we are really looking at is *religion in action* in some long-dead, ancient society" (*Ibid.*: 24).<sup>115</sup> As archaeology is in itself an empirical discipline that refers back to material culture as source of information, this should not be a surprise. Archaeology would not be considered archaeology if the object of our interpretation was not material,

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115. Emphasis added.

however much we want to say something about the (now non-existent) humans and their thought-processes. This unique body of knowledge on material culture is not replicated in any other discipline, and so archaeologists are in an unequalled position to make statements about past people through their things – in many instances the only way to increase culture-specific knowledge. It is for this reason that the present project remains faithful to the empirical in archaeology and wishes to approach technology as a concept through object calibration. Additionally, there is a growing archaeological movement 'back' to the empirical after a long period of theoretical abstraction. **This is seen in both the steadily increasing significance of archaeological sciences, and in expressions in the theoretical debate** (Kristiansen 2014; Olsen 2012; Thomas 2012).

In addition to its empirical nature and multifaceted approach to the past, archaeological reasoning fits well with AM, as previously demonstrated through various exemplification. Archaeologists have long applied modifiers to their language and will rarely talk in decisive conclusions. Rather, we acknowledge our operation within a sliding scale of likelihood through the use of words like "could have been" instead of "was" and "indication" instead of "proof". We include a multitude of approaches as premises for our best inference practice, which is generally subject to consensus. As corroboration can hardly happen the regular natural scientific way – by deductive means (Popper 1958), we attempt to increase the likelihood of an inference through applying multiple methods as corroboration (Aldenderfer 2012). Lastly, we take the overlying ideal of social consequence and meaning forward every time we make an inference that reaches consensual acceptance and, not the least, which evokes public interest: through defining cultural heritages that bear meaning for people other than our individual selves, archaeology is a socially purposeful tool; as requested by neopragmatist archaeologists.

#### **3.4.4. Pragmatic experiments**

Experimental archaeology must naturally apply many of the features discussed for archaeological research, as experiments are but one part of the multiplicity of methodologies archaeologists apply. Even so, experimental archaeology is

diverse in itself, and collates research from a wide range of experiment modes such as controlled laboratory experiments (Chu et al. 2015), field experiments (Whittaker 2010), ethnoarchaeological experiments with living societies (Nilsen 2011), explorations into unknown concepts (Narmo 2011), experiments limited to craft (Høgseth 2012) or to material category (Heeb and Ottaway 2014), experiments with and without professionals (Bakas 2012), a mixture of several of the above (Deter-Wolf and Peres 2013) and many more. What experiments do share is an empirical nature and focus on practice. As experimental archaeologists, we focus less on abstraction of ideas, and more on manifest material culture. This may be one reason why the theoretical discourse about experiments is limited, and why experimentalists mainly stick to their actual practice, rather than focusing on abstractions that are hard to transcribe into the manual labour that usually goes in to experimental work. With a few exceptions, experimental archaeologists generally care about doing and executing rather than theoretical dichotomies. According to Anna Beck, this becomes apparent in the example of European experimental archaeology (Beck 2011: 168): archaeological sciences aside, much of European discourse is focussed post-processually or even post-post-processually. Although experimental problems are geared towards relevance in the general archaeological discourses, research is evaluated per positivist ideals, which are generally outdated in much of archaeology. In this way, an experiment question is not necessarily associated with the paradigmatic research ideals used in its subsequent evaluation (Beck 2011: 168).<sup>116</sup>

This last instance is a good example on how paradigms and epistemological differences are seen as more or less irrelevant in experimental archaeology, which was further displayed through the spread of experimental modes that happily coexisted without touching upon such issues at the recent 2015 EAC9 conference in Dublin.<sup>117</sup> In practice, the borders between mind and body, knowing and learning become blurred in an experiment, where planning, experience, practice, things and ideas merge to produce results. In other words, experimental archaeology is anti-dualist as described by Richard Rorty (1999)

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116. And see p. 98 on how the merging of experiential in practice

117. Conference programme available at <http://experimentalarchaeology.org.uk/2014/11/23/provisional-programme-announced-for-9th-experimental-archaeology-conference/> [accessed 21/1/2015].



and leaves categories flexible and open-ended. In order to execute an experiment as an answer to a research question, we typically acknowledge that there are things we do not understand about what we are doing. Most archaeologists also understand that either their own or the designated craftsperson's skill levels differentiate from the people whom they are trying to replicate, and so contextuality is frequently discussed in relation to skill and operation (e.g. Apel 2006; Coles 1973; Crumlin-Pedersen 1995).

Experimental archaeologists more or less have general consensus that their methodology works better than other approaches for approaching certain research questions, such as exploration of material culture empirically and in real-time (Artioli 2010; Coles 1979; Crumlin-Pedersen 1999; Daire et al. 2011: 47; Grünberg 2002; Gurova et al. 2014: 54; Marsh and Ferguson 2010: 1; Rasmussen 2011: 151). Experiments are therefore seen to provide the best inference to these questions. Experimental archaeology has also moved beyond the realm of strict research into the application as an educational or presentational tool (Drews 2012; Foulds 2013a; Rasmussen 2011), making it ever less dualist – although concerns regarding this consolidation have been voiced (Outram 2008: 3; Preysler et al. 2014: 92; Reich and Linder 2014: 74; Reynolds 1999: 156; Schmidt 2005a; Sørensen and O'Sullivan 2014).

As most archaeological research, archaeological experiments tend to conclude along the lines of "likely" rather than "definitely" (e.g. Gurova et al. 2014: 51). Although many experimenters see the HDM and/or IM as research ideals, however unattainable they are (Grimaldi 2014), in practice it is not clearly demarcated, and abductive procedures are more likely to dominate the epistemological trajectory of an experiment (e.g. Harry 2010: 14; Cura et al. 2014: 15). The theoretical discussion of normative, epistemological ideals are rarely followed by experimental case studies. According to Stefano Grimaldi, "[t]he contemporary presence of objective and subjective components in [...] scientific research should be seen as the 'perfect cocktail' as far as the validity and the success of the research is concerned" (Grimaldi 2014: 2). This statement is incoherent if considering the content of the respective paradigms discussed, and may perhaps therefore be taken to point towards an irrelevance of paradigmatic difference within the experimental procedure – especially when

it is put together with the rare discussion of paradigm in experimental archaeology.

Irrelevance of paradigmatic differences becomes further highlighted as it is claimed that chaîne opératoire approaches, together with practice theory, spans the gap between positivist and postmodern dichotomies, as practice itself is integrative and diffuse as an entity (Bogusz 2012: 33; Dobres 2010a: 106). As the execution of an experiment mainly relies on attempted practical replications of assumed technological or practical operations, it is clear that the applications of these cannot be wholly objective in themselves, and as mentioned, the situatedness of practice is widely acknowledged in experimental discourse through the well known actualistic-criterion.

To sum up; experimental archaeology is a diverse, multifaceted practice that acknowledges situatedness and disregards epistemological dichotomies. Experiments are considered the best inference for a wide array of research questions that tackle the practical implementation of past material culture. The consensual agreement on crucial aspects of experimental practice allows experimental archaeologists to accept and appreciate its application, as witnessed by the low level of disputes over epistemology and procedure at conferences and in publications reviewed for this study. On all these counts, experimental archaeology can be said to be pragmatic. However, its diversity creates a situation where it can be hard to define what this agreement consists of: what an experiment is, how it should be performed, and how it should be evaluated. Through the in-depth study of the experimental discourse detailed in Chapter 2, it has become apparent that the definition is not at present really called for. Nevertheless, for the purpose of this thesis it will be necessary to pursue a temporal definition that will be applied further. In Chapter 5, a methodology for working with pragmatic experiments through a transitory framework will be explored, pertinent to the evaluation of experiments as an access point to intangible technology in past societies.

## **4. Conceptual technology: theories, methods, practice.**

The previous chapters have discussed intangible components of experimental practice; the general normative ideals, and the more obscure backdrop of paradigms that nonetheless clearly defines experimental archaeology. The chapters displayed that experimental archaeologists use their practice to signal that they fulfil viable statements, and that they comply with the values that belong to them. In this manner, experimental archaeologists convert ideals into practice. For viewing the intangible backdrop of a past technology, manifested in tangible material culture, it makes sense to see practice in the same way; as an expression of demands, values and premises for a viable existence, defined by the contemporary society. In the following, exactly how to approach the intangible expression of practices will be discussed. As these are largely based on actions that will only have left some trace in the archaeological record, the normal and typical approach of archaeologists is to approach intangible aspects through theories of how such aspects become embedded into things.

### **4.1. Addressing intangible technological structures**

An introduction of the thesis problem has been given in Chapter 1. Yet, as the problem of addressing something that is doubly lost; both due to its non-physicality and past character, the motivation for investigating the recovery of just this kind of knowledge must be expanded upon. For some, to uncover something that is lost is reason alone for research. Nevertheless, to uncover intangible structures, norms and concepts can also build a profound understanding of a society that should be – and in relation to research on *living* communities, is – sought after. To better understand cultures is the fundamental cause of the humanistic and social scientific disciplines in which we operate. The motivation for the present investigation is therefore to study a methodology that may provide pieces of solid information about something non-physical.

It is the proclaimed intention of this study to focus on the potential of

experimental archaeology for finding the intangible in the physical remains<sup>118</sup> of cultures. Archaeology has long since established that such remains bear meaning, from information about subsistence, territory and dates to customs and style. The ways in which this information has been researched have differed with overlying paradigms, such as national romanticism, scientism and, lately, postmodernism, although experimental archaeology has been more consistent in its material, procedural approach. The next level for this thesis is to re-focus this conceptual part of the study on the artefacts and the people that relate to them. How we study the intangible, technological in practice will relate directly to how we approach information embedded in physical objects, and how it is that they hold both material and immaterial testimony. Over the following pages, it is this unification of the tangible and intangible in the thing that will be broadened upon.

#### **4.2. The physical manifestation of non-physical technology**

It must be stated from the beginning that there is hardly any physical object that will unlock complete information about an entire society. Neither is this the archaeological base method. Rather, today archaeologists will research pieces of information and thereby build a picture of a community through constant reference and cross-reference, much like described in section 3.4.4. (Aldenderfer 2012). However, in archaeology, the more archaeological primary reference there is, the more justification the picture will be seen to have; both in terms of discursial knowledge and actual evidence (Edgeworth 2012; Olsen 2010; Olsen 2012). This is also mirrored in the experimental discourse criteria in section 2.3. To promote the tangible in this manner means that catalogue searches, macroscopic and typological information are normal outsets for most archaeological research, but experimental archaeology is particularly privileged in its approach to the wider scope of things; not just how they look, but how they feel, how they function and how they exist in relation to people. To set up an experiment is to allow the things to truly come centre stage, and the existence of things in experiment dictates the entire surroundings of the things: how people behave with, move around, and perceive the thing.

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118. Including both cultural and biological remains.

Archaeological methods often work specifically with object-focussed methodologies as ideal, and this is certainly the case for experimental archaeology. We mean to say something about a past community through our practical focus on its things and structures, and we believe that it works to do it in this way. However, *what* the things may tell us is mainly conceptualised in the wider archaeological discourse. Nevertheless, often the archaeological way leads us to focus on things to the extent highlighted by Olivier Gosselain (Gosselain 2011: 212): to where we do not separate between object and practice. Sometimes, we will not be able to find the information we want by simply investigating the thing, and we need to see the object within a larger context. In the following, various conceptual approaches to things in such meaningful context will be outlined, with a special focus on those that are commonly used in archaeological studies of contextual technologies.

#### **4.2.1. Societal structure, social agency and relations**

A society makes its own things and constructs its own structures. However many individuals work individually, people usually live and work in groups that together define some or many facets of life. Although the theory set was born within a sociological discourse, such communal, social *structuration* is at current well established and pursued in archaeology (Barrett 1988; Hosoya 2014; Normark 2004; White and Beaudry 2009), through material expressions of general ideas that were reproduced by individuals' hands. Instead of giving the social whole pre-eminence over the individual, such as in scientific, functionalist views of cause and effect, or structuralist views of general meaning, the sociologist Anthony Giddens showed how the social whole is generated by individual agency, which are enabled and restrained by the social whole in a back-and-forth structuration dynamic (Giddens 1984: Chapter 1). An example can be found in monetary systems, which are devised by individual agency and reinforced by individual agency, but direct individual agency into certain forms of action such as exchanging goods for money. Such systems create boundaries within which a person must act to be a regular member of society. The system creates new fields within the larger unit, in the form of work (minting, trading), assets (being rich), beliefs (it is better to be rich than poor)

and other definable and indefinable consequences; all the while being upheld by the new habits and actions (or habits of actions) of people. The system *is* people, but yet it binds them. People can in reality live without it, but very often, doing so feels incomprehensible. Other typical examples are legal norms – nowadays often changed by individual agency through democratic rulings, but still vastly restraining; social morals, and ethnic tradition. Nowadays, a very visible structuration dynamic is the systematic (and restraining) use of IT. Structurated systems are everywhere where a public opinion has formed about how something 'must be' or 'is', and often both create opportunities for and put restraints on, individuals. This does not only occur on an abstracted/intangible level, but frequently also have tangible expressions, such as restraints of instincts such as bodily functions (Mickunas 2010) and sustenance (Fischer 2002), appropriate dress codes (Bergerbrant 2007), or dwellings patterns (Løvschal and Holst 2015). In this way it becomes visible that base of any norm, from aesthetic to cultic to hierarchical, there is social agency of individuals at play. If people really felt like breaking with the system, they could go in the bushes, eat dessert for breakfast, bare their breasts in public and live on someone else's land, but still, we have tendency to rarely behave in an opposing way to systems we find 'natural' or 'appropriate.' Through the theory of structuration, the reason for this is explained in the fact that we ourselves generated the systems we are part of.<sup>119</sup> In this way, habits of actions<sup>120</sup> that are a result of both individual agency and structurated systems are exhibited in material culture and its relating practice, for instance as conforming or deviant expressions of that particular time and place.

Most, if not all structurated systems are reliant on technology: coins and notes facilitate the money system, and computers, phones and ATMs the seemingly unrestrainable growth of IT. Archaeologically speaking, we can expect to see the expression of structuration in the form of repetitive technological objects that form patterns. We do already approach technology in this way; by seeing different artefact types as expression of style trends (Engevik 2008; Skibo et al. 1989a), symbolic behaviour such as expression of identity (Sarauw 2007) or

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119. This is opposed to the passivity of individuals that dominated generalising, processualist thought and was mentioned earlier (p. 92).

120. See p. 114f for a description of this pragmatist view of communication.

religion (Aldenderfer 2012), hierarchical diversification (Hayden 2011), political structures such as trade routes (Charlton et al. 2012; Stoner et al. 2014), and countless other expressions of overlying societal structures.

One type of expression archaeologists work with is aesthetic style. Style usually forms the basis for typologies, which again often coincide with chronological interpretations. Style is considered to express fashion at a given time, influenced by functionality, ritual, symbolic meaning and general taste (e.g. Skibo et al. 1989a: 388-390; Fredriksen 2006; Larsson 1990: 268). According to Bourdieu, (2014 [1984]: 140) taste is "the propensity and capacity to appropriate (materially or symbolically) a given class of classified, classifying practices"; often closely linked to social elite in position to set its conditions. That which equals good taste is therefore normatively fulfilling, and what equals bad taste is normatively poor. The taste criteria symbolise social standing – the better taste an individual has or displays, the more socially acceptable s/he is, and taste therefore functions as a social signifier. Taste is opined upon by individual agents, who will often have points of view relating to their social stance (Bourdieu 1984: 97). In this way, taste can be both an expression of, and a powerful enabler for, structuration processes.

Taste is clearly expressed in material culture through style. However, taste does not only concern aesthetic norms, but also practice and action. A technology performed by established standards, i.e. washing garments with soap, is considered more tasteful and places the practitioner higher on the social scale than a deviant practice, i.e. washing garments with urine, even if the end result in both cases is a clean garment. Taste can therefore directly affect what an individual will do. It is important to note that taste plays a role in technological practice and tradition, and that decisions about which is the right or wrong procedure can rely on taste as much as functionality issues or desired end results.

To act can also be in good taste, such as to seek out the correct things by the norm. In most social groups, individuals who cumulatively have or seek access to that which signifies 'good' are valued higher and positioned better than those who do not (*Ibid.*: 56f). In this way, norms and practice structure our world and

makes legible sense of it (Gosden 2001: 163). The collective want for certain signifiers of 'good' or 'right' can lead to demands for certain commodities and specialisations, such as professions. Furthermore, close links between production and consumption are established and creates a larger aesthetic scheme that lays boundaries on the individual agents (Bourdieu 1984: 230).

Taste, morals and practice norms vary within social units, and within sub-groups such as classes, age groups and professions, in which different norms bear distinctive meaning (*Ibid.*: 228).

According to James Mathieu (2002b), and in the prolongation of the discussion above, the cumulation of society in technological terms forms a system that is inseparable from its individual components: people, groups and objects. Within this system, everything is conditioned by the contextuality of the social situation, and so the only people to have real access to the system are its participants. Only participants can truly understand what their respective technologies entail and what it is (and is not). The system in Mathieu's terms coincides with Bourdieu's field.<sup>121</sup> The field functions as an autonomous body, and within it exist concepts that appear self-evident for the participants in the field (*doxa*) (Bourdieu 1977: 164). *Doxa* are often (or always) regulated by taste. Agents act and react inside the field through *habitus* – which Bourdieu defines as "the durably installed principle of regulated improvisations" (*Ibid.*: 78). The field contains individual agents, but the self-evident concepts steers their action. In other words; it is within the field that a constant structuration takes place, together with innovation introduced by individual agents (Fig. 3). Their role in their respective fields are determined by *doxa*. An example is archaeology, which can be considered a field where individual archaeologists agree on certain self-evident *doxa* such as 'old is valuable' and 'things can tell us something about humans.'

Fields may feel very real, although they are not tangible. The field directs our actions, so that **which** is conceived as right or wrong influences our *habitus*, even if *habitus* is considered individual. Within the notions of unity and self-

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121. As described on p. 22.



evidence, we act. For instance, within the field of archaeology, we excavate, typologise and date cultures. However, as the field has developed through recursive structuration – much like Thomas Kuhn dictates for academia (Kuhn 1996) – archaeology changes, and we now also perceive value not only because something is old, but because that something is created by humans. The taste of archaeology has changed, and the doxa will change with it.

Ultimately, the expression of each of these units' sense of 'good' is channelled down to the respective material culture and forms patterns. An archaeological example is the pattern of two different skirts among women in the Nordic Bronze Age, as encountered in oak coffin burials. A longer skirt consisting of woven fabric seems to have been worn by women with long hair coiffed in elaborate styles and wearing hair nets. A shorter skirt consisting of plied cords with tubular wrappings of bronze was apparently worn by women without hair nets and with short hair. The skirts are considered to be an aesthetic choice, even if they may also carry an unknown functionality. The norm in question was likely related to social status, and possibly to burial, age, hierarchy, and definition of finery (Bergerbrant 2007: 145-146). This particular aesthetic must have been linked to several technological practices; not only production, but also likely to household activities such as repair. While clothing is a classical expression of taste, aesthetic or normative expressions can also be found in choice of technique and gesture (Dobres 2000); in spatial patterning such as the organisation of dwelling structures (Knights 1994); or in the specific choice of form of the Sweet Track (UK) a Bronze Age plank trackway (Coles et al. 1973) instead of other known forms that serve the same purpose, such as the causeway at Tiltreidet (Norway).<sup>122</sup> In other words, taste as norm for practice can also determine technological features that are normally seen to have little to do with aesthetics.

Technological practice and tradition – in other words, acting by established standards of 'right', are delimited by structuration and the recursive movement between societal structure and individual agency as described at the beginning

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122. ID 176147 in State Antiquarian database over archaeological sites (Askeladden). <http://www.kulturminnesok.no/Lokaliteter/Moere-og-Romsdal/Neset/Veganlegg> [accessed 18/9/2014]

of this chapter. But the universe of what is good carries more than just details of right and wrong; it also carries affinity to culture, ethnicity or identity. Affinity, as mentioned and as is commonly observed in archaeological interpretation, can be communicated through aesthetic style, but also in traditions of practice (Bourdieu 1977: 161f). The aforementioned flint dagger technologies from Denmark (Apel 2008)<sup>123</sup> show that distinct technological traditions were connected to specific groups, and that the traditions transcend utilitarian function alone. Through such application of technologies group affinity may be assigned, although not determinately, as there are countless structurations at play in any given group. Furthermore, groups may share certain notions with some and certain with others. It is also important to note that, although these affinities may sometimes be visible, that is not to say that the group or individual itself paid conscious attention to it, as little as current Europeans pay attention to how they use a fork as a tool to eat. The structuration process of technological notions creates a web of relations between individuals, groups, societies, ethics and practice, and those connections form the basis of and situates any given technology in a very specific spatial and chronological context. According to the principles philosophical pragmatism, the acting agent should not be dichotomised into a separate group, but instead be seen as closely embedded in all these relations.<sup>124</sup>

The web of connections between taste, structuration, individual agents and expressions thereof, is in practice theory seen as social life; and for technology the inseparability of tangible and intangible aspects of technological practice (Dobres 2010a: 106); both intra-personal; amongst others skills, knowledge, dexterity, raw material, tools, and inter-personal such as place, people, environment and language. These relations are inside and outside of a group, and tie together entities of all sizes. It can take explicit, tangible forms, such as the shape of a garment, or more discreet forms, such as the use of a raw material – expressing for instance connections and difference. The web is open-ended and can cross time and spaces, as long as there are people taking part in it: for instance, by utilising a folk costume that was once a grandparent's; or by repeating regional recipes. This web can also be seen as entailing

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123. See p. 24.

124. Page 107ff.

antagonistic relations; for instance recognising recipes from somewhere else, or communication between opposing fighters in combat (Horn 2013). A more recent take on this connecting web is the surge in Actor-Network Theory that is now occurring in Archaeology; a theory that concerns relations **as going beyond between people, structures and things, and how these relations connect people (actors) with their surroundings (social, material, natural or other) in an open-ended entanglement (network)** (Fahlander 2012, Hillerdal 2015; Stockhammer 2012; Van Oyen 2015a). While actor-network theory could feasibly fit the purpose of this thesis, it is rarely represented in experimental archaeology, and will therefore not be further explored for use in a typified experiment. A pragmatic web must be considered to be without demarcations, and may go anywhere that connections can be established, in line with the open-ended, flexible categorisation and the inherent anti-dualism of the pragmatic paradigm.<sup>125</sup> However, the scope of the relational web is of less relevance here, and it should suffice to state for the purpose of this thesis that it contains societies, individuals, technologies and objects, and beyond that will not be further described.<sup>126</sup>

The utilisation of a certain technology can function as communication among connections; either explicitly through the demonstrative function of one type of item, for instance a funnel beaker in a hunter/gatherer society (Schenck 2014), or in the implicit function the use of a certain object group entails. For example, by using pure weapon technologies societies are bound together through their participation in combat and their reliance on warriors (Vandkilde 2013: 42). Social connotations of technology can be intentionally expressed, for instance through the celebration of expertise (Dobres 2006: 29), or unintentional, as in the automated application of traditions such as cooking rice (Skibo 1992). These can exist simultaneously and in multiplicity within one object and its practice. For an archaeologist to separate between physical manifestations and different socially structuring processes/collective agencies, we need to conceptualise technologies so as to be able to study them outside of context. A suggested conceptualisation of technology, that will be applied in this thesis, will

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125. See p. 114.

126. To attempt an exhaustive, listed description would indeed be to create false boundaries according to (Rorty 1999).

therefore be presented in the next section.

The combination of theories on structuration, taste, fields, doxa, habitus and agency is collectively called 'practice theory.' Practice theory is nothing new in archaeology. Quite on the contrary; according to Matthew Johnson (2006) it has been one of the theory sets most commonly used, and is well established in regards to research on technological and practical aspects of the past (Busuttil 2012; Dobres 2000; Dobres 2010a; Pauketat and Alt 2005; Rowan 2012). It is also found in the experimental discourse, however with a much lower frequency. Still, with regards to technological issues it is still probably one the most cited theories applied to analyses (Busuttil 2012; Peelo 2011; Stoner et al. 2014). It is also significant that practice theory, practical experience, and pragmatism is a viable combination (Bogusz 2012). When Bourdieu was asked this question, he responded:

*Indeed, the affinities and convergences are quite striking ... . [T]he theory of practical sense presents many similarities with theories, such as Dewey's<sup>127</sup>, that grant a central role to the notion of habit, understood as an active and creative relation to the world, and reject all the conceptual dualisms upon which nearly all post-Cartesian philosophies are based: subject and object, internal and external, material and spiritual, individual and social, and so on.*

Bourdieu and Wacquant 1992: 122

Because of the easy correspondence between practice theory and pragmatism will be pursued as a way of analysing the results of a typical experiment in the remainder of this thesis. However, what it still missing from the theory set that will be applied in the case experiments is a theory about *practical* practice. In the remainder of this chapter, the practical archaeological study of actually practiced technology will be explored.

#### **4.2.2. Technology conceptualised**

Technology can be difficult to define within a web-concept, and to perceive technological web-relations as one large, abstract and blurry entity is not really sought after in the experimental discourse. To generate meaningful analyses, to

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127. See p. 115.

speak of the web of technology is likely too vague for the purpose of object-centred, technical discussion that tends to dominate the field of archaeological experiments, and such expressions are rarely seen, nor would they help the detailed discussion and the focus on materiality we tend to hold (Van Oyen 2015b).

Categorising is one way of conceptualising technology and connecting it to real lives. Current demands for accepted concrete experimenting necessitates clear research questions and hypotheses, and will also in its deep influence of methodology from the positivist natural sciences pose criteria of clarity.<sup>128</sup> In order for the analyses to be meaningfully discussed and therefore procedurally comparable – currently the main criterion for specific, technical discussions, also in practice – analytical units are already defined in the discourse. Typical categories are specific (sequences of) processing and production techniques (Bradley 2013; Gansum 2004), end results – their constituency, properties and use (Mallol et al. 2013; Zimmermann et al. 2004), and taphonomic questions (Adams 2014; Saladié et al. 2015). Other meaningful, discourse specific units are currently skills (Apel 2006; Grimaldi 2014; Liardet 2013), (maritime) travel (Edberg 2009; Englert 2012a), and living conditions (Christensen and Ryhl-Svendsen 2014; Liedgren and Östlund 2011). These concrete categories may be one of the reasons why there is so little research on intangible, technological questions, bar skill.

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128. See discussion of scientific factors in experiment definition in sections 2.1. and 2.3.

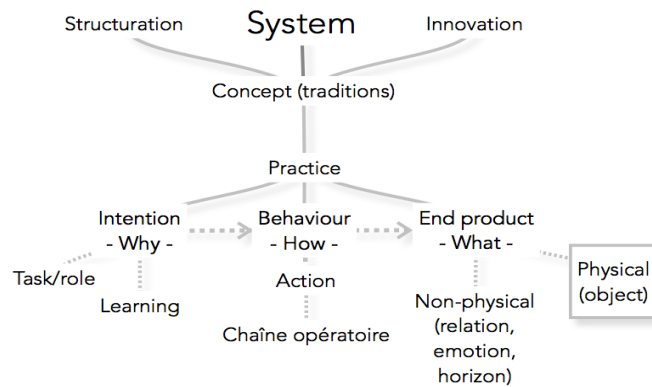


Figure 3: An open-ended conceptualisation of technologies conditioned by a system.

A generalisation of discourse generated categories into system/field-connected units is attempted in Figure 3. It is the aspiration to approach technology with meaningful categories that can provide anchorage as to how an experiment with an intangible outlook is structured, and as the purpose of this analysis is to work with typical experiments, experiment grouping should be discourse driven. The categories are sufficiently fluid so as to not be decisively demarcated, and they are shown to interrelate, inspired by the social web discussed above. The connections are only illustrative, as full embeddedness – the norm according to both pragmatism and practice theory – will have connections between all units.

The categorisation begins with a structured system, from which concept groups develop. These can take the form of for instance religion, traditions, customs, morals or political organisation. Concepts may not be technological on their own, but are larger notions that influence technological practice profoundly by defining and re-defining the frames within which the technology must operate, and are thereby structured. Technological practice takes multiple forms that produce consequence.<sup>129</sup> These can be divided into intentions, behaviour, and end results (Fig. 3), and will be used as a base for experimentation in Part II to highlight the backdrop of social surroundings. Most archaeologists study the end results of technology in the form of physical matter, to address the why and the how. However, the consequences and end results interweave with and create criteria for future practice, as do all the other

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129. For discussion of consequence in philosophical pragmatism, see section 3.4. and section 3.4.1.

components. Systems are also seen as intertwined with other systems. This thought of entanglement is well established in archaeology (Cassen et al. 2011; Dobres 2000; Hodder 2011), and **can be said to be** based on the **constant, subjective pattern of interpretation in hermeneutics**. However, this does not mean that an intertwined system is "... the plastic creation of human subjects" (Giddens 1984: 26), and the closely entangled elements in a society should not be separated into subjects, objects, physical and non-physical as factual entities (Brück 2005; Deleuze 1994; Dobres 2010a; Hodder 2011). The above categorisation should be read in all directions and seen as part of an unlimited process in time, space, action, thought and instinct, and it should be seen as a vehicle for analysis rather than a definition absolute.<sup>130</sup>

Hermeneutics and entanglement renders *any* definition subjective, and for this reason, beyond the categorisation in Figure 3 a fixed definition of 'technology' has not been and will not be attempted in this thesis. It follows from the above discussion of the social web, that definite definitions of 'technology' or any form of social endeavour, such as in system theory (Binford 1962), should be avoided. It is important to note here that the 'system' of Mathieu (2002b) and that of Lewis Binford (1962; 1965) are mutually exclusive: whereas Mathieu sees a system as inreplicable and inaccessible to outsiders (archaeologists), Binford takes the exact opposite view. However, it should still be remembered that categorisation does play a significant role in archaeology, and the entire subject field may be contingent on the premise of categories for definition and conceptualisation of something unknown, in order to make it understandable (and see Apel 2006: 207f).<sup>131</sup> However, in this way, we do create artificial boundaries where there may have been no discernment in the past. Whether this premise rests on credibility, replicability or other methodological factors for reaching consensus in the discourse, is not under scrutiny here. It should be sufficient to state that every research discipline has its own baseline methods to come to grips with the unfamiliar, and categorisation is one of those baselines for archaeology as a discipline that studies objects from the past. As long as categories are flexible and open to be given new meaning and content, this should not hold archaeologists back from studying an endless process such as

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130. See discussion on p. 21

131. Also see section 3.4.2. for a further discussion on flexible categorisation.

a technology.

### 4.3. The study of past technologies

The manifestation of agency in the form of taste, practice, tradition or other conceptual notions is visible to archaeologists through the end products discussed in section 4.2.1.2. As mentioned earlier, conformity in form and practice over a certain spatial area may signify the structuring system of the people who lived there, and it is clear that such an overlying structural system is a valuable source of information about past societies (Dobres 2010a; Giddens 1984). Whether or not these norms and ideas are in fact identifiable by archaeologists through the application of methodology is one of the main questions for this thesis.

Among the most common approaches that relate practice theory to technological procedures is the perspective of the *chaîne opératoire* (Croucher 2015; Dobres 2010b; Gardner 2012). Through this approach, a chain of operations is identified and often followed in reverse order by the archaeologist from the starting point of the physical object or other end product of a technological process, such as a lithic object (Bradley 2013) or iron slag (Baron et al. 2014). Its origins are found in biological principles of evolution, and the notion that a *chaîne opératoire* is pertinent for archaeological study was introduced by André Leroi-Gourhan, who, heavily influenced by Marcel Mauss<sup>132</sup> and Henri Bergson, proposed that technological gestures were ruled by syntaxes similar to language. A gestural syntax would originate from and coincide with the concepts of that particular community that also resulted in its language, and was "...brought forward by memory and born between brain and material environment"<sup>133</sup> (Leroi-Gourhan 1964: 164; Schlanger 2015). Leroi-Gourhan proposed that by identifying a progression (or regression) of gestural techniques, archaeologists could reveal the step-by-step procedure resulting in

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132. Notably in this context, Mauss saw technology as wholly socially integrated (Mauss 1973), while Bergson was revolutionary in uniting heterogeneity and continuity, which later is likely to have led to phenomenological notions of a unified body (Deleuze 1994; Lawlor and Moulard Leonard 2013).

133. Author's translation



an object, as well as cognitive understanding and dexterous sophistication (Leroi-Gourhan 1964; Martín-Torres 2002; Schlanger 2005: 5, 7). Although the chaîne opératoire approach was instituted through experiments with lithic production sequences and has become part of a typical lithic analysis (e.g. Damlien 2014; Sinclair 1995), it has now also moved beyond production to use and discard, and into the realm of other technologies (Dobres 2000: 200f; González-Ruibal et al. 2011; Jeffra 2015); and even into the professional work of archaeologists themselves (Rossenbach 2012: 98). The chaîne opératoire has become one of the most mainstream approaches to technological practice in an archaeology of today, and brings together human being and technology in the object, observable to the archaeologist through embedded gestures. However, it still seems more popular in lithic analysis than elsewhere. It is possible that the striking visibility of gestures, especially in flaked lithics, contributes to the use of this concrete human/thing approach.

An important issue with the chaîne opératoire approach to technology is the generalisation from individual to community. Particular gestures are highly individual and rely on dexterity, skill level and purpose. Certain things, such as a pot, can be functional even at a very crude level, and a minimum level of functionality can be achieved by many different techniques. However, regardless of individuality, there will still be elements of communal notions such as choice of materials and proportions, size, and basic shape. Moving from an object as an expression of individual agency to a general concept is one challenge in archaeology, and generalisation is key to our practice. The specific problem with chaînes opératoires is that the individual must often be considered interference to get to communal features that denote ideas, concepts, notions, norms and overlying dynamics of structuration (Bar-Yosef and van Peer 2009: 15). This may appear illogical, as individuals of a society will carry communality within them through their membership of that society. However, their level of amongst others skill, error, negligence, body movement, alertness and learning ability is individual, and, although sometimes culturally influenced, does not resonate societal structures to the same degree. It is significant that an expression of individual agency does not denote an expression of sociality directly (Davidson 2009).

The generalisation of a chaîne opératoire can take several forms. A common way is to look for similarities in multi-step operations and connect these to certain societal groups as social markers (Rankama et al. 2006: 259). Through this approach, and additional research from other perspectives, one can advance towards a greater understanding of factors that may have influenced technological concepts within just that society. An example is seen in William Banks' use-wear study on Upper Palaeolithic Gravettian toolkits, that concluded that groups arrived on site with pre-prepared toolkits that were used intensively for a short time to process game (Banks 2009: 2, 52). The same perspective is also used by Rocco Purri and Simona Scarcella (2011) to standardise a regional production sequence of Neolithic globular pottery from Calabria.

A subsequent way to generalise chaînes opératoires is to look for disparate multi-step schemes and consider the meaning of difference. This is how Apel's study (2008)<sup>134</sup> approaches the question of social separation of skill levels. This is also how Dobres approaches technological norms for repair and discard in bone tool production and use in Magdalenian societies through an investigation of manufacture wear on Upper Palaeolithic bone tools from the French Midi-Pyrénées. (Dobres 2000: 200f). By investigating repair and wear patterns, her study showed that bone needles from five different sites no further than a total of c. 70 km apart display different craft traditions in the form of localised techniques for forming the eyes. These techniques were generally not shared between settlements. A total of eight sites, including the five above, also exhibit a substantial difference in quantity of repaired and reworked bone and antler artefacts (Dobres 1995: Figure 5), indicating different ideas of when an artefact was worn out and when it merited repair. When the same type of artefacts reached a certain degree of wear, they were repaired at certain sites, but were simply thrown away at others, forming general patterns of difference. In contrast to the established idea of a 'pan-Magdalenian' way, the consistency within each social group indicates particular, situated concepts of manufacture and repair. Looking for difference in the chaîne opératoire in this case provides a glimpse of intangible aspects of a technology: norms concerning production and discard that are otherwise lost. In this way, difference can highlight concepts in a way

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134. Detailed on p. 24.

conformity cannot, and can point to dissimilar purposes, needs, and notions of functionality, amongst other situated and socially structured doxa.<sup>135</sup> The two generalisation modes of a chaîne opératoire are sometimes termed technopsychological and technoeconomic uses of chaîne opératoire approaches (Bar-Yosef and van Peer 2009: 105), a terminology in sync with ecological functionalism more than structuration theories.

Whether a chaîne opératoire carries syntax, traditions or structured compromises of action, it is certain that the conscious and subconscious knowledge, motor skill, understanding and interpretation involved in the procedure is highly individual and situational. This situatedness complicates generalisation, and creates an additional level of immateriality by the sheer difficulty of observation it presents. The **circumstantial** situatedness of a technological activity is intangible in several ways: un-tangible in its lack of matter, but also un-tangible in time and space, as its momentary character means that it has most often already passed. It is also un-tangible as an abstraction in the academic understanding. Because an archaeological research situation assumes an access to the past person via their objects, an additional un-tangibility comes into effect: the present relationship between the archaeologist and the material culture. This relationship depends on cultural background, technological and interpretational experience, morals, knowledge of other cultures, and numerous other factors (Coudart 2006: 134). However, these difficulties are not only present within a chaîne opératoire, but exist in most or all archaeological theory so far. The notion of chaîne opératoire is one of the few theoretical approaches that puts the object first and derives subject-related information from the object directly, which is likely why it is frequently considered to be a suitable perspective for experimenting. As a tool to reveal intangible backdrops for past technologies it has been successfully applied by several (Apel 2008; Dobres 2000; Högberg 2006; Pélegrin 2006). In addition, it is also established in experimental archaeology as a common perspective for the study of technological procedures. However, both Jan Apel (2006; 2008: 95) and Caroline Jeffra (2015) highlight that social background is often left out of experimental designs and chaînes opératoires, and that steps should be taken

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135. See p. 130 for definition.

to consciously advance on the influence of such on the technology at hand.

As mentioned earlier, if we do preach that every human life is fully situated, then researchers are no exemption. For instance, our categorisation schemes, previously discussed, are artificial constructs of our time and field, where such categorisation is important. The intangible embeddedness of a technology both in the past and in the present makes interpretation of that technology doubly problematic. The final interpretation will be entangled with both the subject's and the researcher's contextual situation, and this circumstance is suitably called *double hermeneutics* (Giddens 1984: xxxii; Ginev 1998).

In contrast to a single hermeneutic – where researchers interpret an object via their own horizons, a double hermeneutic happens when a researcher with their horizon presupposes the other person's embedded situation, and hence their interpretation or understanding (Fig. 4a). In other words, one person (researcher) is interpreting what another person (subject of study) thinks. Social scientists, who largely base their research on this situation, normally have the opportunity to ask their research subjects directly. Although the answer may be distorted or sometimes illegible, such exchanges often present the researcher with a deeper understanding of social data, and is the reason why interviews are a common method of data collection in for instance social anthropology (e.g. Vannini et al. 2009: 464). However, as archaeologists do not have this opportunity, the object-focussed mode of investigating materiality very often comes into play, and attempts are made to understand past people's social reality through their cultural remains (Fig. 4b).

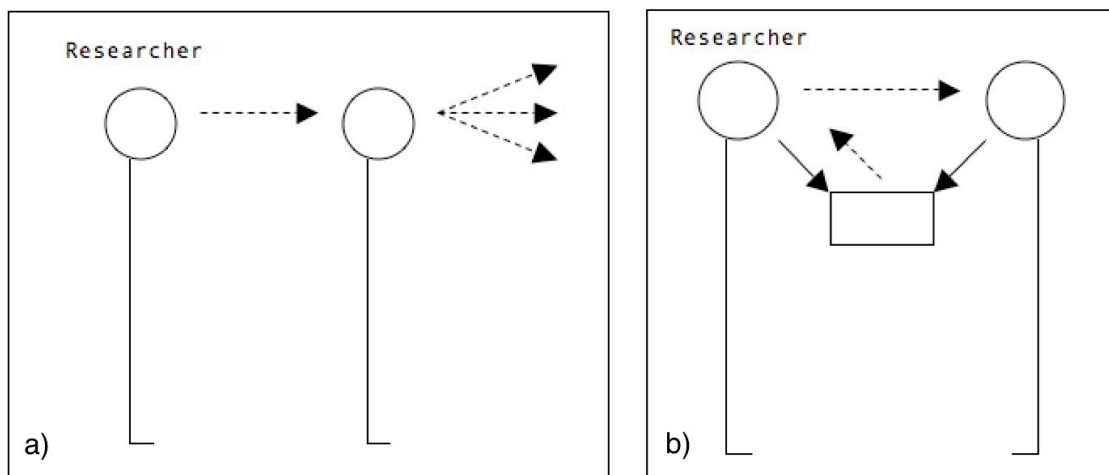


Figure 4: Double hermeneutics in Archaeology. a) Considering a past person b) Considering a past person via their material culture

The object-focussed mode of researching materiality, for instance a technology, aims to establish a link to social agency and structuration processes in a certain community as mediated through the object. As archaeology is an object-focussed discipline, a plethora of methods focus on the object as source of information, and in order to be used for informing on past societies, many of these will necessarily have certain investigative strategies that relate to intangible information; such as the close connection between refitting, the sequential chaîne opératoire approach, and gestural perception (e.g. Driscoll and Warren 2007; Hildebrand 2012). The experimental discourse has made the archaeological reference its gauge, and an experiment must therefore be loyal to the object to be of an accepted standard. Relation to object becomes the benchmark for all archaeological experiments, also those who search for intangible, technological aspects.<sup>136</sup> However, there is a less clear strategy in experimental archaeology for how to approach intangibility. The most applied scheme is probably also the chaîne opératoire/gesture connection, which seems to be especially successful in lithics (Bradley 2013; Knutsson 2006; Smith 2015), and ceramics (Gheorghiu 2011; Jeffra 2015), but also applied in relation to other raw material or processes (Dillmann 2009; Steguweit 2015; Tencariu et al. 2015).

The empirical base of archaeology has remained intact throughout the influences of the postmodern paradigm. More to the point; as well as required in a typical experiment, it is paradigmatically important in this thesis, through the application of philosophical pragmatism which generally concerns empirically observable units. However, intangible information is not only retainable through the primary object itself, but also through the conditions the end result of a process required to reach that state. It is therefore important to note that "object" in this regard, spans all matter available, such as production structures and waste products, as well as the physical and chemical criteria necessary to produce the object and waste, and mineralogical and biological constituency. This information may either come from the object itself, or from other objects in its circumference. Significantly, it is generally empirically available and

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136. Section 2.3.

necessitates no substantive theorising to be observed.

As mentioned, the principal difficulty for archaeologists, and the reason for all our theories, is connecting the object to the overlying societal sphere through the 'interference' of the individual people that made, used and discarded it. This thesis will investigate whether an experimental archaeological approach can provide a beneficial route for creating and consolidating such links between present day researcher, material culture and past social environment. However, there are a number of difficulties that need to be considered to strengthen a link and increase its likelihood – the wanted consequence of abduction.<sup>137</sup> Firstly, the relation between past social environment and object is non-physical or intangible, and this is also the case for the relation between the present social environment of the researcher and the object. Neither of those relations can be empirically observed or measured, and thus cannot be entirely delimited and defined. According to the hermeneutic view; whether we gain the information we desire cannot be confirmed, nor can our bias be set aside with any certainty.

Even if the object is the tangible manifestation of something intangible, it will not contain all the intangible information that went into that product. An axe used for building a specific boat type does not contain all the information about the finished vessel. This creates a problem of selection: which object and which information do we choose to research? The problem also surfaces when an object such as an axe has seen the involvement of more than one agent: the smelter(s), the smith, multiple boatbuilders, and several family members depositing it in a grave. Whose relation to the object do we decide to pursue? And which researcher is present for the interpretation? A credo of postmodernism is that these factors are beyond our control and cause severe interpretational issues; in archaeology only rarely taken to what seems to be its logical end – relativism (Ramberg and Gjesdal 2013; *Shanks and Tilley 1987*). Whether relativism is in fact the end point, is not up for discussion here, but it must be stated that the above problems are not easily overcome by any archaeological method investigating intangible or tangible knowledge. Embedded contextuality, a premise not only for postmodern theories, but also

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137. Discussed in section 3.4.1.

for philosophical pragmatism, perpetuates the problem even when the object is definite. The sociologist Loïc Wacquant, a close colleague of Bourdieu and a prominent practice theorist, speaks of a "'magical moment' of fieldwork that crystallized this theoretical hunch and turned what was initially a side activity into a full-blow inquiry into the social logics of incarnation" (Wacquant 2011: 86). In this way, he illustrates the practical, bodily nature of habitus and agency. Although this is not available under the same circumstances as for sociologists, it does indicate that to experience something as a researcher is to gain understanding for what it is like. The practical, experiential nature of most Experimental archaeology is likely to be the closest possible match in archaeological terms. Although we will never be able to *fully* understand exactly like the study subject, Wacquant goes on to argue that we should not treat our bias as an obstacle to understanding; rather we should use our presence in the situation as a "vector of knowledge of the social world" (*Ibid.*: 88).

The influence of situated bias on experiments has already been discussed in relation to the actualistic-criterion,<sup>138</sup> but the problem of two understandings contained in one archaeologist's interpretation is one that needs constant consideration and awareness, as contextuality and situatedness remain important to grasp the social structuration of a society. In relation to the case experiments in Part II, the practical application and complication of researcher bias will be further discussed in section 9.5.

The study sample reviewed for this thesis<sup>139</sup> revealed that experimentalists as a group are slow on the uptake of interpretational theories. For this reason, this project is not aiming to explore the latest theories, such as Actor-Network Theory, but has rather chosen the well-established practice theory; which embeddedness, field, agency and habitus are compatible with pragmatism. However, as this discourse is small and the fact that interpretive theories are rarely used specifically, there are only a few examples (Busuttil 2012; Peelo 2011; Stoner et al. 2014). A few experimentalists have taken to Tim Ingold's and Bruno Latour's non-dichotomic merging of the body in the world (Koerner 2011;

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138. See section 3.2. for a discussion of this criterion for a good experiment.

139. Appendix A

Marshall 2011);<sup>140</sup> and there is also a small trend of phenomenological studies (Brück 2005; Clarke and Renwick 2013). Neither of these are generally used for technological questions, and so can not provide typical theories. In the Americas, a theory set called *behavioural archaeology* is sometimes applied (Hollenback and Schiffer 2010; Schiffer and Skibo 1987), as are evolutionary approaches (Cross et al. 2002). However, as opposed to practice theory, the generalisation that these theories rely on does not resound with the situatedness of technologies according to philosophical pragmatism (and see Killick 2004: 572), but rather with processual ideals. They will therefore not be applied for the experiments here, nor are they represented in the European discourse to any substantial extent; arguably the hub of experimental activities.

Furthermore, through the considerable application of the *chaîne opératoire* approach in experimental work – a practical conceptualisation of situated practice (e.g. Jeffra 2015; Pélegrin et al. 1988; Pélegrin 2006; Apel and Knutsson 2006a; Bar-Yosef and van Peer 2009; Gheorghiu 2011; Smith 2015; Eitam et al. 2015; Dillmann 2009; Bourgarit and Thomas 2011; Morin 2012), practice theory is assumed to be a theory set that may resound in the experimental discourse about the intangible, and may therefore provide grounds for a typical experiment analysis.

#### **4.3.1. Applying practice theory and the *chaîne opératoire***

To use practice theory in experimental archaeology is to conceptualise *why* someone does what they do. The application would entail seeing technological expression as part of a flexible system that is open to individual influence. To apply practice theory is to generalise the motivations of a community, but also allow for specific, personal motivation of practice. An example is to approach a flint axe as an expression of both a general style template and personal, individual skill (Lekberg 2006; Wentink 2008: 152). The difficulty is the move from one to the other in a non-hierarchical fashion. For example, most archaeologists happily talk about individually crafted pieces as if the craftsman did not exist, but the object was instead a direct product of a

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140. This disregard for division and re-situation of the people in/with the context resounds well with philosophical pragmatism in certain regards.



singular, monochrome and homogenous society (Dobres 2006; 2000: 110-111). Even nail and finger imprints on pottery – definite personal marks – have been used to opine on the community that produced the pottery without much thought for the person attached to the fingers (Natali 2015; Schenck 2010: 61-62). The chaîne opératoire approach postulates that most, if not all, technology goes through a trajectory which includes aspects of institution – use – discard. Even an expedient cobble for cracking nuts is selected, used, and then thrown away. All chaînes opératoires are connected to one or more persons, and the chaîne opératoire approach is constructed and elaborated to provide a view to agents through gestures, plans, skill or concepts, which cannot exist independently of individuals. Combining it with practice theory is useful because it provides an explanation for how the actions of an individual is connected to a society, but not subordinate to it. The combination can lead us away from scientific generalisation of humans (Dobres 2010a; Jones 2002), and towards a consideration of processes that occur between agent and structure, where the agent has the final word and the power to do what s/he wants. Within the application of practice theory and chaîne opératoire, whether individual details or societal details are discussed, is not really a significant determination, as both feed into the same process of existence and practice. Eventually, both levels are expressions of practice in the same society, where one predetermines the other and vice versa.

Practice theory explains how person and society is linked, but does not provide practical, methodological guidelines for detecting connections, nor should it, as it is widely applied across social and humanistic disciplines. Although the chaîne opératoire approach is one way of expressing it in archaeological, technological terms, this is also a principle that is applied very widely and across archaeological methods. In the next chapter, a specific methodology for approaching practice through experiments and the chaîne opératoire will be detailed to conform to the thesis problem of accessing the intangible in a technology.

## 5. A pragmatic framework for evaluating archaeological experiments

As mentioned earlier, 33 % of the study sample<sup>141</sup> explore and present new methods for the exploration to their research problems, independently of their experiment protocols. Very few of these methods are similar to each other. Although presenting new methods often occurs in archaeology – a practical, empirical field without a set methodology – this feeds into the diversification of experimental archaeology – a sub-discipline considered to be a method in itself. Some of the presented methods are narrow, such as Laxmi Tumung et al.'s exploration of the use of SEM for use-wear analysis on shell tools (2015), whereas other are wide and could potentially be relevant for a broad range of applications, such as the general, experimental guidelines of Crumlin-Pedersen (1999).

The proportion of methodological articles in the 79 studies with a technological focus is somewhat lower than overall; 19 studies; 26,6 %, of the technological articles present a new methodology. At the same time, 21 of 33 studies that present new methodologies are *not* technologically focussed. Although the sample size is too small to be representative, it is interesting to note that this could be an indication of a more established methodology for technological studies, which typically does *not* focus on interpretations of the intangible. At current, as there is a lack of discourse to model experiments of intangible technological aspects on, a methodology will be constructed for the specific purpose of this thesis.

In this thesis,<sup>142</sup> the chaîne opératoire will be considered a focal collation of a particular technology into one conceptual and practical trajectory. Nevertheless, it is important to note that what we consider *one* chaîne opératoire, is often constituted by many 'lesser' chains. The way in which many processes grow into one, overlying process, is what becomes so visible in the experiment

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141. Appendix A.2

142. For a thorough argumentation, refer to section 4.3.

categorisation of Mathieu.<sup>143</sup> At the same time, this may also become very clear during experimentation. For instance, if an experiment centres on pottery production, as one of the case experiments does, this process involves several very defined stages with marked intermittent discontinuation (Gibson and Woods 1997): the processing of clay and temper and shaping of the pot may occur in one long process. But if the pot is fired in a wet state when its matrix is still very unstable, it may spall and shatter in the firing due to thermal stress and uneven water evaporation. Therefore, pots are most often left to dry before firing. In an intermediary stage, where the pot is 'leather hard', it can be decorated without warping and burnished with little displacement of the paste, and so this often occurs in between initial shaping and firing. The firing event presents another marked activity. Altogether, these can be seen as one, three, or more activity sequences. Under experimentation, it becomes clear that it would be laborious to shorten the sequence or skip a step, and so the experience of the experimenter very often becomes significant in the interpretation.

The sequential outlook of the chaîne opératoire can and will be used in several regards in the following methodology: first and foremost, it will function as an isolation of a technology into an experimentally manageable unit. This will be part of a categorisation scheme to be presented in this chapter. Secondly, the past chaîne opératoire of a technology will be applied to the experimentally chosen unit as a context of continuity. For instance, the pottery experiment is focussed on the firing, but the fact that the archaeological pots were used after they were fired is significant for how the production was performed from forming to firing. This is what is referred to as the archaeological chaîne opératoire. Thirdly, the experiment will have its own chaîne opératoire, of which certain parts may be representative of a typical experiment, and parts may be representative of the past chaîne opératoire. In this way, an experiment is seen as a sequence of connected practice, from first ideas until final analysis, rather than an isolated event. This is what will be referred to as the experimental chaîne opératoire. This broad view is applied to conceptualise how considerations of practice, assessment, interpretation and dynamics should

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143. See p. 58.

concern the entire experimental process, and is intended to provide a template for the author for how extensive the application of for instance a theory should be. It also highlights that just as in the archaeological chaîne opératoire, valuable information may be situated all along the practice sequence; not just in the experimental end results.<sup>144</sup>

The discourse analysis of experimental archaeology (Chapter 2) was carried out to help establish consensual agreement on what an experiment is and should be. This agreement will function as an overall entry point to the experimentation at hand and form the backbone of the experimental chaîne opératoire. In the following, four key points will be discussed with reference to the discourse guidelines: archaeological primary reference, experiment plan, preparations, and experiment execution.

### **5.1. Archaeological evidence as primary reference for arguments**

As discussed in Chapter 2, tangibility is of such significance to experimental archaeology that it is almost universally demanded as a primary reference for an experiment. How, then, can an experiment be reoriented to tackle the intangibility of the non-object parts of society? In a sense, the aim of this thesis is to investigate whether experiments can be used as a “missing link” between the tangible and the intangible. If so, a connection has to be made between the tangible and intangible in the experiment itself. Because the object as primary reference is a more or less non-negotiable criterion for a viable archaeological experiment, this project sets out to apply this criterion in the representation of a typified experiment. At the same time, the criterion should be used in a thoughtful way that can mediate the tangible and intangible and the past and present.

To use the archaeological evidence in this mediating fashion is not new (Bradley 2013; Dobres 2000: 200f). To investigate why it is less used in experimental archaeology, it seems most relevant to take the established way of study:

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144. For an example of such information derived from the preparation phase of experimental work, see section 6.2.3., including Figure 12.

investigating the isolated elements. In archaeology, arguments and interpretations tend to focus on specific problems in isolation from their larger material and social surroundings. Examples of such isolated problems are site placement and distribution, pottery function, axe symbolism, meaning content of grave goods, or agricultural practice. These problems are in themselves not particularly isolated, but they become so by applying certain constraints such as type specifications; local/regional demarcation such as a site; time period; motif or aspect of subsistence. To produce a narrow problem seems to be established as good form, with more or less variation across regional trends of archaeology (see Bergman et al. 2004, Kristiansen 2012). In the archaeological society in question, these issues may be of cosmological, foundational importance, or they may not bear much notice at all. As archaeologists, such information may not be available to us. What may, however, be available, is the role that axe or a site location played in its society. To get there, we most often use archaeological evidence as the first step.

As the academic experimental discourse is exceptionally unanimous on the fact that an experiment must be based on an archaeological primary reference (Table 2), to isolate the evidence is strongly established in experimental archaeology. This is simply what makes the experiment archaeological, and separates it from a test of personal skills or whims. An experiment may in certain regards carry value although it does not have an actual archaeological reference (e.g. Meijer and Pomstra 2011), but in general the criterion makes sense for the application of experimental archaeology as a research method; the focus in this thesis. As most of the discursive guidelines are normative, the first step of any experiment should therefore be to determine what the archaeological reference is for their research question. At some point, it is pragmatic to investigate the reference firsthand to be able to claim a best-inference interpretation later on. This usually means examining and documenting an object; or visiting and/or investigating the documentation of a structure.

From the first examination of an archaeological object, we attempt to understand what happened to and with the object in its original context. Its surrounding community is gone, but may have left physical, observable trace.

By careful investigation, the nature of certain types of influence and thereby their connected actions, can be identified. For instance, bright, fresh scratches on a ceramic sherd are very often the result of the excavator's trowel use. Conchoidal flake scars can be ascribed the actions of a knapper, and smooth polish can be both intentional engagement of a person to smooth the artefact surface, or convey long time accumulation of wear by a connected action such as rubbing. However, to isolate evidence does not entail to only look to particles and scratches, alloy constitutions and cell composition. Most of this will rather be the job of the archaeological sciences (Jones 2002). Instead, to isolate evidence for experimental use can be to isolate the level of primary reference – how strong the primary reference is – to provide a base for the technological operations under scrutiny. Is there enough evidence to perform the experiment as a full-on reconstructed process at once; do we know enough to perform minute detail experiments in the lab; is there enough evidence to provide a base for an experiment at all? For instance: a heap of potsherds may not be enough evidence to perform a production experiment if the sherds are fragmented, crumbling and/or without crucial, decisive features intact. How do we tell from a potsherd if the pot was pinched or made by paddle-and-anvil technique? How can we tell that additions in the paste recipe are added and not natural? Even more so, an experiment looking towards function may be difficult to approach from a crumbling potsherd if the potential scratches from stirring the pot are not intact, or if the size and shape of the pot cannot really be determined. Sometimes there is not enough evidence to perform an experiment at all. How does one find out experimentally that the function of that crumbling potsherd was purely decorative? For the archaeological evidence to be the actual primary reference, the evidence must be given a close scrutiny with such questions in mind. Is the object alone enough reference? Is it necessary to take in more objects, surrounding structures, or other considerations? And crucially: Will the experiment have a potential to even increase the evidence for the interpretational outcome? It is important to consider the experimental potential to contribute new understanding beforehand, and if this seems meager, to consider whether the experiment will bear much value at all. At this point, if one wishes to go through with the experiment without sufficient grounds for experimentation, it may be that the experiment will not be taken seriously by peers. Potentially worse: the experiment may be seriously flawed but used as

an interpretation of the same peers, who have not been given the opportunity to investigate the evidence and evaluate its status prior to experiment: for instance an interpretation of the potsherds based on the experimental production of a pinch pot, where the features for such interpretation are not clear or even present in the archaeological material. To be able to evaluate the base for the experiment, and what kind of experiment can be undertaken based on that evidence, is therefore critical. Furthermore, to isolate which part of human history exactly one is focussing the experiment on, can be as important for the strength of the interpretational outcome. If one produces a pot, based on sufficient evidence, but the interpretation becomes more substantive than what was really indicated through evidence and experiment, this can be negligent of both.

The important question in this part of the procedure is to evaluate what the experiment itself will really comment on. For example: if the production of a pinch pot was to determine the procedure of pinching, one should remember that this does not say anything about the function of that pot, and vice versa. To isolate the experimental outcome in this manner gives the same benefits as the general isolation of archaeological problems as detailed above. It leads to a better understanding of exactly what the experiment indicates and less oversight of steps that should have been object of an actual experiment themselves, by Mathieu dubbed putting the cart before the horse (Mathieu 2002b: 6). In sum, it will lead to a stronger interpretation, also in terms of the pragmatic abduction we use as base procedure in archaeological interpretation: The discourse approves of the method of isolation, and this method is therefore considered to be better (increase likelihood) than other methods such as free interpretation on a grander scale.

After the initial examination and the further experimentation, the archaeologist may connect relevant object aspects to a specific human activity, habitus or agency. This is probably often done – for instance during the experiment in the intermediary format of "they must have done that and that with this thing." However, such interpretations rarely make it to publication, where a lot of experiments seem to make either no, or only a summary statement. As put forward in earlier chapters, it is the author's contention that notions of

intangibility should be taken into the final archaeological interpretation of the isolated evidence. This can be achieved through focussing on the practice elements we identify, for instance through experimentation, and subsequently applying a theory set such as practice theory as discussed in the previous chapter. It is notable that not all practices are intentional or related to the purpose of the object. For instance, the smooth wear from thousands of hands on the bottom of a stone pillar in a cathedral is not part of the object's intentional purpose (to secure the structure), but with time, it becomes intentional and related to completely different motivations (e.g. to feel the smooth texture). As such, practice elements (touching the pillar) can carry vast quantities (thousands of hands belonging to thousands of people) of intangible worldviews and social outlook (supporting a holy structure, feeling smooth texture that other hands have made). The pillar becomes material, tangible evidence for information otherwise inaccessible.

A good example of how a connection can be made between isolated, tangible evidence and intangible outlook is Lotte Eigeland's (2011b) paper *No Man is an Island. Transmission of Lithic Knowledge in Flint-Scarce Regions*. Through analysis based on her own experience as a knapper, she determines that substantial Late Mesolithic and Early Neolithic flint artefact assemblages from a certain region in Norway show hardly any traditional trace of learning, although the collections also indicate high skill levels (Ibid.: 128,133-134). As flint is not native in Norway, how did people learn to knap flint to the skill level witnessed in the assemblages? The use of non-flint materials for learning seem impracticable, as the region has few sources of transferable cryptocrystalline rocks that could be used to impart necessary skills in traditional knapping techniques. Through an experiment where beginners applied the less wasteful bipolar technique in different ways, she explored different reduction strategies which resulted in assemblages that did not hold the expected amount of classical learning mistakes expected from beginners. One of her tentative conclusions is that people in this region would use the bipolar technique as an alternative reduction strategy, because it is more economical, and less dependent on knapper skill level. Furthermore, she suggests that learning to knap must have been an organised activity, possibly to the extent that novices or apprentices would go for periods of intensive training elsewhere (Ibid.:



135,137). Through her experiments, Eigeland addresses the intangible practices of technological learning, and why this could have been done in the suggested manner, and she does this by interpreting her very tangible results.

Eigeland presents a full picture of knapping skill acquisition from the region and period in question through a reverse identification, from visual evaluation, through an identification of processes (in this example related to skill level), to experiment, where she situates the singular physical processes in alternative microscale contexts in the form of chaînes opératoires relating to the learning of knapping skills (one to one instruction, trial and error, or imitation). She then situates her results in relation to other archaeological research findings, and discusses a macroscale storyline. In this way, the experiments take on results relevant to the surroundings of the technology, and by doing so, create a better understanding of the people who interacted with the objects in question. In the last instance, her results are more relevant to the credo of archaeology: to study the people of the past, than an experiment that provides an understanding of the object unrelated to its makers and users (Ibid.).

In short, Eigeland's isolation of factors such as the presence or absence of certain knapping features (for example hinge fractures) entails that while factors such as style, symbolic aspects, morals, and social interaction were still important parts of the surrounds of the artefacts she examined, they were not up for experimentation. The methodologically notable part of Eigeland's experiment is that by isolation and selection, relevant intangible facets become easier to address and justify. If experimental designs are refocussed on level of evidence, it becomes easier to approach aspects that are supported by evidence, than those that are not. In this way, the presence, indication or total absence of actual fact (e.g. marks on object, or its material) related to microscale context (object itself, physical context, or actual technological action) filters the relevant actions to an extent that they are manageable to address. By filtering an entire lithic assemblage down to the presence or absence of hinge fractures and other definite features, Eigeland could disregard all the other marks and traces as irrelevant, and seek to explain selected features in light of how she associated the evidence with the actions of teaching and/or learning (technological evidence). She could then attempt to recreate the evidence

through experiments, confirming through experiments that other features could also leave traces of learning. The results of the experiments invalidated the association for which there were no actual indications ("the absence of typical learning marks such as hinge fractures means that learning did not take place") and justified indirect evidence that can be associated with learning ("The absence of hinge fractures is no longer a criteria, as the presence of marks of bipolar knapping may indicate learning as well"). The experiments thereby improved the evidence for the intangible process of learning to flintknapping from none at all to evidence by association – the evidence for learning could now be associated from the lack of features in lithic assemblages. Especially so in the societies which assemblages Eigeland examined, but also in bipolar technology/alternative reduction strategy societies elsewhere, where absence of typical learning features such as hinge fractures no longer justify a conclusion that hands-on learning did not take place.

To simplify; the address of intangible facets can be as schematic in form as an encounter with a fact that broadens the researcher's gaze in a reverse hierarchical manner; from the artefact and onto the humans in search for an explanation. This matches the standard archaeological view of understanding human societies from the artefacts, but by looking for indications of intangible as well as the tangible aspects, archaeologists can avoid generalising human individuals, and at the same time improve connections between humans and their artefacts in the conclusions they reach. Ultimately, through the look at individual artefacts, as in Eigeland's study, one can reach new understandings of the overarching society in question by filtering macroscale possibilities down to the microscale level: from the individual artefact, Eigeland could reach an understanding of the ideas of how to teach and how to learn in Late Mesolithic and Early Neolithic societies of Eastern Norway.

The review of academic literature on experiments show that researchers most often experiment on a microscale in line with the ideal of narrow problems discussed above. Experimentation would for example focus on object properties, production of one specific (type of) artefact, or (a part of) a specific physical process. For the sake of evaluation, and to represent a typified experiment as basis for the analysis, the experiments in this thesis will

specifically approach what will be considered the smallest viable unit of analysis as an aid to evaluate the experimental connection to situations. However, at current, there seems to be no systematic fashion to do this, where very different case experiments can be compared to looked at the same indicators. To approach the methodology in this way should be consistent, and in the following, a suggested categorisation based on evidence level and scale will therefore be presented. Over the next few pages, this scheme will be detailed for use in the case experiments to come.

#### **5.1.1. Level of evidence**

In the following categorisation of experiments, the *degree of evidence* is the most prominent factor. This will be the focus point, so that the connection between tangible, primary reference and its intangible surrounds is upheld in a manner where the tangible always takes precedence. The categorisation will address and conceptualise how proximate the interpretation of the experimenter is to the archaeological reference, and whether the evidence has increased after experimentation. If so, the likelihood of the experiment conclusion and interpretation can be considered to have increased as the conclusion has augmented the archaeological reference compared to before experimentation. The proposed categorisation is schematically presented in Table 3, and as it has many components, it will be explained in detail below. The categorisation is constructed around 'degree' of evidence, scale, and chaîne opératoire, and forms the backbone of the method framework in this thesis.

Table 3: Experiment categorisation scheme. Experiments are placed according to scale (horizontal), chaîne opératoire task division (vertical) and type (internal horizontal) and degree (vertical subdivision) of evidence (horizontal subdivision). E = physical evidence. T = technological action evidence. C = physical context evidence. Min = minimum scale. Max = maximum scale.

Chaîne opératoire/ Object Biography ↓		Scale →												Min	Max
		Object			Behaviour			Process			System				
Evidence type	→	E	T	C	E	T	C	E	T	C	E	T	C		
1. Manufacture	Min	Direct			Direct			Direct			Direct				
		Indirect			Indirect			Indirect			Indirect				
		Associated			Associated			Associated			Associated				
	Max	None			None			None			None				
2. (Inter-)use	Min	Direct			Direct			Direct			Direct				
		Indirect			Indirect			Indirect			Indirect				
		Associated			Associated			Associated			Associated				
	Max	None			None			None			None				
3. Abandonment	Min	Direct			Direct			Direct			Direct				
		Indirect			Indirect			Indirect			Indirect				
		Associated			Associated			Associated			Associated				
	Max	None			None			None			None				

As just discussed, evidence is important in an archaeological experiment. The level of evidence should be evaluated along three strands (vertical subdivision in Table 3). If an experiment is part of a series, the level of evidence should be evaluated for each segment undertaken:

- Physical evidence (E): the actual archaeological material for the inference, such as an object or a structure.
- Technological action evidence (T): the actual evidence for the technological operation.<sup>145</sup>
- Contextual evidence (C): the evidence for physical context of the object in question. This can be had if in situ access or documentation is available.

The three strands of evidence are sub-categorised into degree of evidence (horizontal subdivision in Table 3):

- Direct evidence: the evidence is *empirically available* – not only inferred. The object or structure is available for examination. For the technological action evidence (T) category, the evidence consists of actually seeing the researched technology in action through direct observation, such as is possible through ethnoarchaeology.
- Indirect evidence: the evidence is available through a *empirically available* representation, for instance an imprint or use-wear on an archaeological sample, or a shaft-hole in an axe. Other indirect representations are for instance site documentation, as these are inevitably interpreted first hand in the field.<sup>146</sup>
- Associated evidence: evidence is available through associative form, such as through an edge that suggests cutting or through skeuomorphs.
- None: there is no archaeological evidence whatsoever for the inference. This may occur in situations where

The degree of evidence is introduced because it provides a general measure of how the experiment refers to archaeological sources. As reflected in the

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145. Although this schematisation is biased towards technology, this category can be substituted for any relevant action evidence in the instance of further use.

146. The interpretative component is not always evident, but may be biased in elements such as selected angle for photography, over- or underrepresentation of aspects of a structure drawing due to interpretation of significance, or even number of points taken with the reflector when using a total station.

elevated status of the archaeological evidence in experimental archaeological discourse, and as became clear in the discourse analysis in Chapter 2, unnecessary bias, such as an disproportionate representation of ethnography rather than archaeological evidence as grounds for an interpretation (e.g. Hardy 2007), seems to be consensually agreed to decrease likelihood of an experiment, and therefore contributes to create a less than best inference.<sup>147</sup> This is possibly the reason for the scientific idealisation of the HDM in the discourse (e.g. Heeb and Ottaway 2014; Sørensen and O'Sullivan 2014; Clarkson et al. 2015; Kamp and Whittaker 2014; Khreishah et al. 2013).

As such, the structure shown in Table 3 is designed to maintain a focus on the experiment's relation to the evidence for the purpose of this thesis. Such visualisation will function here as a *de facto* gauge for likelihood level of interpretation pre experiment. It will also be a guiding stick for the subsequent evaluation of the experimental methodology and its potential to assess intangible technological aspects. The intention of this strict line of argument is to ascertain that, to the extent possible, the analysis focuses on the material, experimental procedure itself as a form of inquiry, rather than the immaterial opinion by its researchers. Because it is difficult to quantify how 'much' evidence there is for a statement, this is only a proposition for a rough minimum gauge for evidence.

The direct evidence in the form of the actual artefact (E) or context (C) should remind us that archaeological reasoning is not only focussed on experimental identification, but also includes distribution patterns of objects, typological ordering or landscape orientation. These perspectives are rarely directly involved when experimenting, but do often form the backdrop for the experimental outset or conclusion. Such factors as distribution of a tool with marks can provide useful input for analysing an object in relation to an experimental chaîne opératoire, if it becomes clear that the object was for instance moved through the site as part of its operational trajectory (e.g. Stewart 2010). The scatter of debris from the same object from space to space will be a very good indicator of an action for the technology in question; and can

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147. See Table 2, p. 44.

influence how we evaluate the artefact and its adjacent finds and structures. It is harder to attain direct evidence for the technological (T) action, but as mentioned, this can be a category worth exploring in ethnoarchaeology if that society is the object of archaeological scrutiny. Nevertheless, the evidence is maximum indirect if the ethnoarchaeological observation is used for the interpretation of an archaeological context.

The degree of evidence filters the experiment into comparative levels of evidence. However, experiments can be full-scale ship trials such as the Sea Stallion (Nielsen 2011; Nielsen 2012) or micro-scale knapping events like a singular production of a laurel leaf (Bradley 2013). It is difficult to compare these two events, although they both rely on indirect evidence for the technological production process. In this regard, it is mainly the scale that separates them, and the fact that a singular event is just that, but a full-scale ship building is a multiple event experiment – many experimental events into one: production of different components. These two experiments are particularly separated by *scale*, and the factor of scale will be one of the other divisional parameters in the categorisation scheme.

The scalar categories applied in the categorisation scheme originated from those of Mathieu, who groups experiments according to scale of context (Mathieu 2002b), for the purpose of isolation of the analytical units (the experiments). His four main categories *object*, *behaviour*, *process* and *system*<sup>148</sup> will form the horizontal axis of the scheme.<sup>149</sup> The vertical axis consists of a schematic chaîne opératoire, which will be further elaborated below. The division into chaîne opératoire stage, too, is a further subdivision of scale into the scale of an action sequence; for instance a use-sequence.

Mathieu's experiment categorisation covers microscalar to macroscale experiments. Mathieu highlights the relationship between variables and control, where an increase in variables leads to a decrease of control (*Ibid.*: 7). The smallest scale; *object* experiments, is concerned with isolated aspects of the

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148. Described on p. 58f.

149. While his micro-scalar categories are similar to those that are applied here, it was decided to predominantly apply his global categorisation for the purpose of addressing scale together with evidence specifically.

object, and provides a stand-alone present day interpretation. An example can be how a material reacts when it comes into contact with another material or is affected under certain conditions, such as how a piece of flint reacts to different hammer materials or angles of percussive impact. This category gives an insight that is independent of human hands (e.g. Chu et al. 2015), whereas the *behaviour* category will add a person into the picture and thereby increase scale. For behaviour experiments, an experimenter will look at the consequences of certain behaviours, for instance direct versus indirect percussion techniques applied to the same piece of flint, and so it focuses on both object *and* subject. The category often features subjectivity, especially in the interpretation and execution of likely past behaviour. The next scale of context is the process experiment, which entails a composite process of operations from both the lower categories, for instance the reconstruction of an entire production process of an axe, or of the taphonomic process of a bone. This means that process experiments should be preceded by isolated lower level experiments to avoid confusion in cause-effect relations. This is the ideal situation, but does not always happen (Mathieu 2002b: 5). Nevertheless, in the present thesis, lower level experiments will be approached singularly.<sup>150</sup>

The last category, *system* experiments, consists of a real society working with the processes in question, adding the dynamics of a society in which the technology has originated (e.g. Schiffer 2013a). As previously mentioned, the system category of experiments is by Mathieu considered to be encountered through ethnoarchaeological experiments exclusively (Mathieu 2002b: 6). However, in the present project, this categorisation will include full-scale trials of production and use that, like any experiment, can provide relevant data for analogical use, and where the behaviour and agency of people play an integral part. This can be found in for instance the already mentioned Sea Stallion experiment, where part of the experiments were to determine how the weight of people functioned as dynamic ballast, how well a large Viking war-ship can function if half the (ideal) crew is seasick, how large the quantity of food needs to be to work the ship to full function, and more.<sup>151</sup> Other experiments are those

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150. See Chapter 8, where a full reconstruction of tar production from pyrolysis through to collection, refinement and function testing was omitted in favour of an experiment with only one segment – the chronologically first stage of pyrolysis.

151. Interview with research coordinator at the VSM, 8/5/2013.



where a period of actualistic habitation in longhouses can help determination of indoor climate (Christensen and Ryhl-Svendson 2014; Larsen 2007).

The scale of archaeological experiments becomes important both as a comparison factor, but also when viewed in relation to the criterion of archaeological evidence. As seen in Eigeland's experiment presented at the beginning of this chapter, the isolation into smaller units create manageable experimental situations that each have clear relations to the relevant archaeological evidence. In this way it can be avoided to, in Mathieu's own words "put the cart before the horse:" starting at a larger experiment scale (process and system) without subdivision into smaller scale units can lead to misinterpretation of potentially foundational insights from its composites; lower scale level experiments (Mathieu 2002b: 6). This is reminiscent of Jeffra's view of arranging experiments into single segment or multiple segment experiments (2015).

To further promote analytical isolation, the framework will include three broad phases of the chaîne opératoire of an artefact that constitutes the second axis in the categorisation (Table 3). These are the general stages of *manufacture*, *(inter-) use* and *abandonment* that every object and material structure from the past is presumed to have undergone. To isolate these stages forces a subdivision of the use-life of an object that will highlight the important factor that scale and reference to archaeological evidence can also be found in stages of a use-life. The subdivision forces the researcher to look at the object differently for instance in the search for which elements of the object are related to which phase in its use-life. In this way it can focus the researcher's point of view, but is also a useful constraint that can be translated into the level of actualism that is needed in an experiment.<sup>152</sup> As such, the chaîne opératoire subdivision can be valuable in the planning and set-up of experiments and pinpoint what is the relevant evidence to use for an experiment, and what is not (*Ibid.*).

The chaîne opératoire can also highlight that an entire use-life is one long process, of which we sometimes take parts for granted. Therefore, we do not

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152. See p. 48

necessarily look into these parts – such as the chaîne opératoire of abandonment. Did a pot fall to the ground and break? Was it accidental? Did it leave enough trace to recreate the steps? Is the chaîne opératoire of abandonment at all relevant for the chaîne opératoire of artefact production or procurement? For instance; was a pot produced to be used as a grain reservoir or grave goods? To bring the chaîne opératoire into the picture as a transmitter of practice theory also helps to highlight that people's intentions and agency were active components of the use-life of an artefact. It underscores that technology is an action and operation where people do not 'receive' technology passively, but originate, innovate and act with it in a manner integrated into their daily lives.<sup>153</sup> Chaîne opératoire sub-division can therefore underscore that one artefact undergoes a complex web of social situations resulting in its use-life (Jeffra 2015).<sup>154</sup> Moreover, it hopefully becomes easier to connect small-scale actions to distinct intangible surrounds than to connect them to more extensive intangible environments on a higher level of scale.

Some experiments can fall into two scalar categories at once, which is a good example of pragmatic, flexible categorisation.<sup>155</sup> When this happens, a separation into even smaller units is sometimes possible. If this is not applicable, another approach can be to separate experimentation into laboratory work and field stage to explore different types of data. A laboratory experiment usually stays in the object category, as it is hard to mimic human behaviour through lab tests, whereas a field experiment can span all scales.

Although a lab experiment is modelled on the hypothetico-deductive method, as shown in section 3.4.4., it can fit into a pragmatic procedure as one of the premises in an abduction. Furthermore, to use scientific results based on HDM ideals as a premise for archaeological interpretation can be seen as a democratically approved methodology (Heeb and Ottaway 2014; Kristiansen 2014; Prescott 2012; Sørensen and O'Sullivan 2014). In general, to complement experiments with other experiments from the same, or from a different, category may provide a pluralistic perspective of which elements can

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153. See Dobres' 'disembodied hands' (Dobres 2000: 183-184)

154. This is possibly why some researchers prefer to see use-life instead as a socially influenced object biography, for instance Jones 2002: Chapter 5; Joy 2009; Pollard et al. 2014.

155. See section 3.4.2., page 114

be combined to reach a well informed interpretation of why past people chose to do what they did with their objects. Each component (experiment), including scale assessments, thereby increases the inference with premises to get to a 'best' inference argument for the interpretation of specific, intangible research questions.

By placing the experiment into the categorisation scheme as detailed, based on Mathieu's experimental scales, and subdivided into main chaîne opératoire stages, such as introduced in Table 3, the amount of intangible factors becomes more manageable from an experimental standpoint: instead of having to manoeuvre through *all* the concepts within the archaeological chaîne opératoire, only specific, relevant concepts become part of the experiment. To introduce a division according to the degree of evidence into a categorisation scheme sorted by scale and phase of chaîne opératoire, results in an experiment categorisation where one experiment can be isolated into a small segment of an object's use-life, while at the same time maintaining a view of

- 1) How close the experiment refers to the archaeological reference prior to, (during,) and after experimentation so that it has a primary archaeological reference, and therefore
- 2) Whether this (abstract) proximity has increased after experimentation.

Together the two interpretive perspectives on the status of archaeological evidence should be able form an inference that may form the basis for a new 'best' inference after the research is concluded. Nevertheless, It is significant that however much an experiment should refer to the archaeological primary source in an object-focussed manner, the discussion put forward in section 3.4. has indicated that experimental archaeology is but a facet of a multitude of storylines created about the past, and should continue to be so.<sup>156</sup> The above categorisation scheme should therefore not in any way exclude prior, parallel or subsequent investigations by other means, as promoted for archaeology under the pragmatic paradigm (Aldenderfer 2012; Jeffra 2015).<sup>157</sup>

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156. For a discussion of pragmatic approaches to archaeological research, see section 3.4.3.

157. See section 3.4.3.

### 5.1.2. Experimental protocol

Categorisation is only a representation of an experiment in abstracted form. For the actual research execution, and in the interest of a clear research plan for the current project, a close eye is kept on the criteria set forward by a consensual, experimental discourse. The promotion of archaeological primary references is important in experimental archaeology, but section 2.1.1. has shown that other ideals also aim to diminish the influence of unnecessary biases.<sup>158</sup> Although philosophical pragmatism postulates an unavoidable situatedness, as do the commonly used hermeneutical structuration of Giddens, and theories of agency and entanglement, in the address of intangible facets of societies;<sup>159</sup> bias that results from inattention to appropriate practice ideals is easily surpassed and hence usually considered unnecessary.<sup>160</sup> Additionally, it is important in this project to stay aware of biological determinism, which in its widespread generalisation of unconfirmed aspects of humankind, disagrees with most of the aspects of the selected theories of the intangible in technology.

In the following, the experiment protocol for the three case experiments to come will be presented. When this is presented normatively ("should") , this is with regard to the norms set for the present methodology, but it is also modelled on the discourse analysis to be as close as possible to a typical archaeological experiment, and the normative statements may have further reach than just the case experiments in this thesis.

#### 5.1.2.1. *Artefact or structure investigation*

To assess the current state of the object and interpret the chaîne opératoire stage it was last part of, it should be closely examined by relevant means. For an archaeological reference to be *primary*, it is important that an experimenter

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158. See Table 2, p. 44 for a list of experiment criteria that target bias specifically.

159. As discussed in section 4.2.2.

160. A variety of which is listed in Table 2.

examines available archaeological material first hand.<sup>161</sup> The investigator should have the necessary skills to evaluate significant parameters for experimentation, here a reflection of the criterion for familiarity with the experimental process put forward by some (Kelterborn 2005; Nielsen 2006). The examination must be pertinent to the research aim, which should be at a minimum loosely formed at this stage, as this will ensure that the relevant investigation is undertaken, with the appropriate methods. The skill of the investigator can be linked to the actualistic criterion in a field experiment (Coates et al. 1995; Coles 1967; Crumlin-Pedersen 1995; Schmidt 2005a), or the criterion for appropriate control levels in the lab (Marsh and Ferguson 2010; Rasmussen 2007b), as the necessary investigation needs to be undertaken to decide on these issues.

Sample size is important, and should be considered if one wishes to address research problems that exceed the scale of one individual in one moment – in archaeology almost always. To study generalised practice templates or style for a larger community, a reasonably large sample size should be considered. Much of archaeological sampling are done on a highly selective and subjective basis,<sup>162</sup> and it is therefore possible that personal notions and outdated practices have steered sampling, which may produce a less than representative selection if the sample size is small (Langley et al. 2011). In addition, there is often a low frequency of contemporary finds within a region, particularly for prehistory, and preservation issues can be severe.

Sometimes it is necessary to study connected objects as well, such as adjacent structures, debitage, or associated tools. This can particularly be useful to assess and appropriately describe a technological process. In addition to visual examination, documentation pertaining to objects can be crucial, such as site documentation, site plans, log books, photos, maps, field reports and further field documentation, especially if physical evidence does not exist. Other

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161. Sometimes, as experienced by the present author during the course of this project, examination will not be allowed, and an experiment will have to be discontinued because the necessary parameters are not documented sufficiently elsewhere. Such patchy documentation could therefore not function as a standardised experiment for the current method evaluation.

162. For instance: Throughout most of the 20th century, excavators in Medieval Oslo collected skulls, and tossed the remainder of a skeleton in a communal bone-pile, or threw them away altogether (Grønstad 2012: 13).

documentation such as sampling from objects or structures in question, pollen charts, geological information, osteology, and archaeometric results may be relevant to provide a bigger picture. This should be considered necessary if the study involves an object in relation to other substances; for instance when studying ground stone food processing such as in Chapter 7. It is important to remember that site information, where available, gives actual *archaeological* reference to an experiment, and thereby increases the viability of the results through the primary reference criterion. A thorough pre-experimental investigation can often be sufficient for context interpretation to forego results generated through other means, such as ethnography or traditional handicraft.

If archaeological primary references other than the object of the experimentation do not exist, it should be thoroughly considered whether the object alone can provide enough evidence to approach the research problem. If the object needs to be supplied with non-archaeological evidence, particularly if additional archaeological evidence is also lacking, the research problem may need to be reconsidered, depending on methodology. For example: if a stray find is to be analysed, it is important to be source-critical and stay mindful of what is considered "contemporary" in archaeology. For instance, a specific artefact can be associated with a very long period.<sup>163</sup> To then compare the artefact with similar types and make statements about the society it belongs to, would be notionally the same as comparing a standard roman iron axe (or any other implement we share with romans) with one from today and draw conclusions about the entire period as one unit; in other words; all the source criticism relevant to ethnohistory as an analogy is still relevant. As Hodder (2012: 12) has simply put it: "if things and societies in the present and past are similar in some aspects, this does not necessarily mean they are similar in others." On its own, a comparison between similar finds without complementary material is not necessarily fruitful or even valid as a sole base for reasoning. It is the author's opinion that stray finds or finds examined in isolation provide little reference, unless as supplementary material, for an archaeological experi-

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163. This is the case with the Scandinavian Mesolithic Nøstvet/Lihult adze which is diagnostic for a period of around 2000 years (c. 6500 BCE. to c. 4500 BCE) (Glørstad 2011: 65).

ment.<sup>164</sup>

Sometimes, a researcher will wish to experiment with a problem where no relevant finds exist, such as will be demonstrated in the experiments with structures for birch bark tar distillation, detailed in Chapter 8. If there is very limited material, it is highly desirable that other information is available if the experiment is to be considered archaeological. Such information is often found in site documentation, natural scientific analyses, and other relevant literature. It is important to not restrict oneself to the archaeological discourse, but explore other means to relevant information as well. In the case of the tar experiments, results from gas chromatography/mass spectrometry (GC/MS) of archaeological samples of tar was already published, together with available site documentations and analyses of several different tar yielding sites, finds that indicated intentional deposition of hydrocarbons in Neandertal fireplaces, as well as previous experiments by the same group of experimenters, forming the basis for the experimental procedure. However, the finds spanned a total of 200 000 years, and so the research question was designed to be of an exploratory character, as it was understood that no viable conclusion could be reached without proper archaeological grounding.<sup>165</sup> In relation to the birch bark tar experiments, we decided to structure the question so that the interpretation would provide one out of many possibilities, but be as viable as was currently achievable.

Once an examination of available archaeological and supplementary evidence has been undertaken, an abduction is inevitably performed in the mind of the researcher, often more or less in the form of "What is likely to have happened in the [respective] situation?"<sup>166</sup> All the observed, empirical facts and other conclusions drawn at this stage will form arguments for the abductive inference. An example could be:

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164. Note that this is where experimentation and experience becomes visibly separated. Although there may not be enough evidence to provide context for an archaeological experiment, there may be more than enough for personal experimentation with technique, or skill training for certain purposes. If the sample size is large, some generalisation may be possible, but the lack of context would in most cases create a poor base for an experiment.

165. This was a consequence of the lack of primary references and a prolongation of the principle that when in doubt, (re-)examine the archaeological material rather than fall into the habit of reasoning that "it must have been like this" without further archaeological information.

166. Interview with VSM ship reconstructor, 15/5/2013.

*The axes are a long, b wide, c thick, with shaft-holes of  $\varnothing$  diameter on average*  
*The axes have scratches and chippings along the edge*  
*Most axes were found in what appears to have been a forested area*  
*There are no shafts attached, but they are expected to have been hafted*  
*One axe was found close to a darker, humic patch of loam which could have been a workplace for limbing trees*  
*The axes have clear manufacture indications (e.g. flake scars/welded areas) which testify to the agency of the producers and the specific (e.g. knapping/smithing) choices they made to produce a useable implement*  
*etc...*

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Initial inference: the axes were probably used to chop wood

The preliminary assessment should be executed before the actual experiment is planned. The research question can then be re-evaluated should findings reveal aspects that were not known at the initial definition of the research question. Nevertheless, it is advantageous to have the opportunity to go back and forth between material and experiment, if this is applicable.

#### **5.1.2.2. Experiment plan**

After thorough assessment of the archaeological object comes the time to detail an experimental plan. Although the plan may have been initialised before the object examination, it should be re-assessed afterwards, subsequent to new information. This helps to ensure a truly archaeological primary reference and a solid and predictive research plan. There is a high level of consensus on this question in the experimental literature, and authors mostly differ only in detail.<sup>167</sup> (e.g. Adams 2010; Coates et al. 1995; Lubinski and Shaffer 2010). As indicated above, the experimenter will often only have a loosely formed idea of which aspect of the object s/he wishes to experiment with prior to examination. The experimental planning stage is when the research question should be finalised, and details about the experiment conditions and parameters should be decided upon. For the sake of this thesis, the plan will entail decisions on:

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167. Table 2



- 1) feasible and relevant research question(s) and potential hypotheses
- 2) which parameters to experiment with
- 3) motivation (thesis work, disproval of statements, dissemination, education etc)
- 4) how many different experiments are necessary to shed light on the research question
- 5) field or lab experiment conditions, or both
- 6) experiment structure
- 7) minimum level of expertise necessary and whether external help must be sought
- 8) experiment manager
- 9) budgetary limits
- 10) documentation plans
- 11) location, raw material, crew, time, and equipment
- 12) practical preparations (procurement of material, manufacture, training, etc.)
- 13) potential equipment necessary for the analysis (e.g. microscope, GC/MS, etc.)

**In the present case experiments, the plan will be revisited throughout so that feedback from the experimental process will affect experiment structure.** However, it should be stressed that a structured plan should exist before the practical preparations begin, in order for the experimental process to be executed as per discourse ideals. This is not the case for all experiments today, and experiment plans are rarely published in any case. However, it can often be seen from an experimental report whether or not the experiment was executed according to a rigorous plan or just a rough idea. For instance; the much alluded *Sea Stallion of Glendalough*, a full-scale, manually and actualistically built, Viking battle ship that was reconstructed over four years; sailed for three seasons of 4, 6, and 6 weeks respectively from Roskilde in Denmark to Dublin in Ireland, logging 2482 nautical miles total; had a sailing crew of over 60; a fully equipped escort ship; state of the art navigating equipment, and a total budget of 26 million DKK<sup>168</sup> must necessarily have had an extremely detailed plan (Nielsen 2011). Other experiments have less clear plans or strategies, and a

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168. Press release from the Viking Ship Museum 3/8/2008. Per the conclusion of the trials in Aug 2008 this equaled GBP 2,75 million.

substantial number of experiments come across as personal exploration or skill procurement, or dissemination rather than experimentation events. The lack of a plan makes it harder for peers to see which pre-experimental factors that may have influenced the research, and if the experiment has been according to standard, and can therefore be accepted as good. More importantly, it makes it harder for researchers themselves to structure a good research question, and to be prepared for the large variety of factors that could affect the experiment from beginning to end.

The abductive reasoning that should and will penetrate the creation of a research plan can be summed up with the words "what is likely to be the outcome of this particular experiment (if it is structured like this)?" If a thorough experiment plan is constructed, the researcher should be able to predict outcome (form a hypothesis)<sup>169</sup> and/or be prepared for alternative practical trajectories and take necessary precautions. In either case, the research problem and subsequent conditions should always be carefully considered, so that the experiment will be feasible, and the results credible.<sup>170</sup>

### ***5.1.2.3. Preparations and pilot experiment***

Experiments should and will be separated into preparatory phases and actual experiment execution. This is sometimes mixed or presented imprecisely, which can cause confusion for others as to which question is addressed. If this happens, it is often due to preparatory manufacture or other technological practical aspects introduced as part of the experiment. Nevertheless, preparations should be just that: that which prepares the experiment for an exploration of the research question, and this should be according to the research plan. Preparatory work consists of a variety of tasks, from administrative to practical. Typical are risk assessments and necessary permissions, procurement of raw material, skill generation, manufacture of necessary implements, choice of personnel, ensuring the team are sufficiently skilled and available, securing funding and equipment, and setting up of the

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169. In this instance, "hypothesis" does not entail a deductive test, but rather the initial interpretation of what the outcome will be.

170. See section 2.3.

site. However, actual preparations for experiments are not typical; rather they vary completely with the questions that are being asked.

Once a researcher gets a closer relationship to the technology or material in question, certain aspects of the intended research question may be re-evaluated and changed. It is also often a beneficial part of the preparations to execute a pilot experiment, to test equipment, gauge the capabilities of experimenters, and to make sure that the chosen set-up is feasible. This phase should prepare the researcher for eventualities that may occur. The pilot experiment includes evaluations of all the tested aspects and should introduce changes accordingly, which may provide a further specification of the research question (e.g. Comis 2010; Liedgren and Östlund 2011: 906).<sup>171</sup> In certain instances, such as in section 7.2.6., the pilot will reject the research question altogether and render the experiment redundant. When this happens, it can sometimes be used as grounds for archaeological interpretation, but more commonly, it will cause a re-structuration of the experimental process. The pilot phase can be crucial to save both time and resources, so that the subsequent experiment is carried out without significant complications. All the experiments iterated in chapters 6 to 8 were constructed to include pilot experiments, which are considered preparatory, not part of the actual experiment.

Not all experiments will have the opportunity for pilot experiments, for instance if certain equipment is only available for the actual experimentation. However, rarely any instances would not benefit from a pilot experiment, and so, if feasible, it should and will be considered a necessary step in experimental preparation for the present method evaluation. If a pilot experiment is excluded for any reason; conceptual, budgetary or practical, it should be considered whether the experiment is well enough prepared to produce relevant results that will be accepted by the experimental research community.

Sometimes a full-scale pilot experiment is not achievable, but it would still be possible to either scale it down or run a preparatory test of certain parts of the experiment, as was done with the Sea Stallion ship and testing of two models in

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171. However, certain authors believe that the pilot experiment should be completely free in form (Thér 2004: 39).

cardboard (1:10) and wood (1:10) respectively (Nielsen 2011: 65). This is in fact common in maritime experiments, where models are frequently tested in hydrodynamic towing tanks beforehand, as an indication of the reconstructed structure (e.g. Werenskiold 1989; Bischoff 2012). Such tests are generally considered good indications for the sailing properties and later ship trials that form the actual sailing experiments, and this set-up can be used as a model for possible solutions to other experiments in which pilot experiments are not a feasible part of the preparations.

#### **5.1.2.4. Experiment execution**

The experiment itself will for the purpose of this thesis be used to gain a variety of results, thereby

- Identification of physical process (e.g. object experiments)
- Interpretation of microscale context of process (e.g. behaviour experiments)
- Connection of physical process and specific human activity (e.g. behaviour and process experiments)
- Identification of next, unidentified step of process by inference.

To provide for a broad spectrum of evaluation, the case experiments in Chapter 6 to Chapter 8 will illustrate a spread of these formats. The executions will cater for the evaluative aspect of the overarching analysis by the creation of specific research questions that assumed to relate to non-tangible factors. If conclusive, the results will address whether an interpretation of idea, notion, action, consideration, agency, or other relevant factor be made with the respective information available.

The practical, experimental chaîne opératoire will follow the below protocol step by step:

1. Evaluation of material end product: current status
  - a) Study archaeological specimen to the necessary extent in relation to relevant parameters for the experiment to follow.
  - b) Place in categorisation scheme: decide on experiment form and level of evidence.

2. Experiment plan: decide on format of experimentation
  - a) Formation of research aim, research questions (and hypothesis).
  - b) Identification and planning of experimental format
  - c) Practical planning (logistics, time, place, material preparation, etc...)
3. Manufacture stage if relevant
4. Pilot experiment.
  - a) Test of equipment, experiment format and/or set-up, materials, experimenters and other experimental properties.
5. Experiment and transitory/feedback interpretation
  - a) Specific experiment chaîne opératoire
  - b) Evaluation of feedback information under the duration of the experiment
6. Post experiment: situate physical procedure
  - a) Socially
  - b) Spatially
  - c) Ecologically
  - d) Other
7. Experiment evaluation (singular researcher or team):
  - a) Place experiment in categorisation chart post experiment.
  - b) Are there identifiable biases in the experiment?
  - c) Could those biases have been avoided by restructuring the experiment?
  - d) Could other features of the experiment have been improved by a reconsideration of aspects?
  - e) What is the final interpretation of the experiment results?
  - f) Do the results indicate intangible features of the society in question?
  - g) Do other parts of the experimental work yield relevant results?
  - h) Has the total evidence level increased according to the degree of evidence gauge?
  - i) How will the experiment and its results be disseminated?

After the experiment and its interpretation, the experimental results must be connected to the larger discourse on the topic, whether the society, region, period, object category, or other where the experimental results provide relevant input. Most experiments build on an already established storyline, and it is important to note that the results achieved are only a part of the history (Schmidt 2005a). Because of this, experimental results bear little value in isolation, and their dissemination that is so sought after (e.g. Outram 2005; Preysler et al. 2014; Schmidt 2005b; Sørensen and O'Sullivan 2014; Paardekooper and Flores 2014) may bear little meaning if only to present a detached image of a brief moment. This is, as mentioned earlier,<sup>172</sup> expected to be the weakest link in the experimental archaeological discourse of the intangible – or the lack thereof. Part of the experimental protocol for the case experiments will be to insert the experiment results into the larger storyline and discuss their significance. This evaluation will investigate the inference of the experiments, their context and significance for the larger interpretation of the cultural history in question. The inference should take the form of abduction rather than to attempt other formats. The inference should be something like the following:

<i>Experiment results</i>	Premise
<i>Other findings</i>	Premise
<hr/>	
<i>Cultural historical storyline</i>	Inference
<i>Have the experimental results broadened the storyline?</i>	Evaluation

As part of a pragmatic approach, the experimental work in this thesis will be supplied by other forms of data for the analysis, or suggest alternative entry points to take the interpretation further or direct it laterally to increase likelihood and thereby strengthen the inference. After all the experiments have been detailed, the entire experimental methodology will be evaluated in the discussion. It is the aspiration of the author that the methodology just presented will work to its full potential and provide good grounds for an *honest* evaluation

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172. See Chapter # for a discussion.

of experimental archaeology as a tool to understand intangible aspects of past technological practices; all the while still remaining within the bounds of a typified experiment.







# Part II – Reconstruction



In this part of the thesis, the theories of Part I will be put to practice. Thus far, Experimental archaeology has proven itself to be largely practice oriented, and so this seems only logical. However, the reason for making this choice is not only due to the author's own logics, but also to two factors that are critical for this thesis and its research question:

1. There are numerous studies of experimental archaeology that deal with a theoretical analysis of this subfield (Grimaldi 2014, Kelterborn 2005, Nami 2010, Rasmussen 2001, 2007a, 2007b, Reynolds 1999) . Although all these studies come from experienced experimentalists, they generally speaking present ideas that are then concluded upon without a practical demonstration of its purposefulness. As experimental archaeology is an almost exclusively practical field, this seems counterproductive, and studies like these do seem to fall short of reaching a definite demonstration of why just their theories are viable. In the current study, the intention is to go beyond this theoretical, and hence abstracted, conclusion and perform a practical investigation as part of the study. The proposed methodology in Chapter 5 will be the outline for all the experiments to come in this reconstructive part. The intention is to examine whether the deconstructed, typified experiment is an obstacle for researching intangible aspects of technology. In the author's opinion, this can only really be decided upon by performing a number of presumably typified experiments. Without a practical segment, it seems unlikely that a reasonable conclusion can be made about the potential to investigate the intangible with archaeological experiments.

2. The intangible nature of that which is under scrutiny is another reason for including a practical segment in this study. Intangible aspects of technology, or other facets of society, are constantly under scrutiny in archaeology. However, such analyses largely remain on the theoretical plane (Barndon 2012, Fredriksen 2006, Jones 2002). As there is nothing holding experimental archaeologists back from partaking in such theoretical analyses as part of their experimental study, this seems to function less well in relation to this field. Therefore, it is assumed that a practical foundation will provide an even stronger argument for or against intangible aspects as part of experimental studies and research questions.

In the following, the practical part will be presented in the form of three case experiments. If theoretical analyses of experimental archaeology do include practical segments, more often than not the number of experiments is limited, and often only consist of a single experiment (Foulds 2013b, Mathieu 2002, Purri and Scarcella 2011). However, for an analysis of an entire methodology, this seems insufficient, and so for the present thesis it was determined to do three practical studies. The number of experiments would ensure that the conclusion did not hinge on a yes and no result, all the while being able to provide a spread of technologies and periods.

The following experiments are widely dispersed in time (c. 500 CE – 4000 BCE – 250 000 BP). This was a conscious choice, thought to ensure that period bias would not play into the typification of the experimental process. In the same vein, the spread of technologies was chosen to be separated by difference in basal procedure (shaping – applying force by hand – constructing a structure), although two experiments were related to pyrotechnics. The raw materials were different, although two experiments could be said to relate to lithics (soapstone and granite). Further similarities include chemical transformations (ceramics and tar procurement), methodology (microscopy), and the predominant choice of field mode. However, some similarity must be expected when one researcher is in charge of all experiments, and the dispersal of technologies, time periods, procedures, and materials was kept to the maximum that the author was able to perform. This is in itself a nudge to hermeneutics, in the sense that one mind can only contain variations of previous topics.<sup>173</sup> Over the following three chapters, the experiments will be presented with a specific view to the categorisation chart in Table 3. These experiments are the practical input to the research question presented in this thesis: Can experimental archaeology be successful in investigating questions of an intangible scope, and why (not)? After the experiments, the results will be amalgamated in the discussion of Chapter 9. The discussion will not only process the practical results, but also draw in elements that have showed prominence in Part I, in order to bring the entire topic of intangible research questions in experimental archaeology together.

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173. See p. 22.

## 6. Technological processes, meaning, and Bucket-shaped pots

The previous part of this thesis has analysed the conceptual realm of experimental archaeology. However, experimental archaeology is first and foremost an archaeology of the practical. An analysis can therefore not be complete without an extensive practical element. In the following, three case studies conducted with the exploration of the intangible as research aim will be presented to complement the various theoretical discussions in Part I. The experiments have been chosen to represent three different time periods, three different technologies, and three different types of research question, **in an attempt to represent the diversity of experimental archaeology**. In this way, the case experiments are intended to provide three different manners in which to approach the intangible in the technology.

### 6.1. Bucket-shaped pots in space and time

Bucket-shaped pots have long been studied in Norwegian Archaeology as a diagnostic feature of the Norwegian Migration Period (c. 400-560). Although the pots are open to both chronological and regional variation, their uniformity is striking: flower-pot sized, straight-walled or slightly convex small vessels, typically with a P-shaped rim and a capacity of approximately 1-1,5 litres; densely decorated in highly variable decorative schemes, and eventually with remnants or indications of an iron band. In Scandinavia, Bucket-shaped pots form an unmistakable type that does not seem to relate to any known ceramic type before their rapid appearance, and they are clearly distinctive from contemporary types (Figure 5).<sup>174</sup> Equally striking is their abrupt disappearance from the archaeological record around the end of the Migration Period; marking the start of a 1200 year absence of Norwegian ceramic production (Kristoffersen and Magnus 2010: 10).

One of the most significant features of Bucket-shaped pots, and the main problem addressed in this chapter, is their extraordinarily high proportion of non-

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174. Although this has been disputed in relation to the earlier Asbestos Ware (Jørgensen 1988).

plastic inclusions – often up to 80 %, and possibly 90 % of predominantly, asbestos, soapstone, or a mix of the two (Kleppe and Rueslatten 1993; Kleppe and Simonsen 1983: 18). Why this high proportion was deemed necessary is currently unknown, and few other types of Norwegian ceramics display such high non-plastic to plastic constituents ratio. As will become apparent below, this has been summarily discussed, but few authors have chosen to explore this ratio in any detail. This chapter will therefore discuss various aspects of why pots may have been made of more non-plastic than plastic constituents in the Norwegian Migration Period.

The Migration Period was strongly affected by the massive movements of people around the continent, and in continental Germanic cultural influence is visible in Norwegian material (Solberg 2000). However, there was less movement of the population on the western part of Scandinavian peninsula, which was at that time divided into small fiefdoms mentioned amongst others by Jordanes in *De origine actibusque getarum*<sup>175</sup> (551) (Hedeager and Tvarnø 2001: 267f). The fiefdoms are currently interpreted to be founded on a warrior culture, reliant on loyalty and alliances, influenced by the general Germanic societal structure and with farming and fishing as main subsistence activities.

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175. The Origin and Deeds of Goths; also known as *Getica*



Figure 5: From left: Handled vessel S2652a, burial context, Helliesen, Sola. Bucket-shaped pot S2720a, soapstone spinning wheel S2720g, Handled vessel S2720b, burial context, Helliesen, Sola. Source: AM/UiS.

The lack of displacement is indicated in the continuity of a high number of aspects of material culture and settlement patterns, such as another ceramic form; "hankekar",<sup>176</sup> a pot with a well incorporated paste, thin walls, and a clay-to-temper ratio around 15-25 % (Hulthén 1985; Hulthén 1986). Curiously, Handled Vessels are of approximately the same capacity as the Bucket-shaped pots. Together, they have been proposed as a food and drink set (Engevik 2002: 66).

Bucket-shaped pots were in use across fiefdoms and the related chaîne opératoire existed in most of Norway and in parts of Sweden, however chiefly in coastal areas (Jørgensen 1988: Fig 5.; Kristoffersen and Magnus 2010: 9). The pots form a highly distinctive vessel type due their stylistic scheme (Figure 6) in combination with the extraordinary proportion of temper,<sup>177</sup> largely either finely grated asbestos or ground soapstone.<sup>178</sup> Both materials are used in the entire region, also in combination, but there is a certain trend towards asbestos in the Western regions while soapstone dominates in the Southern ceramics

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176. Translation: "Handled Vessels"

177. For reasons of traditional terminology, 'temper' will here be defined as non-plastic constituents in paste recipes, but should not automatically be seen as a stabilising agent.

178. Asbestos is a group of minerals largely consisting of serpentine and amphibole. Soapstone is common in Norway, and is rich in talc, as well as chlorite, serpentine, and magnesite. *Geoleksi*, online geological encyclopedia from the University of Oslo. [www.nhm.uio.no/fakta/geologi/geoleksi](http://www.nhm.uio.no/fakta/geologi/geoleksi)



(Kristoffersen and Magnus 2010: 54). Some vessels contain sand (Hulthén 1986; Kleppe and Rueslatten 1993). Mica is suggested as an added decorative element, but it is unknown whether this is a result of archaeometric/petrographic analysis (Kristoffersen and Magnus 2010: 59).<sup>179</sup>

All these mineral types are fairly common in Norway and often encountered in archaeological ceramics. However, especially in Rogaland county, in the South-West of Norway, there is a move towards regionally specific instead of generalised paste recipes, (Fredriksen et al. 2014: 127). The especially high proportions of asbestos is also found in earlier Asbestos Ware from Fennoscandia, which in some instances yielded 95 % asbestos and 5 % clay (Hulthén 1985), and certain authors have maintained that this kind of ceramic bears resemblance to Bucket-shaped pots (Jørgensen 1988). Despite the uncertainty surrounding datings and what seems like random difference in material proportions, this may be the case for the asbestos matrix ware (Engevik 2002: 51-52). Nonetheless, there seem to be few that hold similar proportions of soapstone, and so in this regard, the Bucket-shaped pots must be considered unique.

Temper or non-plastic additions to pottery can have many functions and is traditionally discussed in relation to shaping, firing, thermal stress, and temperature resistance (Hulthén 2011a; Orton et al. 1993). Thin sections of Bucket-shaped pots have shown that firing temperatures varied, and sometimes were kept as low as c. 500 °C – probably just above sintering. This was not unusual for preceding pottery in some regions (Hulthén 1985; Hulthén 1986). Even so, coupled with especially thin walls; between 3 and 8 mm, the stylistic and normative form followed a different scheme and a new chaîne opératoire, compared to other Scandinavian ceramics both before and after the Migration Period. Astonishingly for Scandinavian ceramics, the pots also show signs of

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179. Older catalogue entries rarely mention soapstone, but very often mica, as a tempering agent. However, it must be assumed that this was not always petrologically established, and that the wording is largely the consequence of archaeological praxis. Upon study of the microscopic images of the archaeological pieces, only a few sherds of the highly characteristic mica crystal could be seen (pers. comm. Carlos Salgado-Ceballos, 15/7/15) The word for mica in Norwegian is "glimmer," indicating the shimmering properties of the mineral. As will become clear later in this analysis, this is also one of the properties of soapstone when exposed to heat.

repair; with or without metal pins (Kristoffersen and Magnus 2010: 10).<sup>180</sup> Together, this results in a pot that deviates from most other pottery patterns in Norwegian prehistory,<sup>181</sup> apart from its basic pot shape. The deviation does not appear to result from significant entries and exits of new population groups, and is rather assumed to be related to a change in regional customs and concepts; coinciding with the appearance of a strong leader class and early state formations (Rødsrud 2012: 128).

A social intimacy has lately been suggested between metallurgists and ceramicists of the time, involving goldsmiths especially. This is highlighted by new archaeometric analyses, which reveal a presence of gold and other metals in certain Bucket-shaped pots that fits a pattern of stylistic correlation and practices such as mending. This relationship becomes especially prominent towards the end-phase of the Migration Period, when elements such as iron bands are introduced (Fredriksen et al. 2014).



Figure 6: Bucket-shaped pot C5872, Aak, Møre og Romsdal, Norway. Source: KHM

Previous research on Bucket-shaped pots has largely followed typological (Bøe 1931; Engevik 2008; Kristoffersen and Magnus 2010; Shetelig 1905) and petrographic (Hulthén 1985; Kleppe and Rueslatten 1993; Kleppe and Simonsen 1983; Magnus 1980) trends, however increasingly focussed on

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180. For instance S1440, S5403, S6396

181. Apart from possibly the preceding (but not overlapping) Northern Asbestos Ware (Jørgensen 1988)

technological traits such as ware composition and manufacture. As the majority of the Bucket-shaped pots were retrieved from burials, research has lately turned from the tangible to the intangible, in the form of the cosmology of the contemporary society, and how the pots may fit into belief systems and worldviews. The pots appear in nearly every grave in certain regions, appearing to be an important metaphor for the contemporary notions of propriety and symbolism (Fredriksen 2006: 126; Kristoffersen and Magnus 2010: 10; Rødsrud 2012: Chap 6). However, they are also found in various other site contexts, such as settlement sites, boathouses, caves and rock shelters (Fredriksen et al. 2014: Note 2). Possibly as a result of this new cosmological perspective, practical function of the pottery is frequently inferred, but not investigated from an empirical perspective. It may be that Bucket-shaped pots were primarily for symbolic, rather than purely utilitarian functions, such as is the case with a lot of for instance finer tableware today. Nonetheless, this does not in itself rule out a utilitarian function, especially considering its highly practical container shape much like the finest porcelain crockery today, which is both socially and practically motivated (and see Larsson and Graner 2010).

Experimental literature on Bucket-shaped pots is scarce, but Johannes Bøe (1931) performed experiments to shed light on the production process from shaping to firing temperatures. It is unclear whether his conclusion on the technical procedure is linked to the experimental work, but he maintains that potters that produced these vessels were not specialists, and that the high proportions of asbestos/soapstone in Bucket-shaped pots were generally linked to stabilising properties, to maintain the pots' delicate shape during drying and heat-treatment. Bøe also states that the fine-grained minerals would have had enough plasticity on their own, and that clay was probably not really necessary (*Ibid.*: 170-171, 204f). Bøe's experiments were not specific to type nor period of Iron Age ceramics, and the results are severely biased by references to modern ceramics and general speculation disregarding situated contextuality. Due to the lack of necessary actualistic focus, they cannot be given any significance from an experimental point of view other than their early date in the history of

experiments.<sup>182</sup> Bøe's hypothesis about stabilisation under production is a normal feature for any tempered ceramic, and will be achieved at 20-30 %. Nevertheless, the added material will usually only provide this benefit when the paste is unfired. Almost any temper will weaken the pot upon firing due to an interruption of tight bonds between clay crystals, and because the slight expansion of clay will cause an additional increase in porosity during firing up to 800 °C (Gibson and Woods 1997: 30-31). Bøe's conclusion therefore needs further investigation before it can be accepted.

The temper of Bucket-shaped pots and its relation to heat is repeated by several authors and has grown to become one of the more significant interpretations relating to the function of the pottery. As crusted protein has been found inside the pots, they are often interpreted as food related vessels (Engevik 2002; Fredriksen et al. 2014: 127; Kleppe and Simonsen 1983: 16; Kristoffersen and Magnus 2010: 10, 15).<sup>183</sup> Although likely used in relation to foods, the utilisation of Bucket-shaped pots for *cooking* is, nevertheless, rarely considered. Asbjørn Engevik points to the rich decoration, flat bottom and small volume, and suggests that they are tableware rather than cooking vessels (Engevik 2002: 65). Siv Kristoffersen and Bente Magnus (2010: 10) put forward the argument that the temper is connected to heat-retaining, rather than heat-resistant properties. However, the grounds for their statement is unspecified, so it is not clear whether they refer to the asbestos, the soapstone, the proportion of non-clay mineral, or the clay itself. The same argument is used by others in relation to asbestos-tempered pottery specifically (Hulthén 1985: 335; Jørgensen 1988: 61; Núñez and Okkonen 2005), also unsubstantiated. Milton Núñez and Jari Okkonen highlight that Ostrobotnian Neolithic Pöljä type pottery, which is tempered with 50-60 % asbestos, is especially thin-walled (6 mm) compared to its counterparts which were *not*, and that the asbestos

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182. The experiments were performed by Bøe himself, who readily admitted his lack of skill, and only consisted of very few pots that were tempered with chamotte grog at a very low ratio compared to Bucket-shaped pots. Chamotte has been shown to barely be present in Scandinavian Iron Age pottery (Hulthén 1985). Bøe speculates that this is irrelevant and does not voice any concerns for the actuality of a blend of 24 % chamotte in reference to iron age pottery that were analysed to contain between 37 and 83 % crushed mineral temper. The pots were manufactured with pinch-technique, although Bucket-shaped pots were moulded, and other contemporary pottery usually coiled. In addition, firing was executed in a modern electric kiln (Bøe 1931: 204-213).

183. And see Ts 7245ee.

vessels would therefore not be comparatively heat-retaining (Núñez and Okkonen 2005). Bucket-shaped pots are generally thin-walled, and although Handled Vessels and other contemporaneous types are not particularly thick-walled, the clay matrix of Handled Vessels is very well incorporated with less temper.<sup>184</sup> Even so, Bucket-shaped pots are seemingly of a slightly more 'delicate' type. However, fire clouds are seen on the latter (Figure 7), and upon investigation, some sherds displayed fire clouds, as well as interior crusting, possibly from food.<sup>185</sup>



Figure 7: S6396, Steinsnes, Rogaland, Norway. Note the clear fireclouds on this vessel. Photo: Terje Tveit/AM/UiS

Contrary to arguments concerning a lack of heat conductive features of asbestos temper (Hulthén 1991: 16), experiments have shown that asbestos tempered ceramics with up to 50 % asbestos<sup>186</sup> are well suited for cooking purposes (Sundquist 2000). Soapstone was used to make cooking vessels in most of the Iron and Middle Ages,<sup>187</sup> as indicated by food crusts from soapstone bowls. Result of analysis of the crusts shows that contents were occasionally

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184. This became apparent upon the author's investigation of specimens found together with Bucket-shaped pots, e.g. C18425a, C18814b, C18839a, C18842a, C18863.

185. C19363 and C12321a

186. Higher proportions than 50 % were not tested in this experiment.

187. One of the Norwegian names for soapstone is *grytstein*, which translates to "cooking pot stone."

heated to up to 300 °C<sup>188</sup> (Brodshaug and Solli 2006: 299f; Rødstrud 2007: 101). Such uses may indicate that cooking properties did not decline due to the high soapstone content in soapstone 'tempered' Bucket-shaped pots. Additionally, **certain pots may have been fired subsequent to production, in a secondary use situation (hereafter secondarily fired)** (Fredriksen 2006; Hulthén 1986), **which may** indicate a higher-temperature use for the type, such as funerary rites. Two pots from the Kvasshheim cemetery, Rogaland, Norway, were shown to have been fired to 700-760 °C, which would be in line with this interpretation (Kleppe and Rueslatten 1993: 298). In sum, the utilitarian question of the potential functionality of the temper proportion in relation to heat is still unanswered, however much inferred by the discourse.

Bucket-shaped pots are named after their cylindrical form. Several finds have indicated the production method by displaying a significant slab overlap in the form of a join on one side, and traces of the addition of a bottom slab.<sup>189</sup> Exploratory experiments into the production of Bucket-shaped pots by Else J. Kleppe and Stein Em. Simonsen (1983) concluded with a tentative manufacture technique, although the (publication of) experiments lack alternative methods or other comparative features. Neither was a firing reported for the manufacture.<sup>190</sup> This slab-on-mould technique is heavily cited as the production scheme for Bucket-shaped pots (e.g. Rødstrud 2012: 324; Kristoffersen and Magnus 2010: 10; Fredriksen et al. 2014: 123; Engevik 2002: 49), although several aspects have not been sufficiently investigated, for instance the functionality of this production technique with a high proportion of asbestos and/or soapstone, as the experiments relied a modern chamotte mix with a temper proportion of 30 %, and no other added temper, in addition to natural clay without added temper (pers. comm., Stein Em. Simonsen, January 2015), hence yielding substantially higher plasticity than the archaeological specimens with up to 80-90 % non-plastic inclusions.

Raised displacement ridges along the incised or impressed decorations were

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188. Soapstone vessel Ts 11179.30

189. E.g. B10890, B10893

190. However, it was later revealed by one of the authors that the firing was performed as an open bonfire-firing in a shallow ditch with diameter 1,5 metres (pers. comm., Stein Em. Simonsen, January 2015).

not observed by the present author, thus the pottery samples selected for investigation were most likely decorated in a leather-hard state. The notion that decoration was executed while the pot was still on the mould (Kleppe and Simonsen 1983: 32) should therefore be kept open for discussion. In a leather-hard state, the pot has already undergone sufficient shrinkage for a prediction of the final size of both pot and the pattern. At the same time, it will still retain enough moisture to affix additional parts such as handles or bulbs (Gibson and Woods 1997: 44). Additionally, if clay is still wet, it will give way under very little pressure, and is therefore very prone to erratic slashes and incisions of various depth. Most pottery is therefore decorated in the leather-hard state. In this state, the clay will yield to impressions without the formation of 'wet' raised ridges (which are apparent in the experimental photos in Kleppe and Simonsen 1983: Fig. 27, Fig. 28). Neither erroneous or 'wet' impressions were observed in the sample archaeological material examined for the present analysis. It also became clear that a proportion of the sample pots/sherds had been burnished before incised patterns were added, previously also noted by Birgitta Hulthén (Hulthén 1986). The same was performed on Handled Vessels (e.g. Stout and Hurst 1985). Burnishing can only effectively happen in a (close to) leather-hard state, as the clay can then be easily compacted to form a dense, even surface. Upon firing, well-burnished surfaces will reflect light and give the pot a shiny appearance. 'Burnishing' in a wet state would not achieve any sheen – if any less than approximately leather-hard, the clay would have been lifted off by the burnishing implement and caused duller areas to form (Gibson and Woods 1997: 45). Altogether, the lack of 'wet', raised ridges, erroneous slashes and the presence of burnish, point to a leather-hard state of decoration for Bucket-shaped pots. It is unlikely that this was achieved unless the pot was first removed from the mould, as the paste would normally otherwise shrink around the mould and crack during drying. Whether it was put back on a mould for decoration could not be inferred from the pots.



Figure 8: Burnished, decorated and reduction fired sherd of Bucket-shaped pot C12321a, Hafsøy, Rogaland, Norway.

Although Bucket-shaped pots have been manufactured by various modern potters for use in museum contexts, information on production techniques has not been systematically assembled.<sup>191</sup> Additionally, little experimentation seems to have been done with regards to the technological aspects of the addition of non-clay material, and its remarkably high proportion. The question remains why a pot would largely consist of something other than clay to the extent that "temper" is not a suitable term. As proposed by Bøe, there may be a link to production properties, specifically related to firing. However, as the properties he suggested are normally achieved by substantially lower proportions of added material, this question must be considered unresolved. The connection to firing is also investigated by others (Fredriksen 2006; Fredriksen et al. 2014), but until

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191. The author has observed that some pots meant for public presentation have been wheel-thrown, a technique that was never discerned in an archaeological specimen, and which only appeared in Norway in the Middle Ages (1000-1500).



now, not through archaeological experiments. However, this problem must be considered to be very suitable for experimentation, as practical experiments will be able to add perspectives to the chaîne opératoire that cannot otherwise be addressed.

As stated in section 4.3., this thesis relies on the use of the object-focussed chaîne opératoire as a key point of departure for experimental work. Apart from a high level of knowledge about the discard of Bucket-shaped pots in burials, most of the chaîne opératoire of the pots is at current still unanswered. To experiment with a later stage of the chaîne opératoire is to create a process experiment as discussed in the experiment categorisation scheme for this project.<sup>192</sup> According to Mathieu (2002b: 6), this should be approached from lower level experiments to avoid putting "the cart before the horse." Many open questions relate to the temper proportion, for instance the very current discussion of the element of heat (e.g. Fredriksen et al. 2014). In the following, a further understanding of heat and proportions of added material in Bucket-shaped pottery will be explored from the start of the chaîne opératoire through experiments with the very creation of ceramics through firing.

## **6.2. Case experiment: soapstone Bucket-shaped pottery and firing**

### **6.2.1. Experimenting with intentions and paste recipes**

Pottery is often studied from experimental perspectives, from ethnographic (Gosselain and Livingstone-Smith 1995; Schiffer et al. 1994) to reconstructive (Hammersmith 2011) to scientific (Stoner et al. 2014). Although most concern the technological process, the experiments are usually not performed with a definition of what technology is, and so either relies on a finite or infinite view of what technology is. In this thesis, technology is not defined beyond the social entangled context of its existence.<sup>193</sup> However, an open-ended description is provided in Figure 3, and will form the basis for the experimentation in this part of the thesis.

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192. Table 3.

193. For a discussion of the lack of definition, see section 4.2.2.

The experiments regarding Bucket-shaped pots and its remarkable 'temper' proportions is built upon the categorisation in Figure 3, and will study one of the consequences<sup>194</sup> of technological practice: the intention; *why* something came to be part of the technological practice in the respective field. In the case of Bucket-shaped pots; the question becomes why such high soapstone proportions were considered appropriate in the creation of the clay paste recipe. If an interpretation can be reached, the experiments have contributed to a new understanding of the technology.<sup>195</sup> Furthermore, the intentions of enactors of a technology can contribute to an understanding of concepts of pots, and in which situations pots and related consumption were significant.

### **6.2.2. Interpretational chain: evaluation of the archaeological chaîne opératoire<sup>196</sup>**

It is likely that the new chaîne opératoire that occurred in the field of Migration Period pottery production in the form of a massive increase in the proportion of non-clay to clay, is linked to a change in normative ideas. Since Handled Vessels continue to exist, the chaîne opératoire is not necessarily new for the general notion of 'pottery.' Nevertheless, something is new; the idea of what a pot can be has changed, and quite abruptly so. Where the idea came from, can be difficult to address. However, it is still a significant part of archaeology to attempt to understand *why* a certain concept, such as the use of materials, was changed, and to achieve this in experimental archaeology, we often address its (potential) practical purpose.

To approach the paste constituency of Bucket-shaped pots in relation to heat, it is important to acknowledge that ceramics endure several forms of heat-treatment, and that "heat" spans a wide range of temperatures. For instance, a pot is often heated to 100 °C prior to firing, to release chemically bonded water trapped between clay crystal platelets; so-called 'water-smoking.' This ensures

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194. This relates to the consequences discussed under section 3.4.1.

195. Whether our interpretation is what *actually* happened is not relevant to this thesis. However, whether *we* consider the likelihood of the interpretation increased or even the best inference (available), will be. This will be debated in the discussion in Chapter 9.

196. P. 136.

a fully dry pot which is more stable and has less chance of cracking, spalling, or exploding during firing (Gibson and Woods 1997: 47). The actual firing can be both primary to fire the vessel to a ceramic state, and secondary for other purposes, such as the firing of glazes.

After the production is finished, a high number of pots go through tertiary heat exposures, for instance through cooking or as aid in technological operations, such as birch bark tar production (Regert et al. 2003a), beer brewing (Hulthén 2011b), or dyeing of textile fibres (Koh et al. 2015). Ultimately, many Bucket-shaped pots are thought to have been submitted to a cremation pyre (Fredriksen 2006: 134; Rødsrud 2012: 233). Temper will benefit the use of the pot in all these situations, if the pot needs to stay intact during and after the procedures. That the addition of temper was intended can be abductively inferred for all the situations in which something was needed to remain contained within the intact vessel. Of the procedures mentioned above, only cremation can be highly functional even without container properties.

The constituents of a pot may also have undergone heat exposure before they were aggregated into one shape. For instance, tempers may have been heat-treated for stabilisation or to aid fragmentation, such as when a hard temper like the archaeologically common granite is used (Hulthén 1986; Hulthén 2011a). Additionally, when chamotte is added, temper from previously fired ceramics is integrated into the new form. The current discourse does not mention chamotte in relation to Bucket-shaped pots. Rather, it seems quite established that asbestos and soapstone are the common tempers (e.g. Engevik 2002: 49). Whether or not these materials were heat-treated before integration into the pot has not been specifically investigated.

The change in a ceramic chaîne opératoire is likely to be explained by new normative notions, either stylistic or functional. To speak of function in pottery is often considered to encompass only day-to-day tasks, and function is readily considered as something else than style. As Bourdieu (1984) has stated, a practice such as doing something 'right', can be an expression of taste, and therefore stylistic and behavioural norms are often interlinked. It is therefore important to note that intended functionality can be related to practices both

within and outside of the everyday household, such as the explosion of varied burial customs that appear in the Migration Period (Fredriksen 2006); or perhaps new norms for appropriate cooking or food presentation (Engevik 2002; Schenck 2010; Velasquez 2013); or countless other functionality aspects. Either of these could be related to heat, or heat could not be a factor. Currently, we know that some of the vessels have held foods,<sup>197</sup> and that some pots in certain regions have been secondarily burnt (Hulthén 1986: 73), presumably on a funerary pyre. Additionally, there is a whole spectrum of symbolic functionality that is currently assumed through the rich decoration, the apparent practice of personalising the pots by depositing them individually in graves, as well as through their potential connection to a new social order including new aspects of commensality (Engevik 2008; Fredriksen 2006; Kristoffersen and Magnus 2010). We do not currently know which function(s) the pots had. But we do know that the pots consist of extraordinary amounts of non-clay minerals, and that such proportions of temper in pottery are unusual, both in Norway and in most of the world. We do not know if this is explicitly related to function, but it is a question that should be explored before further conclusions are made.

Following the discussion in chapters 2 and 5,<sup>198</sup> a standardised experiment should have a primary archaeological reference which must be examined by a suitably skilled person. As all Bucket-shaped pots can determinately serve as a reference to certain technical properties, for instance related to ware and capacity, this criterion is achieved as long as the focus stays on these properties. However, apart from the sherds themselves, there is scarce archaeological evidence for the actual heat exposure that Bucket-shaped pots had to endure throughout their use-lives. Only two instances of potential firing structures have been discussed in the archaeological record, both supposedly consisting of pits lined with stone slabs. The first specimen, a pit lined with stone slabs, was discovered by school teacher Mehus at Austbø, Rogaland county. The only publication of said find was through a letter to the local newspaper.<sup>199</sup> The supposed firing structure site was later re-excavated, but the structure described ca 40 years prior could not be relocated, and there is not

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197. E.g. Ts 7245ee, S4343c, S6299

198. Summary in section 2.3.

199. Aftenbladet, 10/8/1933

enough information available from the second excavation to establish that the stone slabs were in fact part of a ceramic firing structure (Kleppe 1970).

The second find was made at the 1974-75 excavation of a site at Augland, Kristiansand. The site consisted of six building structures; four pits interpreted as clay reservoirs; 138 pits including graves, fireplaces, charcoal pits, cooking and refuse pits, and 14 pits interpreted as ceramic kiln pit structures. The supposed kiln pits were circular, 30-80 cm deep, with a diameter from 60 to 110 cm, with a flat bottom. They held fire cracked rocks, charcoal, and ceramic sherds, and lower phosphate levels than all other pits (Rolfsen 1980), which may indicate a difference in function possibly linked to lack of organic debris (Holliday and Gartner 2007). Seven had an internal, square chamber of ca 40 x 40 cm. Furthermore, the site yielded c. 55 000 sherds of various pot types from the Roman (0-400 CE) and Migration Periods, amongst them sherds of 80-90 Bucket-shaped pots. Sherds from soapstone vessels were also recovered (Rolfsen 1980). Analysis across the types recovered showed that all 42 sample sherds – including 4 samples of Bucket-shaped pots, had been fired under reducing circumstances between 600 and 700 °C. One Bucket-shaped pots sherd was secondarily fired in an oxidising fire between 900 and 1000 °C (Hulthén 1986: 73). The publication of the site was very limited, with no drawings or photos of the potential firing structures (see Rolfsen 1980), and so a deeper investigation into the firing technology was not possible without an examination of the site documentation. However, upon a thorough search in the current archives of the excavating organisation, the Museum of Cultural History in Oslo, none of the documentation could be found. It appears that an unidentified loaner had taken them out and not returned them, which means the documentation must currently be considered lost.

The examination of the artefacts formed the basis for the general experimental design, and took place over 2 weeks in June 2014, both macro- and microscopically, with a combination of an Oitez eScope DPM15 5 MP CMOS portable USB-microscope and a Firefly GT800 2 MP portable USB-microscope, as well as photography with Canon EOS 500D 60 mm and 18-55 mm zoom lenses. The investigation was performed on a sample of vessels from Southwest Norway, primarily constituted of clay mixed with what must be

assumed to be soapstone for lack of petrography.<sup>200</sup>



Figure 9: C18839b, Nærheim, Rogaland, Norway, with traces of black and red slip.

The author's analysis of selected samples from the Museum of Cultural History in Oslo showed that the sampled Bucket-shaped pots were decorated almost exclusively with incision or impressions. Additionally, several of the finds studied for this thesis were also adorned with burnish or with paint/slip<sup>201</sup> (Figure 9). A few pots displayed a distinct metallic sheen, which was macroscopically of a silvery appearance (Figure 10), but upon microscopic investigation also

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200. Some of the pots were noted with 'mica' temper according to catalogue entries, or with a 'mica'/asbestos blend. However, an analysis of entries reveals a notable generational shift in catalogue entries around 1960s, when the use of 'mica' is exchanged with 'soapstone.' That the vessels did not contain large amounts of mica was confirmed by a specialist (pers. comm. Carlos Salgado Ceballos, 14/7/2015). As no petrography had been undertaken for the sample vessels, they will be considered as *general* representatives for Bucket-shaped pots. For logistical reasons, it was not possible to examine the few sherds which have actually undergone petrography.

201. C18425b, C18814a, C18839b, C18842b, C18863.

displayed golden temper particles.<sup>202</sup>



Figure 10: C19363, Løland, Vest-Agder, with metallic sheen and interior crust.

Many of the investigated sherds<sup>203</sup> produced a so-called 'sandwich' morphology, consisting of a black core between oxidised surface margins. This type of wall structure can be indicative of incomplete burning of organic compounds in the clay, and is often seen as a result of low temperature in the firing process (Kaal et al. 2014; Maritan et al. 2006). Nonetheless, this is not an absolute indication, as black core wall structures may result from several combinations of clay paste and temperature (Nodari et al. 2004). Also, many pots have been interpreted as secondarily fired in burial (Fredriksen 2006; Rødsrud 2012).<sup>204</sup> The sandwich structure may indeed be a result of a primary reduced firing, for instance in a covered pit, with a (brief) secondary oxidised firing (Noghani and Emami 2014). Further analyses are therefore necessary before conclusions can be drawn as to the meaning of the black cores.

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202. E.g. C19363, C12321a

203. C14044, C18425, C18425b, C18429b, C18814a, C18842,

204. However, this argument is generally not tied to specific morphological features in the ware (see Fredriksen 2006; Rødsrud 2012).



All the sampled Bucket-shaped pot sherds displayed a high density of inclusions of non-clay material, presumably soapstone, although this has not been petrographically determined (Figure 11). This **interpretation** relies on previous analyses that have determined that soapstone was a prominent feature in Bucket-shaped pots (Kleppe and Rueslatten 1993; Kristoffersen and Magnus 2010: 10).



Figure 11: High density of inclusions, C18842a, Nærheim, Rogaland.

After all the necessary investigation of archaeological sources and other documentation was completed, the knowledge of the archaeological chaîne opératoire could be worked backwards from the present state of the sherds. The pre-experimental state of knowledge can be summarised as follows:

13) The pots were often deposited individually in graves.

12) Recovered pot sherds have also been located in settlement sites, bathhouses and rock shelters.

11) According to food crusts, some pots have been used for food-related tasks.

10) Some pots had handles attached for hanging, carrying, lifting, or for other practical or symbolic purposes.

9) Some pots have been secondarily fired in an oxidising fire.



8) The pots were fired under varying temperatures and environmental circumstances.

7) Presumably, some pots were further dried to bone-dry state for the application of decorative slips.<sup>205</sup>

6) The pots were sometimes burnished. Then most pots were decorated with a variety of tools, techniques and schemes.

5) Many pots were left to dry to a leather-hard state.

4) The pots were generally manufactured by a uniform, strictly patterned slab-on-mould technique.

3) The pots consisted of high quantities of soapstone, asbestos, and/or sometimes potentially other minerals,<sup>206</sup> and a smaller proportion of clay (Kleppe and Rueslatten 1993: 298-299).

2) The materials for the clay and temper mix were sourced and **subsequently** processed.

1) The pots were made according to a template scheme that was open to chronological and regional variation throughout the Migration Period (Engevik 2008; Kristoffersen and Magnus 2010).

Comparing this chain to the archaeological evidence, it becomes clear that we currently only hold artefactually grounded knowledge about certain stages in the chain: the composition and construction techniques until formation of the vessel are more or less established, together with some of the depositional

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205. C18814a, C18825

206. The temper is in most cases determined by macroscopic investigation, and there seems to be a clear partition between older catalogue entries that describe vessels tempered with "glimmer," the Norwegian word for glimmering mica particles, and younger entries that usually use soapstone denomination, only occasionally referring to glimmer, and often in addition to soapstone (e.g. S5662b, S5046g). Some specimen are listed with other tempers (e.g. S5560 with quartz), but it is unclear whether this has been geologically determined or otherwise analysed.

circumstances. An indirect association with food through macro-fossil remains as well as chemical analysis of lipids and other organic remains in handled vessels (Isaksson 2008) usually found in the same burial context, where the two occur as a set (Rødstrud 2012).<sup>207</sup> However, the food-related use of Bucket-shaped pots seems to be circumstantially inferred (Engevik 2002; Fredriksen 2006; Kristoffersen and Magnus 2010: 9), and the actual use of the pots is not yet known. This is an area that is often considered suitable for experimenting, but as mentioned on page 163, to do so would be a process experiment as per the current categorisation, which may benefit from lower-level experiments to avoid making inferences that are not well-founded (Mathieu 2002b: 6). In line with the chaîne opératoire approach, the isolation of the experiments below will instead follow the technological steps in chronological order, starting with **procurement and** manufacture, to base conclusions of temper use on a material understanding as close to the contemporary knowledge as possible. Additionally, to make statements without understanding previous technological steps and the limitations or possibilities that result, can be to make unnecessary uninformed statements, and may contradict the criterion of having the necessary skills for the task prescribed by the discourse.<sup>208</sup> Naturally, an actual contemporary understanding cannot be expected to result. However, it is likely that the contemporary use and/or conceptualisation of Bucket-shaped pots were interlinked with the new chaîne opératoire, and to approach a contextual understanding of the pots, an understanding of the technological chain is in this project considered foundational. By piecing together the chain, it should theoretically be possible to get a glimpse of intangible circumstances that may inform on the problem.

Because no prior experimentation has been performed (or published) to back up Bøe's statements that the high quantity of added material was connected to manufacture and heat tolerance, the latter because the added minerals were expected to not expand significantly during heat exposure (Bøe 1931: 170), the aspect of heat-treatment can be considered the next step in an experimental chain that started with Bøe's and Kleppe/Simonsen's (1983) ventures into the manufacture of Bucket-shaped pots. To understand the role of the unusual

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207. Performed by Fourier Transform Infrared Spectroscopy and GC/MS.

208. Summarised in section 2.3.

quantities of non-clayey materials in the paste recipe, the present research question will centre on the proportions of such in relation to firing. Because this is a complex experimental research question on its own, the current experiment is focussed on only one non-clay material; soapstone. One way to assess this in relation to function is to perform a comparative experiment. Because the general quantity of temper in previous and other contemporary pottery was considerably lower (e.g. Hulthén 1986), the proportions of the paste recipe becomes an important question to address to understand the technological choices made in the field of Bucket-shaped pottery production. To address the proportions specifically, it was decided to work from a research question that applies to observable dissimilarities between ceramics fired with different quantities of soapstone. In light of these findings, it would hopefully be possible to take the discussion of the role of Bucket-shaped pots further, and to discuss how intangible aspects such as conceptualisation of 'pot' played into this technology.

### 6.2.3. Preparations

The first step in the preparatory work was the experiment plan, including a design. It was determined that the experimental vessels would need to be actualistic in capacity, wall thickness, wall irregularity (decoration), soapstone, particle size, and manufacture technique, and so it was decided that the vessels would be manufactured over a flowerpot mould of an appropriate shape and size. It was also decided that since the experiments were neither geochemical nor reconstructive in purpose or aim, the clay type would not be of **any functional relevance in relation to the heat transformation of soapstone**, as long as the soapstone was of the correct type. Furthermore, the clay in Bucket-shaped pots may stem from local sourcing of smaller deposits of marine clays (Kleppe and Simonsen 1983). Because of this, the clay was a standard raw, unprocessed and powdered red marl.<sup>209</sup> The soapstone, however, was sourced by Espen Kutschera, Hordamuseet, in a region of Western Norway that has produced a large amount of Bucket-shaped pots, including specimens tempered

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209. *Red Earthenware Powdered Clay 1135/2, Etrurian (UK), bought from Bath Potters' Supplies. Recommended firing range: 1050 – 1170 °C.*

with soapstone.<sup>210</sup> The soapstone was therefore considered actualistic to the necessary degree. However, it was not petrographically analysed, which was deemed unnecessary for the present experiments.

It was decided to compare quantities of 25 %, 50 % and 75 % (volume) soapstone addition to clay in a number of temperature relevant experiments. A field experiment was chosen to be accompanied by a subsequent controlled experiment, as it was considered that a field experiment would better address potential functionality issues in a firing situation, and should therefore be executed first. Both actualistic vessels and handmade test-tiles would be subjected to the same firing circumstances and then compared. The specific firing experiments were

3-1 Open bonfire-firing with a pilot experiment, including pots and test-tiles

3-2 Three different kiln firings at specific temperature thresholds, including pots and test-tiles.

The research aim was to investigate if there is observable difference between vessels with different proportions of added material, representing the substantial change in the chaîne opératoire relating to temper and inclusions. All experiments were placed in the object/manufacture category of the categorisation scheme, with associated evidence level for technological procedure (T), but direct evidence for physical evidence (E) and context (C).<sup>211</sup>

The research question for all three experiments was

– *is the choice of proportion of added material connected to firing?*

Although a hypothesis is not strictly necessary in an archaeological experiment, it is common procedure for highly controlled, scientific experiments to work from a hypothesis. To ease comparison between results, the hypothesis was therefore used as a starting point for all experiments.

– *there is an observable difference between ceramics with 25 %, 50 % and 75 % added material (related to temperature)*<sup>212</sup>

The intention was to reach conclusions that could inform on the choice of

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210. The soapstone was sourced at Vargavågen, Os municipality, Hordaland county. Nearby finds of Bucket-shaped pots containing soapstone are for instance B14441 and B10890.

211. Appendix C.5.

212. Parentheses only part of the hypothesis for experiment 1-2 and 1-3

proportion of non-clayey material in Bucket-shaped pots, particularly related to soapstone. Experiment 1-1 was constructed as an actualistic firing, but rather than a stone lined pit that is at current more hearsay than indicated in (available) archaeological evidence, it was decided to set up a shallow pit-firing, completely open to natural forces. As it has been demonstrated through ethnographic experiments and firings that a) bonfires yield vastly different temperatures, even within one structure (Gosselain 1992a) and b) that pots cannot feasibly be used as indicators of type of firing structure (Smith 2001), this was determined to be the least restrictive version of firing. Although a contemporary two-chamber kiln find has been made at Hasseris, Denmark (Jakobsen 1962), there are not sufficient indications to warrant a kiln construction for the firing of Norwegian Bucket-shaped pots, especially considering that the current status in Norway point towards the use of stone lined pits, and no further kiln finds have been reported.

Experiment 1-2 was designed as a lab experiment<sup>213</sup> using an electric kiln, to tightly control temperature. In this manner, the higher variable control would add to the interpretation of the behaviour of soapstone, further highlighting the choice of proportions. As a prolongation, a third experiment (1-3) – was executed under controlled circumstances in material testing equipment in the X-AT, UoE, laboratory in collaboration with Dr. Tommy Shyng, material scientist at X-AT.<sup>214</sup> This experiment was designed to address potential differences in toughness between ceramics with different proportions of soapstone. Even if both experiments were executed under controlled circumstances, the test-tiles were handmade and the distribution of the components in the clay matrix was therefore relative to each tile and hence an uncontrolled variable.

A pilot experiment included a class of archaeology students from the University of Oslo, and the pilot manufacture phase was quite useful in addressing certain key issues that should be addressed in the test vessel manufacture. The experiment tested an open bonfire-firing with vessels of 25 % and 50 % soapstone, and took place 9/5/2014. The firing was uncomplicated and all vessels survived. No changes were made to the firing procedure in the final

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213. See p. 52

214. Exeter Advanced Technologies, University of Exeter

experiment.

As the present author has adequate experience with hand-built ceramics from previous experimental work (Schenck 2010; Schenck 2014), the test vessels were produced by the author. **As part of the pilot experiment, the soapstone was ground and pounded to angular particles between c. 20 x 20 and c. 250 x 400  $\mu\text{m}$ . This soapstone was subsequently used for all experiments.** The clay and soapstone were measured and sieved together before water was added. The paste was mixed to 25 %, 50 % and 75 % soapstone, and wedged. It was rolled to an even 5 mm with the help of a rolling pin and two guides, before being moulded over a plastic flowerpot with the approximate technique used by Kleppe and Simonsen (1983).<sup>215</sup> However, what was not addressed in the 1983-experiments was that the 75 % soapstone mix is extremely loose with low adhesion, possibly due to the high talc contents.<sup>216</sup> In other words, to make the pot in a vertical position, as suggested by Kleppe and Simonsen, was demanding, as the clay did not adhere well in the join, and tended to rupture or fall off altogether. This was somewhat remedied by making the wall-slab much wider than the intended wall height, so that it could support itself before the bottom was fastened. Instead of consistently made out of one wall slab and one bottom disc, the main slab was placed around the mould, and additional smaller slabs were added where necessary before a bottom disc was attached (Figure 12). As the archaeological pieces examined were decorated when leather-hard, the decorative strategy used by Kleppe and Simonsen (*Ibid.*) could not be used to loosen the vessels from the mould, and sticking was prevented by a thin layer of ground soapstone on the slabs before they were attached to the mould. The vessels were loosened by rolling the mould carefully from side to side, and the vessel came off easily.<sup>217</sup> It is important to note that the preparatory phase is not part of the experiment, but it is detailed here to display how kinks in the manufacture process cited in the literature have still to be resolved.

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215. See Appendix C.1. for a thorough illustration of the preparatory phase.

216. One of the prominent features of talc is its low friction, which is used to lessen friction in for instance talcum powder.

217. In effect the same principle as decoration on mould would entail: The light pressure caused sufficient expansion of the walls to create a gap between mould and vessel.



Figure 12: Moulding the test vessels. Note that the 75 % paste is being used to support itself on the table to prevent rupture, and patches are mended with additional, smaller slabs.

The vessels were dried in room temperature, burnished with a polished stone when leather-hard, and subsequently decorated with a pointed tool to provide actualistic variations in wall morphology. The same scheme was followed for each pot, with the same amount of dots and lines. Thirty test-tiles of each proportion were also made (45x45x5 mm) by using a square cookie cutter.<sup>218</sup>

#### 6.2.4. Experiment 1-1

A bonfire-firing was the first experiment, and took place at the UoE campus on 4/10/2014, with the aid of experimental archaeologist Maggie Smith and a group of volunteers.<sup>219</sup> A bonfire was constructed around a Jenway 220 Temp Meter Thermocouple, with locally collected deadwood. The thermocouple was used as a control measure to ensure that the bonfire was not too hot overall, although a single bonfire can show a very wide temperature span, and the temperature measured is not relevant for the firing of the individual vessels (Gosselain 1992a; Smith 2001). The maximum temperature measured during the firing was 906°C.

Four vessels and 15 test-tiles of each proportional type, were water-smoked for

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218. See Appendix C.1. for experiment specifications.

219. David Hancock, Michael Pitts, Katie Pitts, and Matthew Swieton.



c. 1 hour. The fire was raked and a c. 5 cm thick bed of hazelnut shells was arranged on the embers to prevent direct contact with the white-hot charcoal, but still utilise the bottom heat to create even heating and reduce the risk of thermal stress. The vessels were then placed sideways on the bed in a single layer. A bonfire was built around the vessels, and was fired for c. 1 hour, until the vessels were tapped and deemed ready by sound.<sup>220</sup> The fire was raked and the vessels left to cool. All vessels survived the firing, and no spalling was observed. One 75 % vessel was cracked prior to the firing, and this, too had survived.<sup>221</sup>



Figure 13: Experiment 1-1 upon conclusion.

The aim of the experimental work in this thesis is to provide results that can translate to information about the archaeological chaîne opératoire.<sup>222</sup> As all the pots survived in their entirety, experiment 1-1 did not provide immediate input for an interpretation of the background for choices made in the production of Bucket-shaped pots, and the proportions of added material. However, the experiment did show that there was no significant functional difference between

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220. Ceramic pots will yield a resonance quite different to a clay pot.

221. This vessel was later used as an exercise in drilling and repairing a pot by MA student Michael Pitts. Two holes  $\varnothing$  4 mm were drilled with a flint borer and the crack was lashed with sinew without any complications.

222. Figure #



vessels of a standardised (25 %) and actualistic proportions of soapstone, when fired on a bonfire-firing – presumably the most temperamental of all, which normally demands more temper than controlled firings due to its unpredictable variability. However, the difference is likely to be of greater variety at the low end of the scale, as untempered vessels can usually only survive kiln firing, but archaeological vessels have presumably survived bonfire-firings tempered with as little as 5 % additions (Vandiver 1987: 17). The explanation for the high proportion of soapstone in the archaeological vessels must therefore be sought elsewhere.



Figure 14: Detail of 75 % soapstone vessel after firing is completed. Note the metallic sheen in the vessel wall due to the combination of burnish and soapstone content.

Interestingly, the unburnished test-tiles consequently displayed a prominent aesthetic difference between the different paste proportions that can be related to the additions and its firing: whilst the 25 % soapstone tiles presented a matted appearance, the 50 % and 75 % tiles were significantly more golden in appearance, macroscopically highly visible **at lighting at slanted angle**.<sup>223</sup>

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223. **For image of the test tiles after firing, see Figure xii, Appendix C2.**

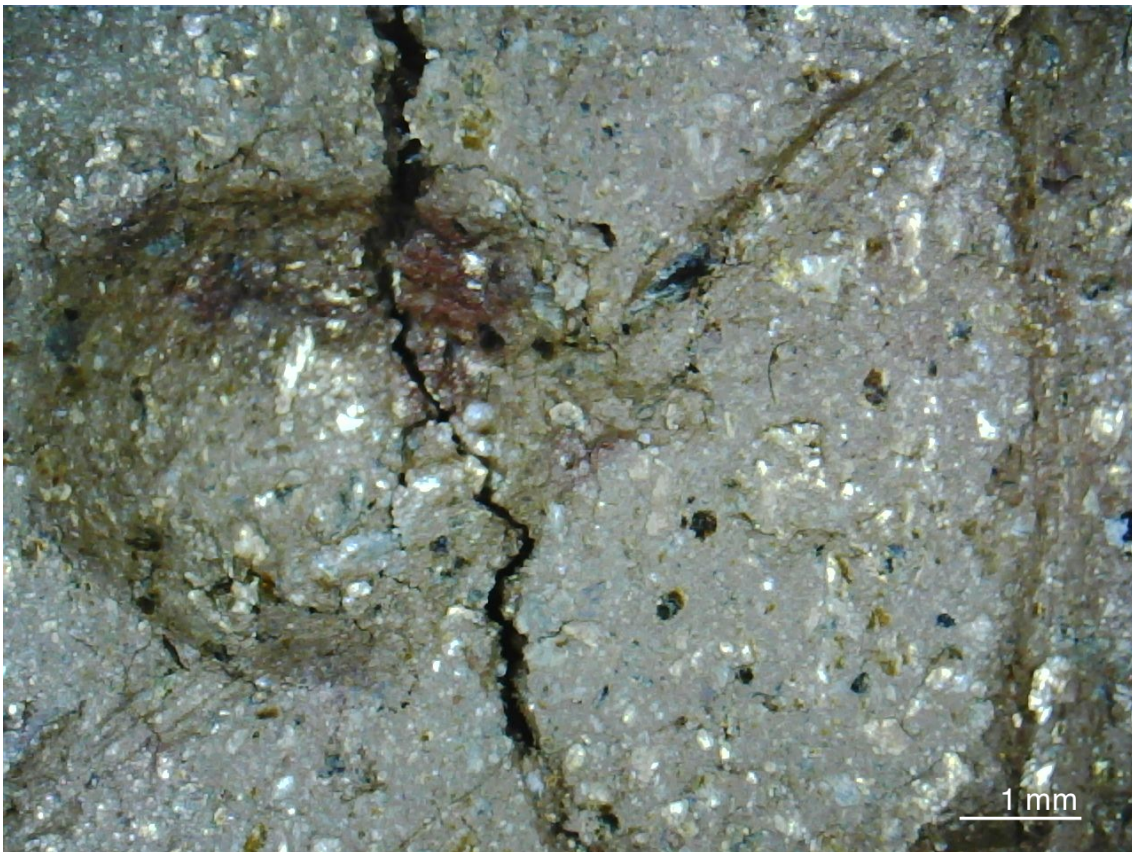


Figure 15: Experimentally bonfire-fired pot, 50 % soapstone. The golden appearance is visible in both the unburnished pit decoration, and in the burnished, surrounding area.

This feature was also apparent in the experimental pots, both macro- and microscopically (Figure 14 and 15), also on burnished surfaces. Whilst burnishing is not new in the prior ceramic chaîne opératoire, the metallic lustre seems to be a novelty. If all Bucket-shaped vessels categorised as soapstone-tempered are in fact made with soapstone,<sup>224</sup> it is interesting to note that the 25 % soapstone specimen are not golden to the same extent, and so higher (temper) proportion than used previously must be added to the paste to gain these characteristics. Exactly how high should be further explored by experiments.

#### 6.2.5. Experiment 1-2

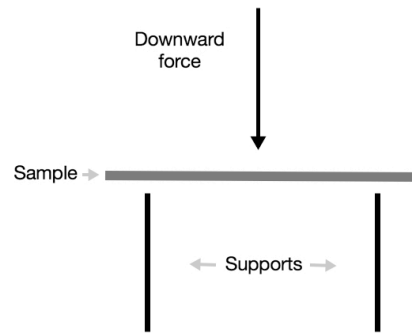
As a comparative experiment, one vessel and 14 tiles of each temper proportion were fired in an electrical kiln at the UoE pottery laboratory. The experiment was

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224. Up to now, only a limited, published sample has undergone archaeometric and/or petrographic analysis (Hulthén 1986; Kleppe and Rueslatten 1993; Magnus 1980). However, this is changing (Fredriksen et al. 2014).

set up to investigate the temperature variable more closely, and to further maintain control, the same pots were re-fired 3 times to different temperature thresholds: 500 °C, 750 °C, and 1000 °C. These thresholds were chosen to highlight different temperatures to which Bucket-shaped pots have demonstrably been fired to (Hulthén 1986; Kleppe and Rueslatten 1993).

Several features became visible as the tiles were fired to increasingly higher temperatures. Before-pictures showed the soapstone particles as matted silvery and greyish, but already at 500 °C, certain particles change colour to redder hues. At 750 °C increasing amounts of the mineral blend has reddened, together with a substantial change from matted through shiny silvery and eventually to golden (Figure 16). This golden appearance is again seen macroscopically, but the particle reddening generally occurs at a microscopic level. At 1000 °C, the majority of particles are golden, however, the proportion of red particles has only increased somewhat. These appear to be different particles, and the change is probably due to the specific mineral formula that makes up soapstone in each specific case and its reaction to heat.





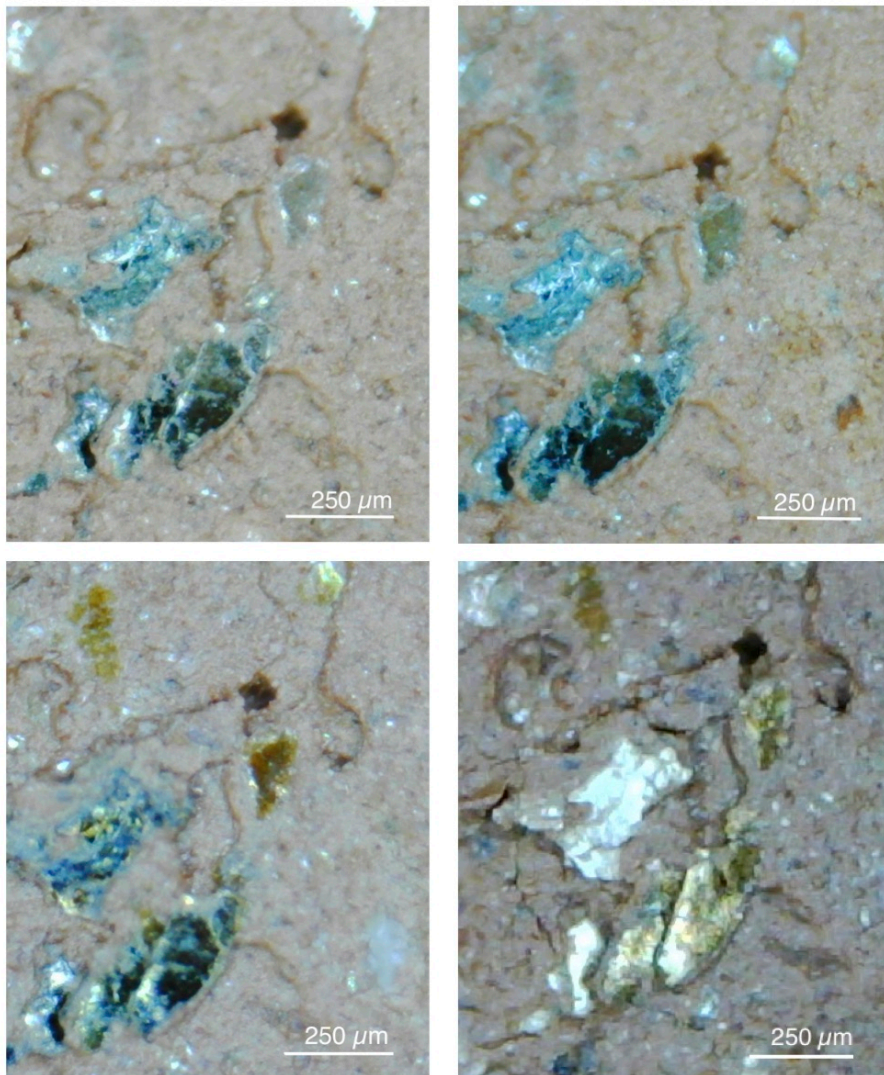


Figure 16: Experimental tile (25 % soapstone) detailing the change from silvery to golden. Firing temperatures clockwise from top left: before firing, 500 °C, 750 °C and 1000 °C.

Similar features are visible in the experimental bonfire-fired pottery (Figure 17), however, bonfire zones may simultaneously differ vastly in temperature (Gosselain 1992a). This pottery can therefore not function as a reference for the archaeological pieces as firing mode is at present unknown.

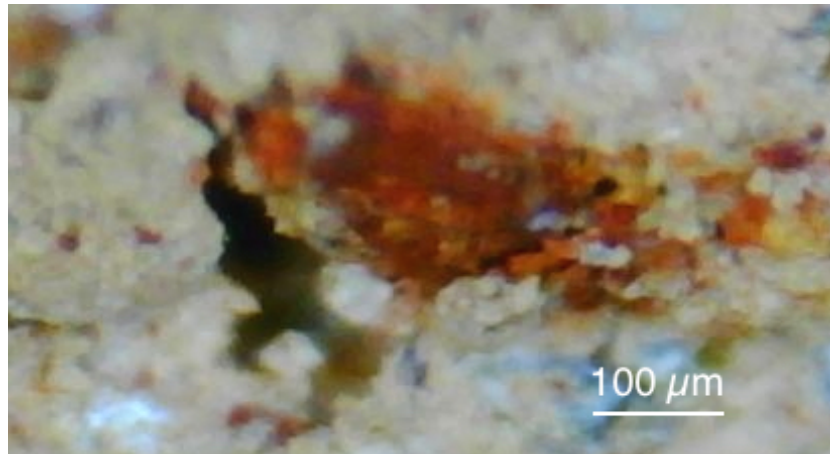


Figure 17: Red particles clustered in a 50 % bonfire-fired specimen. .

Even more so, it is interesting to note the presence of certain features in the archaeological pottery. Although it is at this point not possible without petrological/archaeometric analyses to go into detail about firing situation and functionality thereof, the particles visible in C12321a (Figure 18) are both golden and reddened, which suggests a temperature over the 500 °C threshold.

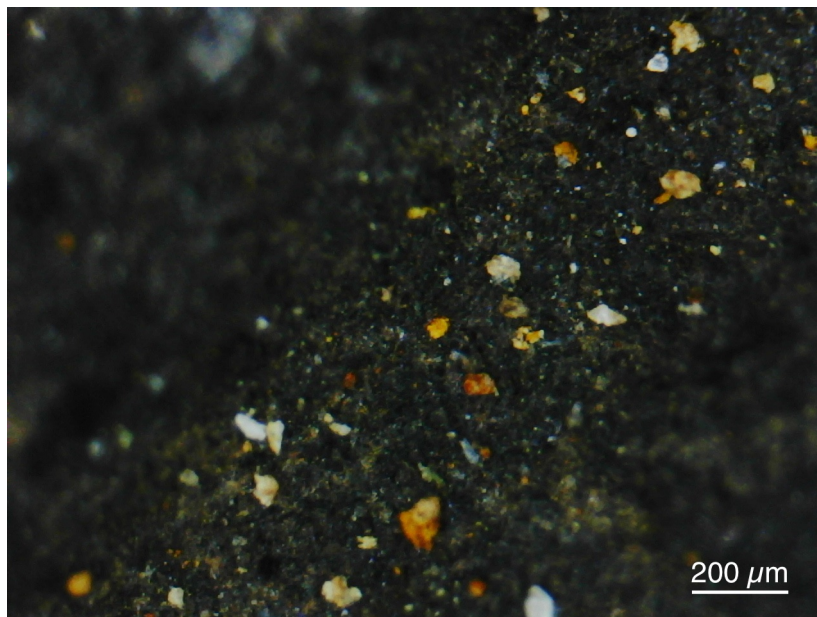


Figure 18: C12321a, Hafsøy, Eigersund, Rogaland. Golden and reddish particles in decorative groove.

Nevertheless, experiment 1-2 provided another interesting result; namely that the 500 °C test-tiles and vessels provided a soapstone with a silvery hue much like the unfired specimen. More significantly, the experimental 500 °C pieces are very similar to archaeological sherds that have been catalogued as mica ("glimmer") (Figure 19).

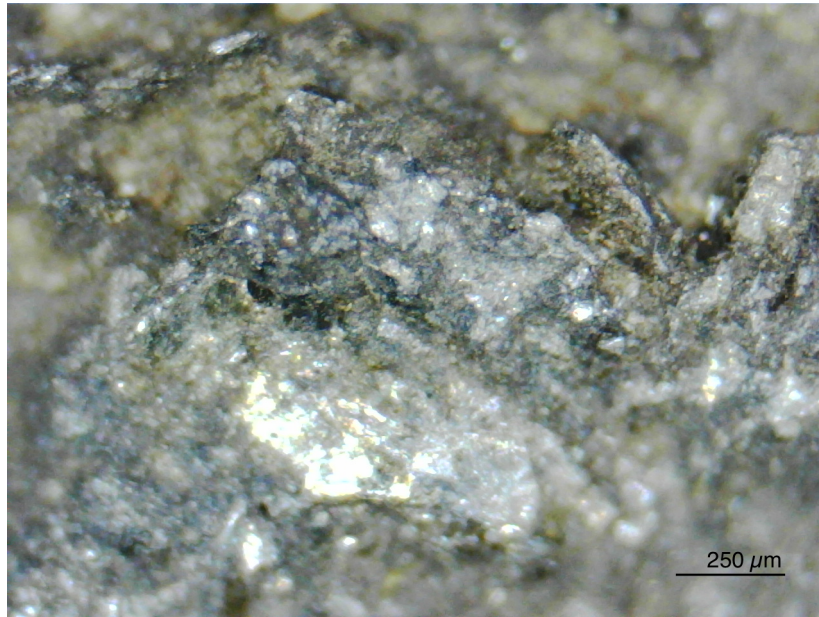


Figure 19: Temper particles in C18425b, Vasshus, Suldal, Rogaland. The particles in the paste are strikingly similar to the soapstone in the experimental pieces when fired at 500 °C. The catalogue entry describes this pot as tempered with mica.

In sum, a feature that becomes prominent as a result of the kiln experiment is the relation between heat and the metallic appearance of soapstone additions. At this point, it is suitable to ask the question whether the glimmering mica mentioned as a decorative element by Kristoffersen and Magnus and others (Kristoffersen and Magnus 2010: 59) may in certain cases turn out to be soapstone that has been added in the necessary proportions and fired to the appropriate temperatures. As the test pots at 500 °C do display a significant silvery appearance, which both after bonfire and 750 °C and 1000 °C kiln firings turns strongly golden, the previously suggested connection to metallurgy should be kept open for further exploration.<sup>225</sup>

#### 6.2.6. Experiment 1-3

As the firing experiments yielded inconclusive results, additional mechanical tests were conducted by Dr. Shyng at the X-AT laboratory. We selected three-point bending in a Lloyd EZ20, rather than pendulum impact tests which are sometimes used (Bronitsky and Hamer 1986; Pierce 2005). The test indicates

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225. Certain archaeological specimen – for instance S11759 and S11762 – display a silvery appearance, which would coincide well with the low temperatures pots have demonstrably been fired to in certain instances (Hulthén 1986).

how much force a material can support without fracturing, and how much it plastically deforms (deflects) to absorb the load in the process. The results provide information on gradual force resistance rather than the catastrophic impact which would be indicated in a pendulum test. It can therefore inform on durability in assumed, everyday use situations, such as stacking or knocking against something (for instance surface, utensils or other vessels). To keep the variables as controlled as possible, only kiln fired test-tiles were used.

Three point bending consists of a sample that rests on two adjustable supports while a set load bears down upon the sample over a set interval of time. The output can be read in load in Newton (N), and deflection in mm before fracture. Due to the handmade test-tiles, this should not be read as a definite modulus, but the results nevertheless provide a valuable idea of durability of different temper proportions. The test parameters were set to 0,1 mm/min speed, on supports 25 mm apart. The tiles tested were fired to 750 °C and 1000 °C respectively.<sup>226</sup>

The test yielded interesting results (Figure 20). As can be seen in the diagram, two clusters formed according to firing temperatures: a lower score cluster which consisted of 750 °C fired tiles, and a higher, slightly less uniform, score cluster comprising the 1000 °C tiles – clearly stronger, as can be expected as the mass develop towards vitrification with increasing temperature (Skibo et al. 1989b: 125). Nonetheless, all samples yielded instantaneously at low deflection, and can be described as brittle.<sup>227</sup> Moreover, there was no observable difference between tiles with 25 %, 50 % or 75 % soapstone content. Translated to an actualistic scenario, this would indicate very little difference in durability between pots comprising 25 %, 50 % and 75 % soapstone in relation to gradual impact such as stacking.<sup>228</sup> As a conclusion, even in large amounts, soapstone does determinately not weaken the toughness of the ceramic structure, quite opposed to the general idea that temper weakens the ceramic mass upon firing

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226. See Appendix C.4. for detailed results table.

227. Pers. comm., Tommy Shyng, 6/7/2015.

228. This is quite surprising considering the mohs hardness (1) of talc in soapstone, as well as chlorite (2-2,5), whereas well fired ceramics can fluctuate around 7. However, it must be presumed that the hardness is usually measured in an unprocessed state.



(Gibson and Woods 1997: 30-31).<sup>229</sup> However, neither did it strengthen the body, and so no relation between temper and production firing can at this point be identified.

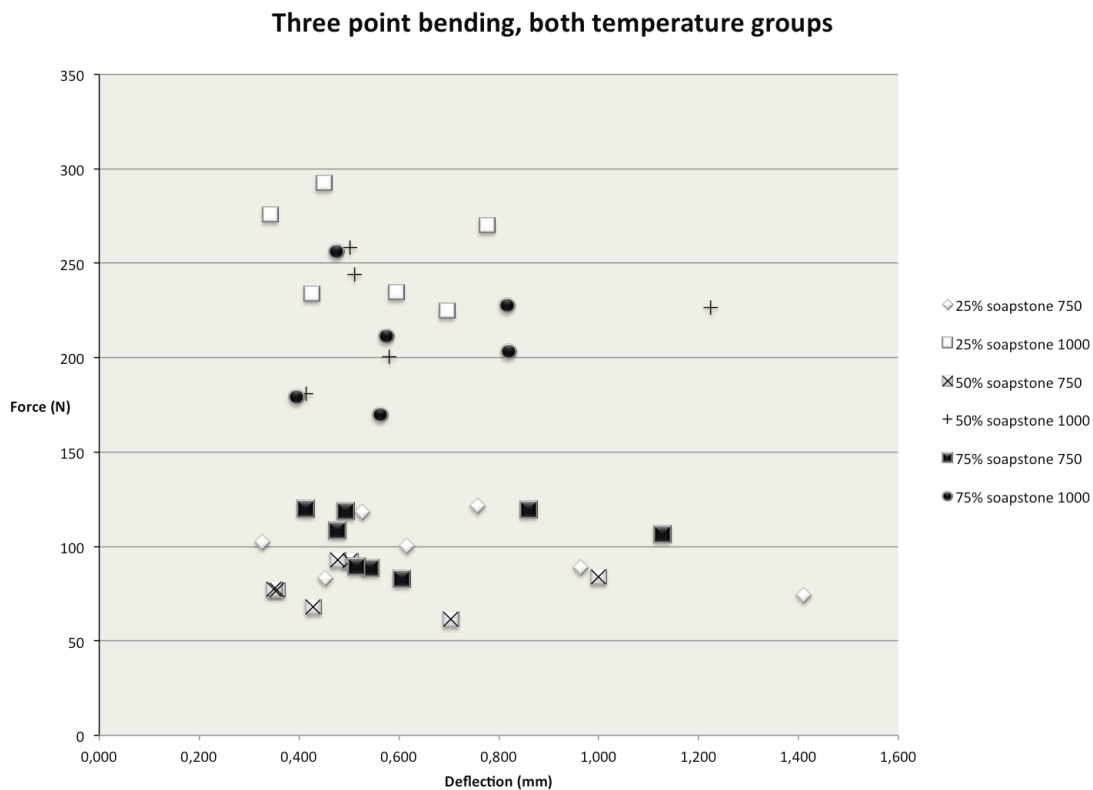


Figure 20: Scatter plot of three point bending test, kiln fired test-tiles to 750 °C and 1000 °C

### 6.2.7. Conclusion: the intangible concepts of tasteful dying?

The experiment did not yield observably different results in firing functionality between temper proportions, as shown through the bonfire-firing. Neither did aspects of durability seem to be related to the temper, as could be inferred from the results of experiment 1-3. However, these conclusions may help to shed light on intentions for the substantial proportion of non-clay additions in Bucket-shaped pots. As observed through both experiment 1-1 and 1-2, differences *are* visible between soapstone proportions. A metallic sheen occurs after 500 °C, but sometime before 750 °C, and this sheen is increasingly prominent in experimental pieces that consist of 50 % and 75 % soapstone. However, the

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229. This could be related to the platy structure of most minerals in soapstone, which will help dissipate the energy, prevent crack propagation and may even increase toughness (Tite 1999: 219-220).

metallic appearance is less eye-catching in pots comprising the standardised proportion of 25 % soapstone. Considering that previous pots in South Norway have been shown to hold significantly less temper than 25 %, it can be concluded that a 'normal' proportion of c. 15-20 % added material would probably not achieve a significant metallic effect.

What does this mean? When considering the technical manufacture process, the increasing amount of temper seems counterproductive, as the pots barely adhere internally due to lack of clay in the paste. However, it does seem likely that firing has been a key factor in the stylistic scheme and taste. This could be related to the actual appearance of the soapstone – matted silvery before, or golden after the threshold for colour change. Another alternative is that the soapstone played a representational role as a *proxy* for metal, as elements of gold have been found in recent analyses of the pots (Fredriksen et al. 2014). An interesting feature in combination of the metallic appearance is the fact that many of the pots seem to have been fired at low temperatures, some as low as 500 °C (Hulthén 1986). As may be indicated by the black core morphology, many pots could have been secondarily fired on the funerary pyre (e.g. Fredriksen 2006). If so, and as there seems to be little by way of organic compounds in the clay paste, the pots would have been reduction fired in the primary firing, rendering them black. If this was indeed the case, and over 50 % soapstone was added, the initial ceramic ware could have been black and silvery. Upon secondary firing of such a specimen on a funerary pyre, a full transformation to red and golden would occur.<sup>230</sup>

In providing metallic appearance, the soapstone proportion plays a significant role that may possibly link the pots to metallurgy and gold smithing towards the end of the Migration Period (Fredriksen et al. 2014). As a post-experimental abduction, the following arguments can be set up to feed into the final inference about the field of practice of Bucket-shaped pots:

- The ceramic chaîne opératoire changes abruptly around 350 CE, but not due to people displacement.

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230. Given that oxidisation occurred.

- The pots probably held a strong symbolic value as a frequent part of a set of funerary equipment.
- Recent research has established a likely connection between metallurgy and pottery, mediated by heat.
- The novel, high amounts of soapstone in Bucket-shaped pots does not influence the purely utilitarian functionality of temper in the firing of vessels, even thin-walled vessels are adequately fired on a bonfire-firing with 25 % soapstone. For firing purposes, the high proportions are neither functional nor impractical.
- The high amount of soapstone lacks a functional paste adhesion, as opposed to statements made by Bøe (1931: 170-171, 204f).
- The higher proportions of soapstone do influence the aesthetics of the pots towards a metallic appearance.
- The soapstone appearance is significantly affected by heat.

According to the above arguments, it does no longer seem likely that the soapstone proportions are tied to the production process and desired stabilising properties. However, Bøe was right that heat-treatment plays a role in the production of these pots, but rather than utilitarian, it is the aesthetic significance that becomes prominent through the experiments reiterated here. Instead of tying the soapstone proportions to preconceived, unresearched notions of practicability, experiments have shown that intangible elements can come forward through experimental findings. Considering the status of soapstone in Bucket-shaped pots pre- and post-experiment, it does seem more likely that aesthetic functionality, related to social taste, was a desired end result of the production chain revolving around soapstone, and this functionality was mediated by heat. In this case it would imply that within the field of Migration Period pottery, aesthetic perception changed to include something that was previously presumably added for utilitarian function; in the words of Bourdieu, the doxa<sup>231</sup> changed.

**The** experiment produced a result which must be considered more likely than what the previous discussion of non-clay quantities has provided. It must

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231. See p. 22

**therefore**, for the purpose of this thesis, be reviewed whether the experiment evidential level has increased. The physical evidence and contexts were already at maximum (direct evidence) for the majority of the archaeological Bucket-shaped pots. However, the evidence for the technological process relating to soapstone is no longer just *associated* (as it was when the undistinguished soapstone/asbestos/other were given the standard roles of temper; beneficial to production and/or cooking). Instead, there is now new evidence, however *indirect*. The addition of soapstone is directly evidenced, but the intangible motivation behind the use of soapstone specifically is only indirectly implied through its use in large quantities. However, a new intention for the addition can be suggested, and should be seen in context with the metallurgical aspects launched recently (Fredriksen et al. 2014). The soapstone does not change from silvery to golden until the temperature threshold in the interval 500 °C to 750 °C is reached, however, many Bucket-shaped pots were fired to below 500 °C (e.g. Hulthén 1986). It is therefore conceivable that the change was meant to occur in the funerary environment that would take it to above the – max 750 °C – threshold, such as seen at Kvasseheim, and that the colour change would take place during rites such as cremation. Even if this argument cannot be decisively made, the evidence indirectly does show that Migration Period potters probably did *not* add these quantities to the paste due to the stabilising, heat-resistant properties. In sum this all renders the evidence for an aesthetic reasoning augmented, and the experiment has produced a conclusion that yields better grounds for the interpretation presented here, all the while refuting previous interpretations.

It is not within the scope of this thesis to fully explore the meaning of the aesthetic role of soapstone in Bucket-shaped pots, as this will necessitate a deeper analysis of the society in which the pots occurred. Whether it is related to a new functionality of the metallic, of surrounding landscapes, the function as potter, living, dying, or something else, was all determined by social agency. The experiments have, however, launched new interpretations, and established connections between soapstone use, aesthetics and the related role of fire. In this way, they are a good demonstration of how something intangible can be approached through the tangible experimental process.



## 7. Technological intentions and pecked pebbles

The Bucket-shaped experiments<sup>232</sup> indicated how experimental archaeology can be successful in the production of meaningful results that concerned the intangible. In contrast to the previous experiments, this chapter will discuss a technology that is both less understood and investigated: faceted pebbles from the Scandinavian Mesolithic.

### 7.1. Case study 2: Mesolithic faceted beach pebbles

#### 7.1.1. Faceted stones. Context, period, technocomplex

The South-East Norwegian Mesolithic (c. 10 000 – 3 800 BCE) spans approximately 6000 years and is divided into four phases as seen in Table 4. The general Mesolithic features of a mobile hunter/gatherer subsistence, marine site orientation, axes or adzes, microblades, and general blade and flake technology dominate most of the Mesolithic cultures. Nonetheless, there are distinguishing typological features that have led to the below categorisation, such as a variety in settlement size and pattern, raw material use, and transitions from macro- to microlithic technocomplexes (Bang-Andersen 2003; Bjerck 2008; Damlien 2014).

Table 4: Chronological phases of the Mesolithic in Southeastern Norway. Based on Fuglestad 2005, Glørstad 2002

Period	Phase	BP	BCE
Kjeøy	IV	5800-5000	4650-3800
Nøstvet	III	7500-5800	6350-4650
Tørkop	II	9000-7500	8250-6350
Fosna	I	Pre 9000	Pre 8250

The focus artefacts for the present experiments stem from the South-East Norwegian Late Mesolithic, which is divided into two phases: *Nøstvet* (6350 –

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232. See Chapter 6

4650 BCE) and *Kjeøy* (4650 – 3800 BCE). The *Nøstvet* phase is characterised by the appearance of the *Nøstvet* adze; a roughly knapped adze type, usually not flint, with a ground edge, and trapezoidal/D-shaped cross section. In addition to the diagnostic adze, other typical features are grinding slabs, presumably used with the adzes; borers; microblades from handle cores, and extensive use of bipolar technique. The absence of formal projectile points and a high occurrence of microliths and debitage may suggest an increased reliance on composite tools (Bjerck 2008: 81; Jaksland 2003b: 272). Settlement sites are primarily coastal, and can be both large and complex with repeated and planned use, or small sites indicative of a single visit. Circular hut dwellings are found on several sites (Glørstad 2004: 62-63), and may indicate an increasingly stable settlement system, potentially in the form a base camp system, much like the contemporaneous Ertebølle culture in South Scandinavia (Jensen 2003: 40-44).

*Phase IV/Kjeøy* (4650 – 3800 BCE), is often seen in light of the transition to the subsequent Neolithic. This phase is sometimes named *Late flint point using groups* or the *transverse point phase*, which underscores the diagnostic inventory dominated by transverse flint arrowheads. Composite tools are indicated through the resumed, but diminishing microlith production, and circular hut structures are still the main dwelling features in the archaeological record.<sup>233</sup> Along with the varied settlement size, the *Kjeøy* phase can rather be seen as a prolongation of the *Nøstvet* phase (Bjerck 2008: 79f; Glørstad 2004: 62f). The absence of classic *Nøstvet* elements such as adzes and grinding slabs, in addition to the decreased frequency of borers, may nevertheless indicate a break in subsistence and/or technology; perhaps a changed technology that becomes indeterminate. A few *Kjeøy* elements indicate the transition to the Neolithic, such as the introduction of macroblade technology that has been largely absent for c. 2500 years (Bjerck 2008; Glørstad 2004: 37-38). As flint is not native to Norway, flint objects was presumably fashioned out of small beach nodules washed up by glacial retreats (Fuglestad 2007: 100; Jaksland 2003a: 231). The increased flint blade size along with the widespread exchange of supra-regional concepts such as transverse arrowheads, indicate an

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233. However, some sampling bias is conceivable, as truly mobile dwellings are most likely very hard to trace.

establishment of trade and connection networks that would allow for the production of larger blades on material from South Scandinavia, and eventually facilitate the introduction of polished axes and ceramics (Ballin 1999: 18, notes 4-5; Eigeland 2011b; Glørstad 2004: 31).

Within this flux of technology and networks, a particular artefact category surfaced at two Kjeøy sites – Torpum 13 and Rørbekk 1 – of the Svinesund site complex in Østfold county, South-East Norway. Three round or oval, granite beach pebbles, the otherwise smooth surface disrupted by one or two pecked, circular/oval facets, were eventually interpreted as anvil stones (Jaksland 2003a: 231-232, Fig. 125; 2003b: 258, Fig. 138), but vaguely classified as "stones with pecked facet" (Jaksland 2003a; Jaksland 2003b). However, upon closer investigation and through knapping experiments by bipolar specialist Lotte Eigeland, this interpretation has been discredited. Eigeland did, however, confirm that the stones do indeed seem pecked.<sup>234</sup>

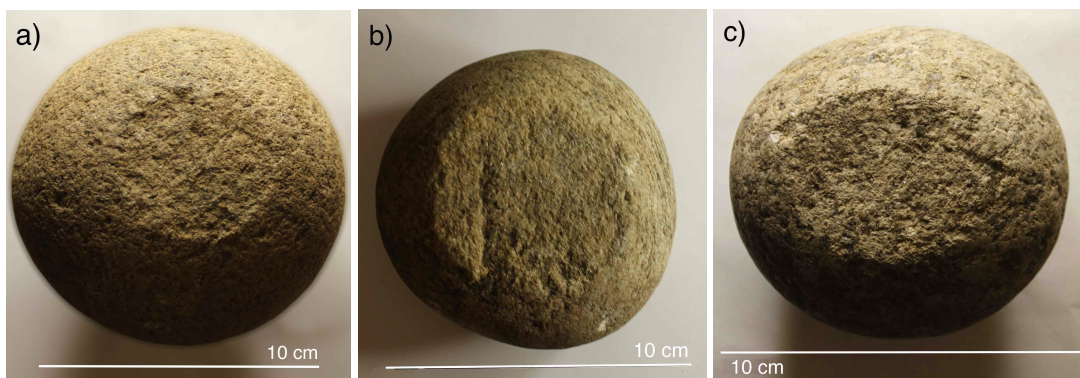


Figure 21: Faceted beach pebbles from Svinesund. a) T13/1 and b) T13/2, both Torpum 13: C53852/31. c) R1, Rørbekk 1: C53854/22.

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234. Pers. comm., Lotte Eigeland, June 2014



Table 5: Find details: pecked pebbles, Svinesund, Halden, Norway.

Catalogue no	Internal ref no	Site	Material	Dimensions	Facet	Weight
C53852/31	T13/1	Torpum 13/ felt 2	Granite (fine- grained)	90x91x74 mm	a) Round. $\varnothing$ 43 mm. Potential pitting. b) Round $\varnothing$ 38 mm. Potentially unfinished/unexhausted.	1172 g
C53852/31	T13/2	Torpum 13/ felt 2	Granite (fine- grained)	85x82x48 mm	Round. $\varnothing$ 61 mm. Deep crack along one side, otherwise even surface. Covers most of one side.	682 g
C53854/22	R1	Rørbekk 1	Granite (rough- grained)	78x74x56 mm	Round. $\varnothing$ 51 mm. Potential macroscopic wear. Two deep pits along surface edge.	612 g

The settlements that yielded the stones were shorebound, and situated on a Late Mesolithic island. All are dated to the transitional Kjeøy phase. All finds stem from areas near the contemporary beach. The two stones from Torpum 13 (C53852/31)<sup>235</sup> were found in an activity zone associated with microblade production and a hearth, at the outskirts of a repeatedly used site with a circular hut structure. A substantial amount of flint scrapers, flint debris and the majority of hammer stones were also found in the same area (Jaksland 2003b: Fig. 141-145). An interpretation based on the find distribution alone could point to hide working close to the water, or potentially a midden zone; in which case the stones in question were possibly considered unusable and were discarded.

At Rørbekk 1, a small, possibly singular visit-site without dwelling structures and only one or two fireplaces, a faceted pebble (C53854/22)<sup>236</sup> was found in an area with low find frequency (Jaksland 2003a: Fig. 126-127). All the stones are presumably sourced from local beaches, as the island bedrock consists of granite and gneiss (Glørstad 2004: 60).

Although the stones were encountered with puzzlement upon excavation, a search through UNIMUS database for portable antiquities yielded a number of finds that could possibly be of the same kind, and a selection was made and

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235. These stones were catalogued under the same museum catalogue number, and are in this thesis given the internal reference T13/1 and T13/2 to separate (see Table 5).

236. Internal reference no R1 (see Table 5).

examined at the Museum of Cultural History in Oslo.<sup>237</sup> Although all stones were described with similar characteristics, they bore different categorisations in the database; most typically grinding stone,<sup>238</sup> hammer stone,<sup>239</sup> or anvil stone.<sup>240</sup> Functional hammer stones typically display a less regular contact surface, as exhibited by the large collection of experimental hammer stones at the University of Exeter, generated by a varied user group of all expertise levels, and so was excluded.

A list of characteristics was developed to group the artefacts into three types:

- 1) Classical pecked facet pebble: beach pebble with one or two pecked facet(s) clearly separated from consequential wear by a preconceived plane and level expression. The facet edge is notably demarcated, the facet is even, and has no visible grinding striations or other classical grinding features. The facet constitutes an entire aspect of the stone and therefore lacks level surrounds. Most of the artefacts investigated at the KHM fall into this category.
- 2) Probable pecked facet pebble: beach pebble that either lack one of the features above, or where textual description and pictures from the database makes it a likely match to 1).<sup>241</sup>
- 3) Potential pecked facet pebble: beach pebble where textual description and/or pictures from the database makes it a possible match to 1).

The current distribution of identified artefacts can be seen in Figure 22.

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237. See Appendix D

238. Norwegian: Malestein

239. Norwegian: Slagstein/Knakkestein

240. Norwegian: Amboltstein

241. Artefacts that lacked two decisive features, and which also did not resemble T13/1, T13/2 and/or R1 were excluded.



Figure 22: Distribution of faceted pebbles according to UNIMUS' database. Illustration: Author. Map resources: Kartverket, Hayden120 and NuclearVacuum.

Two distribution patterns seem to aggregate among the pecked facet stones. One concentration occurs to the east, in the Northern Oslofjord area, whereas the other concentration is dispersed along the western coast. As so many stones are mislabeled, these patterns cannot be considered decisive. Nevertheless, it is interesting to see that few stones appear along the southern- and northernmost parts. This may be related to the use of the stone. The stones

are notably also mainly found in coastal areas.<sup>242</sup>

Alongside the three focus artefacts, a select number of artefact from the Oslofjord concentration were macroscopically and microscopically examined, with an Oitez eScope DPM15 5 MP CMOS portable USB-microscope and a Firefly GT800 2 MP portable USB-microscope.<sup>243</sup> The investigations took place in February and June 2014. Due to the frequent misclassification, classical examples of the misnomers were represented for comparison.

Under the microscope, it became clear that T13/1, T13/2 and R1 displayed loosened crystals and large pits on what appears to be a pecked surface. These are wear characteristics typical of fatigue wear, which indicates a vertical movement pattern that crushes the surface. In addition, a common characteristic was a rounding of crystals, suggesting a pliable contact material that could access interstices between elevated crystals (Adams 2002: 30, 39).<sup>244</sup> Neither of the three exhibited the abrasive or tribochemical wear characteristics of typical stone-on-stone grinding, such as striations and flattening of the crystal particles (Adams 1989: 260-261, 264). Nor did they show the irregular fatigue wear and severe pitting that would result from use as anvil or hammer stones. Two of the three case stones, in addition to one comparative stone,<sup>245</sup> exhibited fibres that look like very fine, black hairs of approximately 5 to 15  $\mu\text{m}$  thickness.<sup>246</sup> A human reference hair measured for comparison was c. 90  $\mu\text{m}$ .<sup>247</sup> Whether the hairs are organic or synthetic is currently unknown. All fibres do need further analysis before any conclusions can be derived from their presence.

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242. As all stones are the result of rescue archaeology, This may however be biased by the Norwegian pattern of development, which currently favours the coast.

243. Also used were Canon EOS 500D with 60 mm (macro) and 18-55 zoom lenses.

244. All three stones also had a yellowish surface coating, probably from encompassing sediments, since the coating also appears on the natural surfaces of the stones, for a detailed catalogue see Appendix D.

245. T13/2 and R1; C24164b, but also C33214 (mace).

246. Hairs of a similar appearance, but 200  $\mu\text{m}$  thick and both black and white, occurred in the half-finished shaft-hole of a reference club and will be disregarded in the present analysis.

247. Reference range 40 to 120  $\mu\text{m}$ . (Rosenblum et al. 1991: 141)

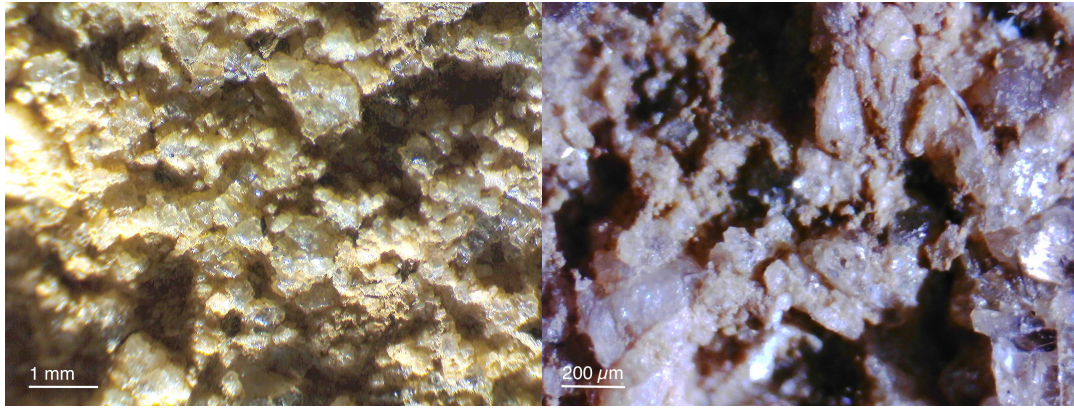


Figure 23: Crystal rounding, some surface debris and substantial pitting, T13/1.<sup>248</sup>

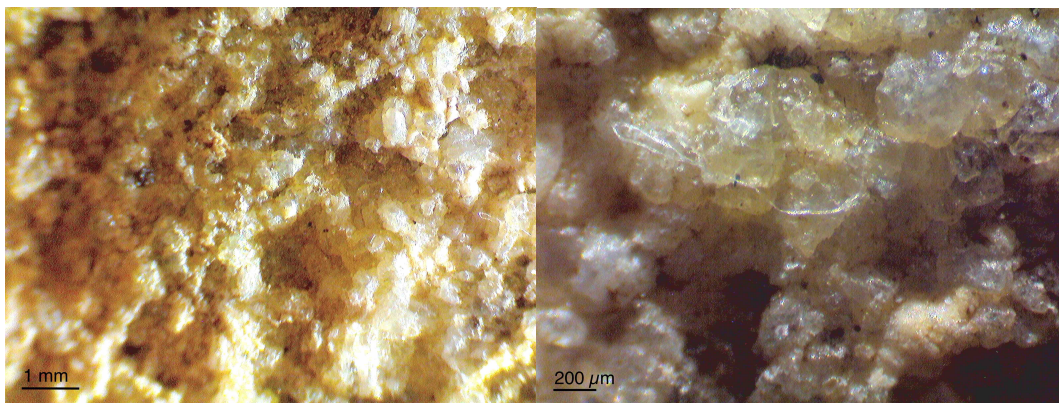


Figure 24: Crystal rounding, some surface debris and substantial pitting, T13/2.<sup>249</sup>

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248. The fibre in this picture is most likely the remains of a cotton fibre from packaging (pers. comm., Douwtje van der Meulen, 29/7/2015.)

249. The fibre in this picture is most likely the remains of a cotton fibre from packaging (pers. comm., Douwtje van der Meulen, 29/7/2015.)



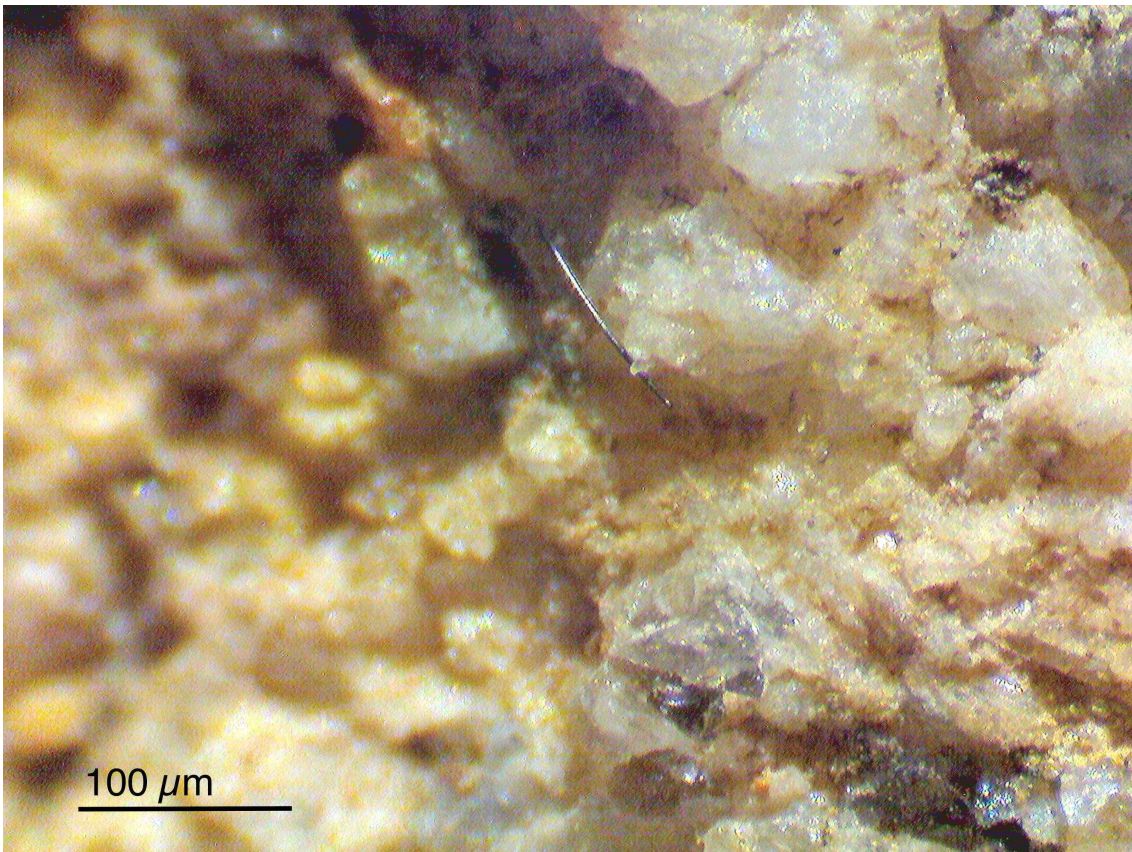


Figure 25: T13/1 with embedded hair close to a pit in the surface.

Two of the similar stones that were found in the database were discovered at the Nøstvet site Frebergsvik in 1969-70, which exhibited a classical Nøstvet inventory. The settlement was shoreline dated TPQ<sup>250</sup> 4900 BCE and is therefore close to 4650 BCE, which marks the transition Nøstvet/Kjeøy. The faceted pebbles were of syenite (no 1)<sup>251</sup> and diorite (no 2)<sup>252</sup> respectively, of which the first produced two pecked, opposite facets and the second one flat facet and one flat, unpecked surface opposite. They were entered in UNIMUS as *Unknown* with the reference C33904t, but were interpreted as grinding stones in the excavation report (Mikkelsen 1975a: 75-76, 107-108). The situation of the stones on-site is not known from the 1975-publication, and is not accessible today.<sup>253</sup>

None of the additionally investigated stones are granite, and an immediate comparison between the Svinesund stones and the remainder of the selected

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250. *Terminus post quem*

251. Internal reference number

252. Internal reference number

253. Pers. comm., Egil Mikkelsen, August 2014

archaeological sample cannot be made.<sup>254</sup> However, tribological wear patterns have certain general characteristics that can be inferred to result from certain types of wear (Adams 2002; Adams 2014).

Examination of other stones with similar facet morphology, located through the database, showed that most followed the same wear pattern with clearly and regularly pitted facets, with a large amount of surface debris and distinctly rounded crystals. In two instances, potential specimens have been interpreted as maces with unfinished shaft-holes.<sup>255</sup> For instance from the Middle Neolithic settlement of *Auve*, c. 3000 BCE (Østmo 1983: 52).<sup>256</sup> This stone was interpreted as a mace-head with an unfinished shaft-hole (*Ibid.*: 93). However, the stone is very similar to the case stones. The other specimen, C52162,<sup>257</sup> fulfils the description of both simple mace-heads and pecked pebble, but its small size of only  $\varnothing$  67 mm implicates the latter rather than the former.<sup>258</sup> What generally seems to differ between shaft-hole initiation and the pecked pebbles is that shaft-holes are generally centred on an already flattened aspect (Ballin 1996; Vitenskapsmuseet 2012),<sup>259</sup> whereas a pecked facet largely constitutes the entirety of the flat aspect.<sup>260</sup> Also, microscopic comparison with wear in a mace-head shaft-hole<sup>261</sup> showed a substantial difference to that on the pecked facets.

In the eastern concentration of pecked facet pebbles, the largest distance between two specimens is 130 km,<sup>262</sup> whereas in the western distribution area, the pebbles are scattered from *Helganes* on Karmøy, Rogaland<sup>263</sup> to Vikna in Nord-Trøndelag,<sup>264</sup> 694 km away. Of the 34 artefacts identified, 20 are stray finds; two are unknown; two are Middle Neolithic; one Mesolithic/Neolithic, and

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254. Petrological determination has only been done on the Frebergsvik stones.

255. C38626/41 Reg 99 and C52162.

256. Based on bone and artefact inventory, this site had a part hunter/gatherer, part pastoralist subsistence with a more sedentary lifestyle (Østmo 2008).

257. A stray find, undated.

258. Neither of these artefacts were examined by the author, but a picture in Østmo (*Ibid.*) has been found to show a nearly macroscopically identical stone to the Svinesund specimens.

259. It is important to note that mace classification in Norway is largely based on Danish typologies for Stone Age and Bronze Age, so these types cannot be considered definite.

260. E.g. B8808, B7529 and S7051

261. C33214

262. C50541a (Ullensaker) and C38626 41 Reg 99 (Auve)

263. S12792/99

264. T17641b and T14685.

eight are Mesolithic. Of these, only one is possibly not Nøstvet or transitional Nøstvet/Kjeøy. Together, the preserved contexts point to a general Late Mesolithic to Middle Neolithic date for the stones. As Norway only sees properly established farming from the Late Neolithic onwards, it is possible that the faceted pebbles were an integral part of everyday hunter/gatherer life. It is the aim of the following experiment to identify information about the practice these stones were part of.

## **7.2. Case experiment: faceted beach pebbles as implements**

### **7.2.1. Experimenting with use intention and subsistence**

As opposed to the ceramics in Chapter 6, there is less discourse on macro-lithic/ground stone tools, under which these should be classified (Dubreuil and Savage 2014: 139, 140).<sup>265</sup> Nevertheless, macro-lithic tools can hold important information that can lead to renewed understandings of economy, subsistence strategies and other parts of community organisation. Such information should primarily be sought through experimentation specific to tool type and processed resource in combination with use-wear analysis of the use surface (Adams et al. 2009: 56).

Several questions have arisen regarding the faceted pebbles from Norway, most prominently what they were for and why there were so few compared to for instance adzes or scrapers. The stones may possibly influence the interpretation of coastal life at Late Mesolithic to Middle Neolithic settlements. They are exotic enough to not have been classified yet, but frequent enough to provide a generalised interpretation. Nevertheless, the overall lack of in situ contexts<sup>266</sup> must be remedied by other means, and to start with a functional experiment seems like a straightforward way to begin.

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265. This definition has no connection to actual ground surfaces and includes pecked implements.

266. Currently, the only documented contexts stem from Torpum 13 felt 2, Rørbekk 1, Helganes at Stokkdal, Auve, Vinterbrokrysset at Nøstvet, and Dal Søndre.



### 7.2.2. Interpretational chain: evaluation of the archaeological chaîne opératoire

To interpret the archaeological chaîne opératoire of faceted beach pebbles is a useful way to initiate the experiment when there is a lack of other information. We know the stones have been sourced and most likely pecked before use. As there are so few on each site, one can assume they were not expedient, and so there must have existed a notion of task. Their wide distribution point to widespread, common ideas of a template.

In microscopy, the pitted surfaces corroborate pecking, and granular rounding indicates that their function was probably related to a pliable material. The surface debris points to a use motion parallel to the contact material (sideways). Sporadic occurrences of tribochemical wear sometimes indicates a high-intensity use, with frictional heat generation. Even if the stones in the eastern concentration seem to have the same type of use over a sizeable area (and time span), it is nonetheless important to remember that the stones may have seen multiple types of wear. Already, the stones have wear from both pecking and the additional, unidentified task, but it is certainly possible that the tool has layered wear from several types of function (Adams 2014; Dubreuil and Savage 2014). Whether the pebbles were handheld is not confirmed, but it is difficult to picture how a functional hafting would occur on their very rounded surfaces without disrupting the pecked facet. This seems counterproductive, and there is no distinguishable wear nor on the facet nor on the natural surfaces. The stones are of a size that will fit comfortably in a variety of hands. **As Norway has numerous pebble beaches, and offers a wide choice of forms, this could be due to personal preference. It could be assumed that their variation in size is to suit different needs or taste; either in weight, in surface area, or in shape or other factors. If so, the size of the stones alone could refer to an important intangible element of that society.** However, the difference between stones may also be coincidental, or not significant.

The fact that the stones have been located along the coast, could point to tasks related to marine settlements exclusively. However, this could also be due to the resource proximity. As T13/1, T13/2 and R1 are found along a beach, it is

possible that their function is related to processing of marine resources. However, these seem to also possibly be the midden zones of the settlements.

The pre-experimental, archaeological chaîne opératoire can be interpreted as follows:

- 6) The stones were discarded/left behind
- 5) The stones were used for handheld task(s), among others with pliable contact material, most likely (partly) parallel
- 4) The stones were brought to use site
- 3) The stones were pecked according to template
- 2) The stones were sourced
- 1) The stones were needed according to an idea/notion

The current experiment is classified as a behaviour/inter-use experiment. The evidence levels relate to the use of T13/1, T13/2 and R1 as main reference points, and are E: direct, T: associated, and C: direct.<sup>267</sup> Because the definite Middle Neolithic stones have not yet undergone microscopy, the chaîne opératoire discussed here will concern the Late Mesolithic chaîne opératoire, specifically located to Svinesund.

### **7.2.3. Preparations**

Because the technological evidence is associated in multiple directions, the experiment was designed to address the most common interpretations that have not yet seen experimentation;<sup>268</sup> especially considering that not all stones have been examined. Of course, there may also be regional and/or temporal variation, but it is assumed that the existence of one template over large distances is somehow connected to a certain technological field in which the pebble held an established function.

The experimental entry point was focussed on functions that could be indicated by a flattened or roughened surface for usage. The following interpretations were therefore selected as starting points:

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267. See Table 3, p. 158

268. As Lotte Eigeland has experimented with use as anvil stone, this was not repeated.

- Unused artefact
- Pounding tool
- Crushing tool
- Grinding stone

As the archaeological facets displayed rounded crystals and interstices, the experiments that seemed most representative for use reference included a pliable contact material. In addition, upon microscopic investigation one of the stray finds of a similar character had an embedded fibre that may bear resemblance to sinew fibres – a material commonly used across time and around the globe for various binding purposes such as fastening elements in composite items or stitching leather (Ingold 2007: 8; Stone 2009: 229; Wescott 1999: 204). It was therefore considered relevant to include at least one experiment involving sinew fibres.

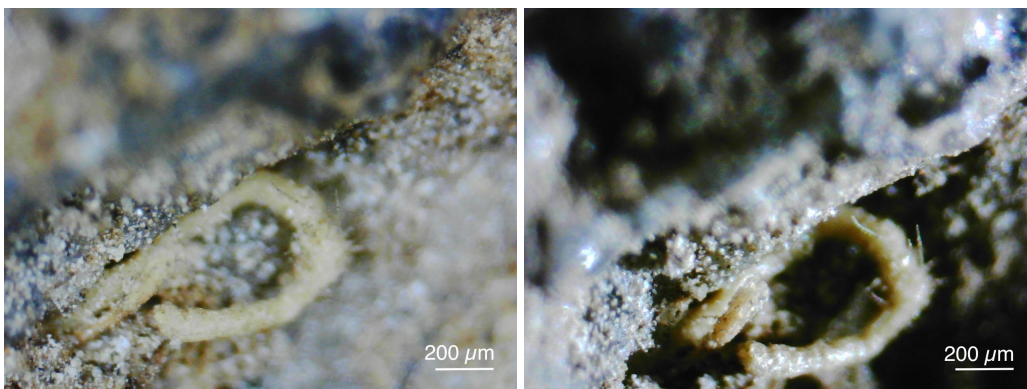


Figure 26: Coiled, embedded fibre from a crack on the surface of C24118 b, Raastad, Akershus, Norway,

A reference sample of the prepared deer sinew was photographed, and the similarity between the sinew fibre and archaeological fibre is notable (Figure 26 and 27). Nevertheless, the lack of preservation conditions for sinew fibres from the Norwegian Stone Age is also notable, except in glacial conditions, and C24118b is a stray find with a possible association to Stone Age through the find of a rhomboid, miniature, Neolithic type axe nearby. Even so, sinew fibres would be a pliable material that can be pounded for separation, and was therefore deemed a suitable task for the objects as hand.

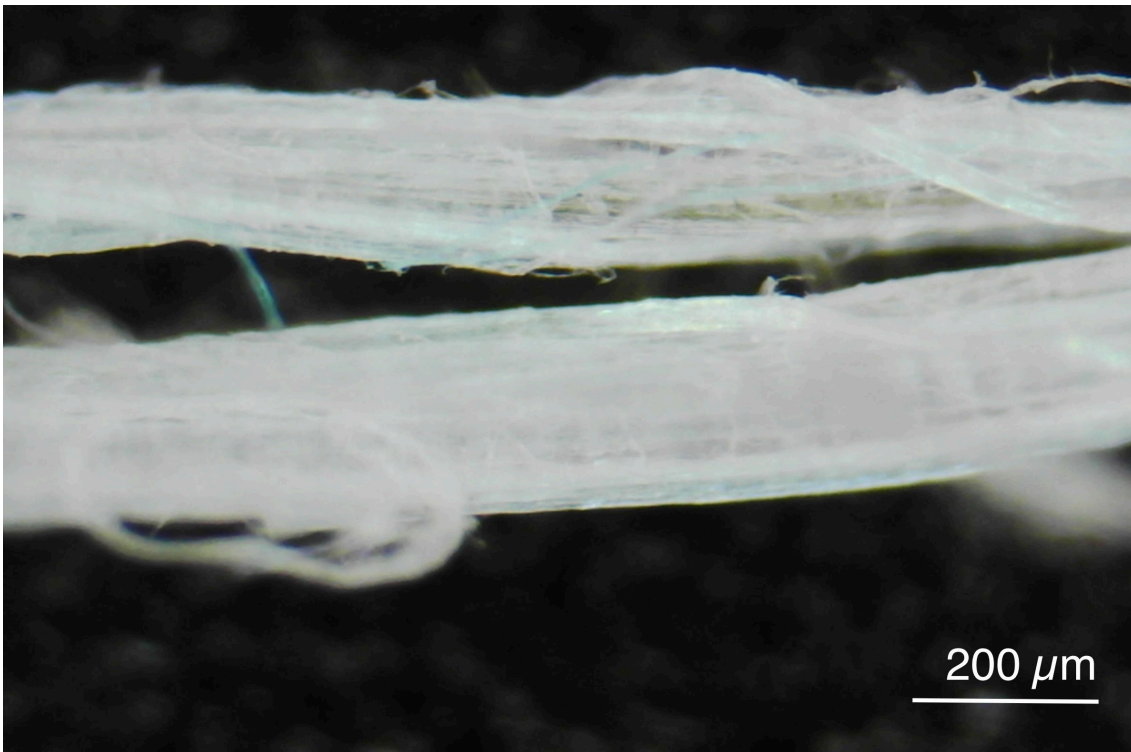


Figure 27: Experimental deer sinew displaying the frizziness also visible on the large fibre piece on C24118b.

All experiments were designed to put each type of use-wear on 3 faceted surfaces for comparison with the archaeological artefacts. The select mode was field experiments, which was chosen due to the fact that an interpretation of functionality was incorporated in the experiments, and they therefore needed to be conducted under functional conditions. However, none of these were intended as a replication of a past situation, as such are largely unknown. All experiments were conducted in the summer season of 2014 under weather conditions representative for East Norwegian Mesolithic conditions.

Granite stones of similar type, size and shape were sourced at a pebble beach in Østfold county, at the outskirts of a moraine (Raet) deposited by the last glacial retreat, in the Younger Dryas. Both Rørbekk 1 and Torpum 13 are located c. 5 km from this geological moraine zone, and situated on a similar moraine deposit (Onsøykomplekset) (Erikstad 1998; Jaksland 2003a; Jaksland 2003b; Lundqvist and Vilborg 1998). The collected stones should be considered representative of T13/1, T13/2 and R1.

#### 7.2.4. Experiment 2-1: production of unused artefact

These experiments were the first to be conducted, as they were considered necessary to assess whether the facet surface was prepared and used respectively. A pilot experiment was performed and upon evaluation did not require any changes to the procedure, so the experiment was performed according to the original scheme.

Three granite stones were pecked with discarded flint cores<sup>269</sup> The facets were easily pecked with the selected cores, and the granite was workable without any considerate effort. The experiment went according to plan and produced three stones with facets very similar to the archaeological specimen on a macroscopic level.



Figure 28: Pecking experiment: finished stones. From left: Stone 1, Stone 2 and Stone 3.

The micromorphology was however quite different. Whereas the archaeological stones showed pitting and a rounding of crystal and interstice contours, the experimental pieces produced a roughened surface with sharp edges, step fractures, cracks and frosted appearance (Figure 29) typical of fatigue wear resulting from vertical pecking (Adams 2002: 30). This was an expected outcome, and indicates that the archaeological facet surfaces experienced subsequent activity that produced additional wear.

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269. The cores were taken from the experimental waste heap, produced by students and flint knappers at University of Exeter, primarily on local flint or chert.



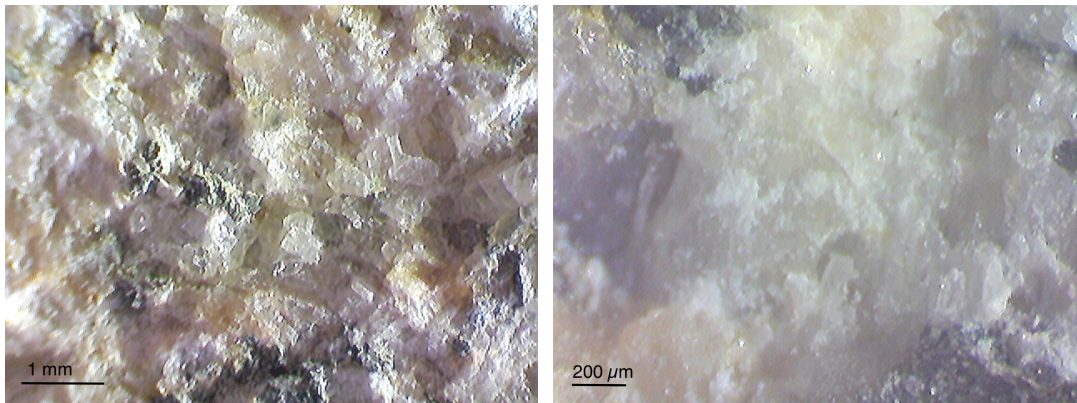


Figure 29: Experimental Stone 3 after concluded pecking experiment.

The pecking experiments were a good entry point for further experimentation with the use facets, and it became part of the overall experimental plan to prepare a pecked facet in subsequent experiments as the macroscopic similarity between the archaeological and experimental pieces were striking.

#### 7.2.5. Experiment 2-2: cracking hazelnuts

Another use task that was considered actualistic for the local context in the Mesolithic was the cracking of hazelnuts. Carbonised hazelnut shells are a common find category on Norwegian Mesolithic sites, although the sites in question did not yield any. However, the nearby Nøstvet settlements Torpum 9a and 9b, c. 500-1000 years older, produced a large quantity of <sup>14</sup>C dated hazelnuts shells (Rønne 2003: 153; Tørhaug 2003: Tabell 8), and the pollen charts show a continuous presence of *corylus sp.* (hazel) throughout the Mesolithic (Høeg 2002: Figur 46). Hazelnuts were also ubiquitous at Helganes (Solberg 2014: 125). As hazelnuts have a high calorific value (2664 kJ/100 g) compared to other food sources available, such as mussels (229 kJ/100 g), Norwegian common mushroom (92 kJ/100 g), raspberries (122 kJ/100 g), blueberries (215 kJ/100 g), cod (345 kJ/100g) and roasted elk (604 kJ/100 g)<sup>270</sup> it is likely to have been exploited also in the Kjeøy phase, to which Rørbekk 1 and Torpum 13 belong.

Hazelnuts must be cracked before consumption, but this can in principle be achieved with any implement that bears enough force onto the nut shell. The

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270. Calorific value of select foods retrieved from *The Norwegian Food Composition Table*, a database produced by the Norwegian Food Safety Authority. <http://www.matvaretabellen.no/?language=en> [Accessed 28/9/2014]

pre-experiment was designed to compare a pecked facet against an unprepared surface for functionality and wear. The nuts were cracked against a smooth, flat stone on soft ground.

Both the unprepared and pecked facets proved functional for the task, but the pecked surface displayed the advantage of a rough surface for increased friction. Nuts were less likely to slip away and the efficiency therefore increased compared to using the unprepared surface. Both surfaces quickly gained a macroscopically prominent, brown wear patch that formed a continuous area on the pecked surface which is not comparable with the archaeological specimen.



Figure 30: Stone 7 after pecking 2,3 litres of hazelnuts against each of the surfaces.

The experiment was repeated twice with pecked facets, both times the same brown patches formed.<sup>271</sup> Microscopic imagery of the patches produced enlarged expanses of levelled crystals, interspersed with sinuous interstices in the form of trenches (Figure 31). Neither on a micro- or macroscopic level did the experimental stones bear any resemblance to the archaeological pieces.

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271. Experiment details in Appendix E.

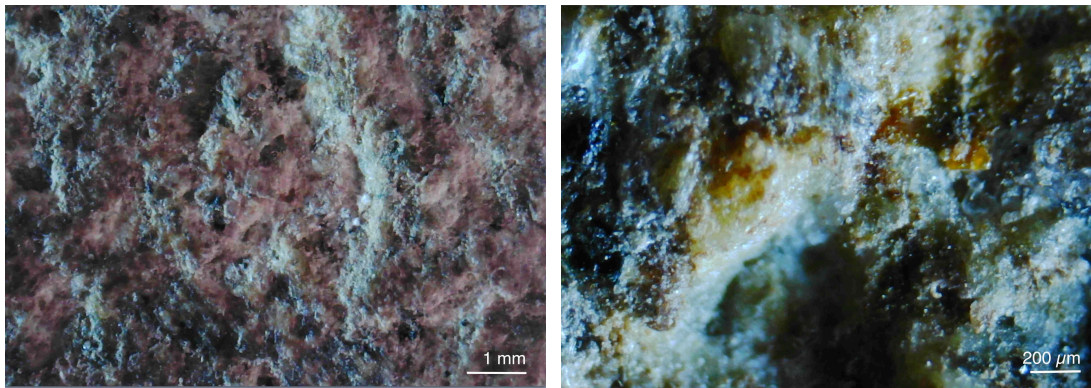


Figure 31: Stone 7 facet b (pecked) after 2,3 litres of hazelnuts. The lack of surface debris along with the frosted appearance is a result of the vertical cracking that produces fatigue wear with level areas.

### 7.2.6. Experiment 2-3: grinding experiment

As the pebbles are primarily interpreted as grinding stones, the prolongation of Experiment 2-2 was to grind the cracked hazelnuts. The experiment was executed on the ground, and a large granite slab was used for this purpose.

Both the grinding stone and the faceted pebble (9) quickly clogged up by the oily nut matter, and had to be cleared repeatedly to produce any grinding effect at all. However, the natural, smooth surface of the pebble worked very well, as could be expected from typical grinding stones from the Norwegian Meso- and Neolithic. On the untreated surface, classical grinding striations formed rapidly, and the surface did not clog due to the lack of friction/adhesion. The pre-experiment was decisive enough to not pursue further experimentation, and was concluded after 1134,1 g and 1111,5 g nuts respectively.

Microscopy confirmed that the experimental grinding surfaces and archaeological pecked facets are very dissimilar. The experimental pecked facet was almost completely level, with a waxy appearance and expected surface debris.



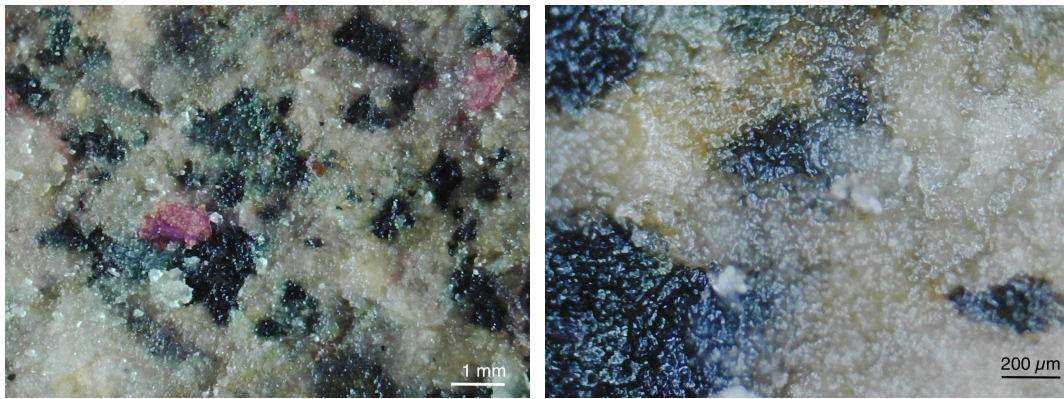


Figure 32: Pecked surface after hazelnut grinding. The interstices are completely obliterated and in their place is a level surface with a waxy luster and scattered debris.

### 7.2.7. Experiment 2-4: pounding sinew

Before sinew can be used, it needs to be prepared, and a chaîne opératoire could be separation from carcass, scraping, cleaning, drying, fibre separation and resoaking. All these steps need various tools, and a smooth, handheld stone is highly functional for fibre separation. However, as the fibre on the (most likely Neolithic) C24118 b could suggest such use with the faceted stones, an experiment was set up to prepare sinew.



Figure 33: Left: experiment set-up for stone-on-stone experiment. Right: sinew separation after 20 mins.

A pre-experiment included two natural, rounded use surfaces and two pecked facets. Deer sinew was pounded against a birch trunk and a stone on soft ground respectively. This was done because a subsurface rebound could potentially force the pliable fibres further into interstices on the rock facet, and so create different quality of use-wear, and the two types of subsurface were

chosen to represent hard and yielding surfaces. The pre-experiment indicated that the fibres separated more easily on a hard surface that pummels the fibre from both sides. Furthermore, a pecked, rougher facet tore less into the outer sinew tissue than a smooth surface, due to an increased friction which prevented slippage of the tool on the tissues. In this way, the pecked facet produced a neater separation.

The remaining two experiments applied pecked facets to sinew on a stone subsurface with a similar high functionality and the same conclusion. The experiments did not result in distinct, macroscopic change to surface topography, however there was a slight smear towards the centre of the facets after the experiments which may be residue from the sinew. Nevertheless, microscopic investigation did show a strong similarity to the archaeological specimen, with rounded crystals, pitting and surface debris (Figure 34, Figure 35), and even sinew fibres lodged in the surface (Figure 36). In total, these experiments gave a good reference for the archaeological task.

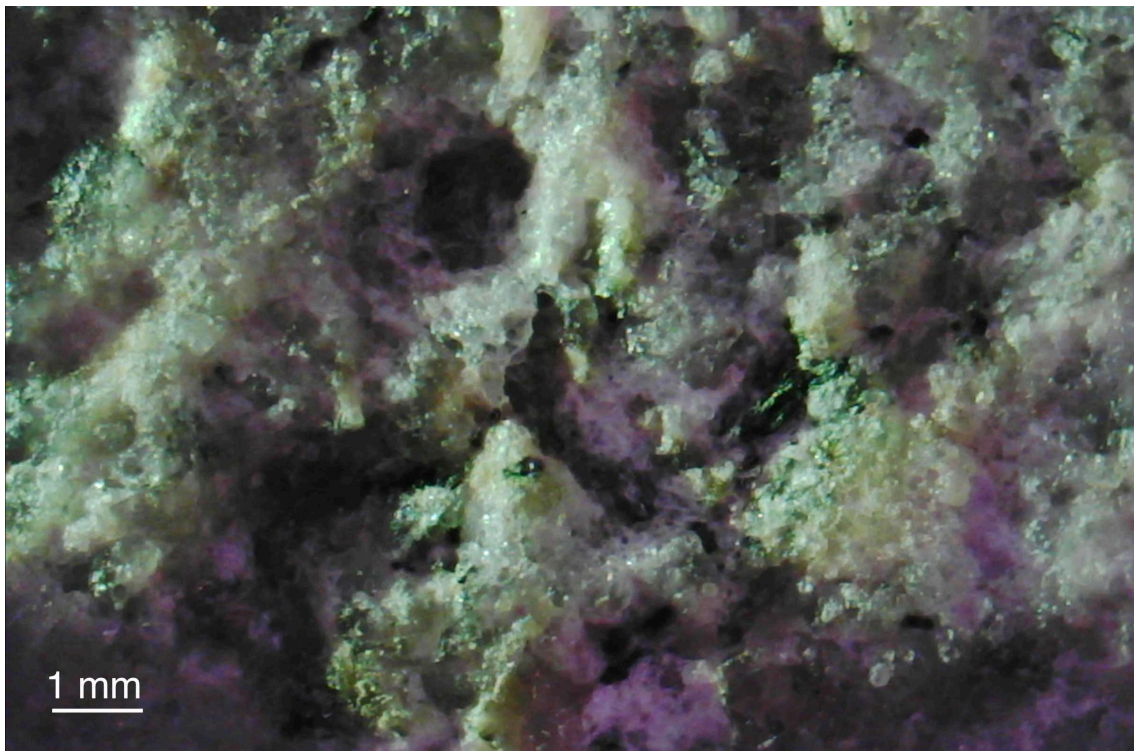


Figure 34: Wear trace after pounding deer sinew against wood for 1 hr. The results are interstices, crystal rounding and some surface debris.



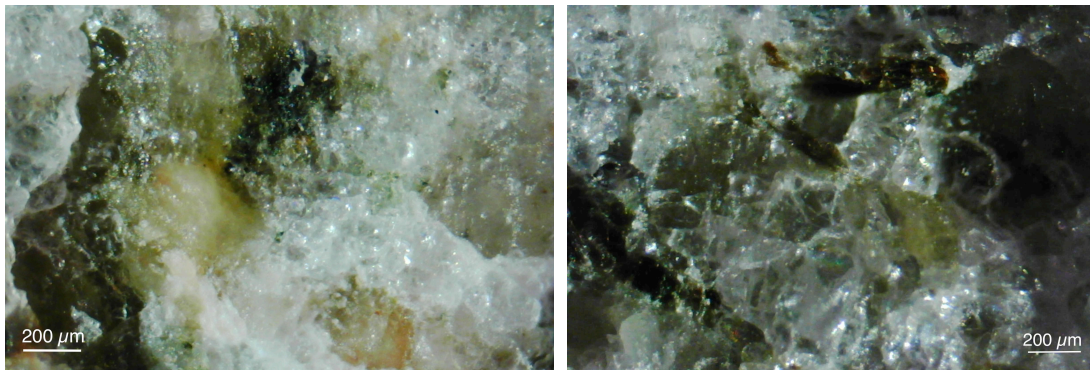


Figure 35: Substantial crystal rounding after 1 hr pounding of sinew against wood (left) and stone (right) subsurfaces.

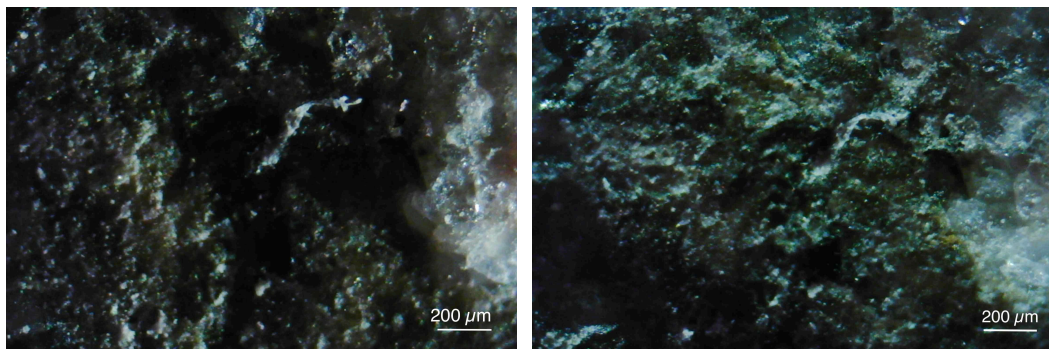


Figure 36: Experimental sinew fibre embedded in pecked surface (stone 5) after stone-on-stone processing.

### 7.3. Conclusion: intentional creations but for what?

The conclusion of the experiments point to sinew fibre as the at present best reference contact material for faceted beach pebbles. However, because hide-processing stones used in experiments are reported with similar features (Adams 1988), this is a use that should be tested before a conclusion can be made. Nevertheless, there is now reason to refute that faceted beach pebbles were intended as grinding-, anvil- and hammer stones, as well as nut-cracking implements; currently the interpretations found in the central database and literature.

The implements were probably only considered necessary in very limited numbers for each site, as opposed to for instance scrapers, which are automatically presumed to be the hide-processing tools of choice. However, this is in itself not without issues and biases of tradition (McDevitt 1994; Shott 1995)

It is possible that the low number relates to the idea of a singular work-station, alternatively that the one or two specimens fulfilled so many tasks that there was no need to make specialised implements. The experiments have shown that, although the pebbles are functional for nearly all tasks attempted, they do not seem to exhibit a build-up of layered, different wear patterns. Nevertheless, the stone could easily be re-pecked between uses. Even so, almost all the microscopically examined pebbles displayed wear that seems to coincide with sinew separation.<sup>272</sup> As seen in T13/1, it is possible that the pebble originally had one use-surface that could be expanded to two if necessary. Alternatively, the task(s) in question occurred very rarely.

So: the experimental results point to practice that involves a use of faceted pebbles as *at a minimum* sinew pounding stone or processing implement for similar pliable material such as hides. The proximity to scrapers at Torpum 13 may possibly connect the stones further to animal processing. Even so, from the author's experience, sinew separates more easily in a dried state, and its animal origin is not enough to put a sinew processing location together with fresh animal processing. However, scrapers can also be used on dry hides to help make them pliable and refine the skins.<sup>273</sup> Rather, it would be expected that sinew processing was part of composite tool production, which can also be connected to the hearth at Torpum 13. Especially considering how lashing and hafting was integrative to several interregional technologies in both the Mesolithic and Neolithic, it is perceivable that faceted pebbles were part of a (site-bound) toolkit for preparing sinew bindings. As the preparations for the experiments also showed, pecking of facets and sinew pounding can be quickly learnt, and it could be considered whether this was one of the tasks that anyone could undertake in the day-to-day life on the settlement. Why these stones are missing from the interior may be related to taskscapes, or possibly logistics; there may have been little gained by transporting the sometimes over 1 kg heavy implements, as a pecked surface of similar nature could be prepared on a large variety of stones. The notion of a pecked surface is a preconceived idea (horizon), in this case considered to correspond to a certain task where either the faceted stone itself was appropriate, and/or where other implements were

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272. See Appendix D.

273. This has been successfully executed by the author under the direction of Bruce Bradley,

inappropriate. This notion was probably conceived due to functional need, as the stones have decidedly been used after manufacture. If the stones were used for sinew pounding or skin rubbing, these are tasks with relatively low skill requirements. As the preparations for the experiments also showed, to peck facets, as well as pound sinew, can be quickly learnt, and it could be considered whether these were tasks that anyone could undertake in the day-to-day life on the settlement. The same is likely to be true for skin-rubbing. One could imagine that sinew/hide processing was a shared task between the inhabitants of Torpum 13, Rørbekk 1 and other sites, and that only a few stones were needed for these tasks. Alternatively, the high variation in size and the singular occurrence of the stones may be due to the personality of the task.

As the preparations for the experiments also showed, pecking of facets and sinew pounding can be quickly learnt, and it could be considered whether this was one of the tasks that anyone could undertake in the day-to-day life on the settlement.

The experiment was categorised in the behaviour/inter-use category, with an evidence level of E: direct, T: associated, and C: direct.<sup>274</sup> The evidence cannot be said to have increased from associated to indirect for technological practice until more experimentation has been undertaken. This experiment can therefore not be considered a successful experiment by the categorisation chart. Describing the societal intangibility, including the extent of sinew-pounding, hide-processing or other practices, is reliant on further research and/or increased contextual information about the stones. The experiment may conceivably lead to an increased level of evidence in the future, for instance if further experiments discredit an interpretation as possible hide-processing implements. Nevertheless, the research undertaken for this experiment has resulted in the knowledge that the practice was shared over a larger region. This is valuable information about the intangible aspects of the practice that involves the stones, for instance in the form of exchange of ideas, and the application of similar technologies and possibly subsistence strategies within a larger group. Nevertheless, the faceted stones remain faceted stones, and we may at this point know more about what they are *not* than what they are.

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274. See Table 3.

## **8. Manifesting ideas: Neanderthal birch bark tar production**

The last case experiment will tackle a research situation in which barely any circumstantial evidence exists. The cultures in question cannot be specifically identified, and additionally, less can be said about their specific subsistences and ways of life than for most other human cultures. Where the other case experiments dealt with tangible evidence for a technological operation, the current case study leans on only the end result - tar - of an unknown process. The question becomes whether or not this is sufficient basis for an experiment that seeks information about the intangible.

### **8.1. Case study 3: Neanderthal birch bark tar**

Birch bark tar is an ancient product. Its history stretches from the Middle Palaeolithic until modern times, and its use takes many forms; from adhesive agent to waterproofing matter, and even potentially as a masticant medicine (Aveling and Heron 1999; and see Groom et al. 2015). Birch bark tar in its processed form, birch pitch, may well be the first refined product produced by humans (Grünberg 2002: 15), and the earliest finds possibly date back to c. 250-260 000 BP<sup>275</sup> (Mazza et al. 2006).

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275. Pers. comm. Paul Mazza, 1/11/2013.

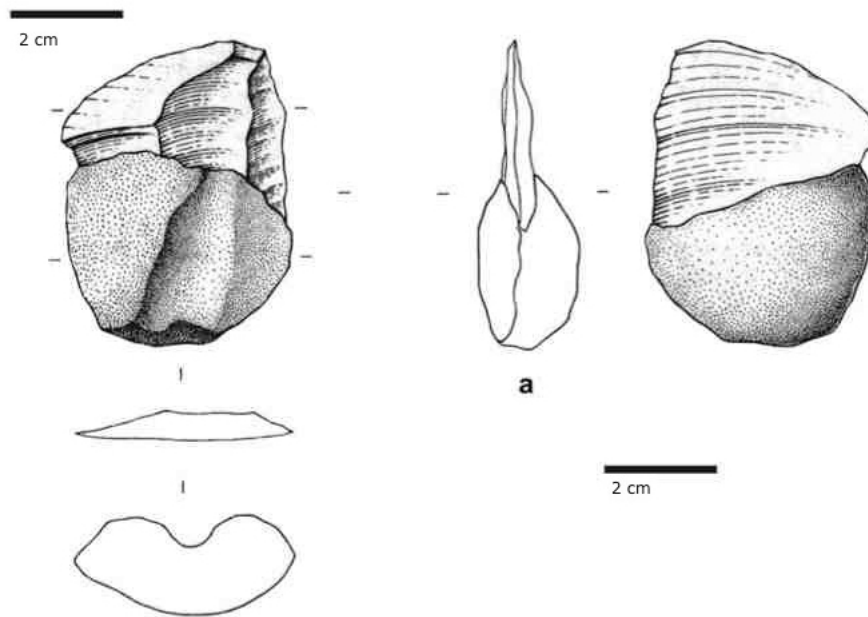


Figure 37: Mousterian flint flake embedded in birch bark tar, Campitello quarry, Bucine, Italy. Illustration courtesy of Mazza et al 2006.<sup>276</sup>

These extraordinary finds come from Campitello quarry in the Upper Valdarno Basin, Italy, where a flint flake embedded in birch bark tar was discovered in 2001 (Figure 37). A second flake had birch bark tar on the ventral aspect.<sup>277</sup> The flakes were of a generic nature and hard to date typologically, but were intermixed with bones of a straight-tusked elephant,<sup>278</sup> voles<sup>279</sup> and mice,<sup>280</sup> which in combination indicate a specific stadial phase before the end of the OIS<sup>281</sup> 7 (*Ibid.*; Roebroeks and Villa 2011).

Another group of finds stems from the Micoquian site Inden-Altdorf in the Inde Valley, Germany, where 81 artefacts dated to c. 120 000 BP yielded small

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276. Reprinted from *Journal of Archaeological Science* 33/9, Paul Peter Anthony Mazza, Fabio Martini, Benedetto Sala, Maurizio Magi, Maria Perla Colombini, Gianna Giachi, Francesco Landucci, Cristina Lemorini, Francesca Modugno, Erika Ribechini. "A new Palaeolithic discovery: tar-hafted stone tools in a European Mid-Pleistocene bone-bearing bed," p 1310-1318, Copyright (2006), with permission from Elsevier.

277. A third flake was associated, but without tar residue.

278. *Elephas (Palaeoloxodon) antiquus*

279. *Clethrionomys cf. glareolus*, *Arvicola cantianus*, *Microtus (Terricola) gr. multiplex-subterraneus*, *Microtus arvalis*

280. *Apodemus sylvaticus*

281. Oxygen isotope stage

amounts of a black residue that analyses confirmed to be birch bark tar. The residue was observed on a wide spectrum of artefacts, from scrapers to projectile points. Many implements were also associated with hafting use-wear (Pawlik and Thissen 2011b).

Notable finds of birch bark tar also come from Königsau in Saxony-Anhalt, Germany, dated to c. 50 000 BP.<sup>282</sup> At Königsau, a site excavated in 1963-64, one specimen, *Königsau B*,<sup>283</sup> forms an oblong lump, seemingly kneaded by a hand, and is decidedly Mousterian. The other specimen, *Königsau A*,<sup>284</sup> is assigned to the Micoquo-Prodnikien culture, and has imprints of both a flint blade and a wooden haft, indicative of its adhesive function (Grünberg 2002; Koller et al. 2001).

Both the Campitello quarry specimen and the Königsau pieces have been confirmed to constitute of birch bark tar through GC/MS<sup>285</sup> analyses, which confirmed the presence of the triterpenoids *betulin* and *lupeol*, clear biomarkers of birch bark tar. Other biomarkers varied between the sites (Koller et al. 2001: 390-393; Mazza et al. 2006). The multitude of finds from Inden-Aldorf have been analysed with optical microscopy, SEM<sup>286</sup> and EDX,<sup>287</sup> and hafting wear was identified in combination with what turned out to be a birch bark tar with incorporated organic fibres and other plant tissue (Pawlik and Thissen 2011b).

As birch bark tar is produced through controlled *pyrolysis*, the discussion on production procedure was instigated, and experimental archaeology has been put forward as the method for answering this question (Grünberg 2002: 16). The debate is lively, although as of yet, no-one seems to have published period-relevant, experimental results that produce uncontaminated birch bark tar like the Königsau A piece (Koller et al. 2001).

Some procedures for the production of small amounts of birch bark tar are well

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282. AMS-dating of Königsau A (stratigraphically older) was 43 800 BP and of Königsau B; 48 400 BP.

283. Catalogue no HK 63:150/0 Landesmuseum für Vorgeschichte, Sachsen-Anhalt

284. Catalogue no HK 64:1/0 Landesmuseum für Vorgeschichte, Sachsen-Anhalt

285. Gas chromatography/mass spectrometry

286. Scanning electron microscopy

287. Energy dispersive X-ray spectroscopy



known, but most employ some kind of airtight, fireproof container (*retort*) such as ceramics or metal tins, of which neither have a particularly long history (Pawlik 2004: 173; Weiner 2005: 25-26). However, as birch bark tar must undergo pyrolysis under controlled circumstances, it is unlikely that it was produced by coincidence to produce such a high number of haftings. The process takes place in an anoxic atmosphere in the general temperature span of 250-400 °C. If the threshold is exceeded or oxygen is allowed to enter the pyrolysis chamber, the tar will burn away. However, if the temperature is too low, the transformation will not occur (Charters et al. 1993; Koller et al. 2001: 393; Meijer and Pomstra 2011). In other words, the procedure demands full control and the attention of the producer. Currently no preceramic tar-extraction structures are known, although bonfire remains from the Neanderthal site *Abric Romani* in Capellades, Spain (70000-40000 CalBP) have showed a presence of liquid hydrocarbons, interpreted as fuel, but not a by-product of the predominantly coniferous wood (Courty et al. 2012).

The question remains whether a birch bark tar production event could feasibly have occurred without leaving much archaeological trace. In the following, this problem will be tackled experimentally, as the second part of a series of experiments that started in 2009 (Groom et al. 2015). If a method for tar-extraction could be constructed that would leave scant or no archaeological marks (Meijer and Pomstra 2011), that could provide evidence that Neanderthal birch bark tar extraction may not be an accident that was sourced opportunistically, but was rather an event that required a firm chaîne opératoire to be able to provide necessary control.

## **8.2. Case experiment: period relevant birch bark tar extraction**

### **8.2.1. Experimenting with tar-distillation in untraceable structures**

Pyrolysis, or dry-distillation, is the destructive burning of organic matter into char, volatiles (distillates) and non-condensable gases in the absence of oxygen or air. When applied to birch bark, this thermal conversion process transforms bark into char and tar. The latter can be fractioned into a dense tar pitch and

volatile tar oils, a separation that happens by sedimentation of the heavier pitch to the bottom of a receptacle (Tiilikkala et al. 2010: 112). In order to be used as an adhesive, the mixture is usually boiled to evaporate the volatile compounds and extract the pitch, an adherent substance that solidifies upon cooling<sup>288</sup> (Pollard and Heron 2008: 236; Weiner 2005). Upon cooling, the tar will set firm, but can be re-melted for application. When set, the tar can be rather brittle and may shatter upon impact. To counter this, it was sometimes mixed with beeswax, which improves its plasticity (Regert et al. 2003a: 1628).

To produce birch bark tar using pottery is a straightforward procedure that was probably adopted in the Neolithic onwards, and may be reflected in finds of ceramic vessels with birch bark tar residue (Ottaway 1992: 691-692; Urem-Kotsou et al. 2002).<sup>289</sup> Tar can be produced using a single-pot or double-pot method. A single-pot method is often performed by the distillation of bark under a cover such as a metal can. The container requires ventilation holes to allow for the evaporation of volatiles, which results in a ready-to-use adhesive that has been processed post-distillation within the bark chamber. This tar is frequently contaminated with charred bark, or sediments if the bark is placed on the ground rather than contained within a vessel (Weiner 2005: 25-26).

The double-pot method requires two, stacked, airtight containers connected through drain holes. Pieces or strips of dried birch bark (or wood) are placed in the topmost vessel, which is subsequently sealed, and a fire is then lit around this structure (Piotrowski 1999). The author has accomplished this successfully in as little as 15 minutes on a particularly windy day, when the fire gets to a high temperature rapidly, but experience shows that the procedure for one small flowerpot<sup>290</sup> filled with bark normally takes around 30 minutes once the fire has

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288. However, in a single-pot distillation, this reduction happens as part of the procedure, as the tar is kept in the heated pyrolysis chamber.

289. As it appears, the sherds were encrusted with birch bark tar, and these particular vessels may also have been involved in other parts of the process such as a subsequent thickening of the tar. Experience shows that the dry distillation does not necessarily leave tar residues in the pyrolysis chamber walls other than around potential drainage holes. If involved in the chaîne opératoire, the pots have in other words been a receptacle for the tar.

290. Capacity approximately 1-2 litres. The author usually seals the pot with a terracotta saucer and a mixture of 1:1 sand/clay, left to dry overnight.

caught properly. The procedure is usually complete when, or just after,<sup>291</sup> the prominent, acrid odour of tar is noticeable. Archaeological finds of pierced and/or tar-coated pots have been found, and may indicate this procedure (Regert et al. 2003b: 1628).

The double-pot method produces an uncontaminated, liquid tar that drains out of the bark container as soon as the pyrolysis is complete. This method transforms the bark to tar and charred bark remains, and the reducing atmosphere leaves the interior of an untreated pot black and glossy. The tar separates into the thin, yellow, and volatile oil, floating on a dense, black pitch, presumably processed by boiling as described above.

As of yet, not many researchers have experimented with ceramic tar production. Those who have achieved tar without air-excluding containers have either not published their experiments; rely on *de facto* containers or structures that are likely to have left distinct archaeological trace, or have not necessarily produced tar as such (Czarnowski and Neubauer 1992; Osipowicz 2005; Palmer 2007). Most often, the amount rendered is very small (e.g. Meijer and Pomstra 2011).<sup>292</sup> Additionally, few authors have maintained a connection to the discourse on hominin cognition, under which the question of cognitive levels of Neanderthals and early humans is an ongoing debate (e.g. Wynn and Coolidge 2012; Fiore et al. 2015; Morley 2014; Speth 2015). Lyn Wadley (2010: 111-119; 2013) regards the manufacture of adhesives through the processing non-adhesive substances as a benchmark for complex behaviour. In addition, the use of sophisticated protocols that require augmented working memory may be an indication of planning skills (2010: 116; 2013: 173). However, the exact cognitive capacities of humans 250 000 years ago are still hard to determine. As the procedure required for pyrolysis of birch bark tar is in itself complex, the

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291. This depends on the circumstances; the wafts of odour may for instance be travelling in the direction of the wind, where experimenters, due to smoke, are less likely to stay during the firing. As has been demonstrated in experiments where the author has participated, it is also highly dependant on personal sense of smell, and whether or not the experimenter notices immediately or at a later time.

292. It has been claimed that the greatest problem for Neanderthals in this regard was the lack of retorts for the pyrolysis (Koller et al. 2001: 394). However, this must rather be considered the greatest problem of modern experimenters, as we will most often have a pre-supposition (horizon) of retort-using derived from initial research, even before we experiment ourselves. Conversely, Neanderthals did not inhabit a world with (the same) airtight containers, and consequently had different horizons to build their experiences and experimenting on..

following experiments with tar production were designed to explore the simplest possible way to produce tar through pyrolysis, without an airtight container.

Regardless of cognition and missing diagnostic structural features, there are certain structures that have piqued our interests considering Neanderthal pyrotechnology. From Abric Romani in North-Eastern Spain, a site with many Neanderthal household sized hearths, fireplaces have yielded mineralogical evidence of a production of liquid hydrocarbon that may be connected to "moderate heating at a few hundred degrees with no flames in a limited supply of air oxygen" (Courty et al. 2012: 309); in other words slow pyrolysis. The geological/stratigraphical dating for this site was c. 70-40 000 BP, the same approximate period that the pieces at Königsau were produced.<sup>293</sup> The authors propose that the combustion structures indicate a variety of specific firing activities (*Ibid.*: 295, 309). Birch bark tar production seems to be a task that could fit this frame of reference. As such, the size of the structures was based on known hearth sizes of Neanderthal origin (Preece et al. 2006; Roebroeks and Villa 2011). In this way it was hoped that the experiments could achieve a minimum period-relevant format without interfering with the discourse on cognition. Nevertheless, the experiments should not be taken as an argument for a low cognitive capacity in humans of the time.

### **8.2.2. Interpretational chain: evaluation of the archaeological chaîne opératoire**

As no identified pyrolysis structures exist to be analysed, a generalised chaîne opératoire for the process must be interpreted from the tar samples themselves. Apart from GC/MS analyses that show the molecular composition, such as *Betulaceae* specific biomarkers that occur in birch bark tar, little has been published about the samples themselves. Based on the accessible information; the tar fractures conchoidally, typical for set, brittle tar (e.g. Pawlik 2004: Figure 19.14), and seems structurally uniform (Mazza et al. 2006: Fig. 3). The available samples seem to not comprise the heavy pollution of organic debris reported in Osipowicz's experiments (2005: Photo 6). The Campitello tar was in other

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293. However, the discrepancy to the 48 000 BP Königsau pieces may still be substantial, as the dating of Abric Romani is not specifically determined.

words skilfully made, possibly refined, by people who knew their chaîne opératoire well.

The Königsauwe pieces show a high proportion of betulin, a triterpenoid biomarker for birch. The amount can be both indicative of a production temperature span, the age of the bark, and sometimes of species. In the case of Königsauwe, the proportion may be connected to *Betula pendula*,<sup>294</sup> which has the highest reported proportions betulin of any birch species, or possibly of the use of young bark (Koller et al. 2001: 392-393; Pollard and Heron 2008: 249).

The samples from Inden-Altdorf show evidence of exposure to ash, probably from the structure, the subsequent collection, or situational to the hafting chaîne opératoire. Many of the pieces also seem to indicate plant and sediment debris in the morphometric structure (Pawlik and Thissen 2011b: 1706).<sup>295</sup> Inden-Altdorf was also the only site hitherto known to produce tar adhering to what may have been part of the production procedure, in the form of three flat pebbles; one is c. 5 x 10 cm and has <100  $\mu\text{m}$  of birch bark tar covering parts of the surface (Pawlik and Thissen 2011a: Fig 7f).<sup>296</sup>

Other than the tar finds, which due to a vast difference in time probably indicate three different chaînes opératoires with different horizons, norms, and social meaning, there is little to base the interpretation on. However, if we presume that tar production was intentional, the finds allow us to set up a certain imprecise procedure which should be valid for all three cases:

- 11) The tar was deposited under unknown circumstances
- 10) The tar was used
- 9) (The tar was refined)
- 8) The tar was collected
- 7) The bark underwent pyrolysis
- 6) The bark was heated

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294. English: silver birch

295. As the tar samples from Inden-Altdorf did not undergo GC/MS, they cannot be directly compared to the two other Middle Palaeolithic finds in terms of composition.

296. These stones are reminiscent of stones used in certain experiments, typically for catchment, such as in Meijer and Pomstra 2011; Palmer 2007.

- 5) A bonfire was built on or near the bark deposit
- 4) The bark was deposited in an airtight structure<sup>297</sup>
- 3) The bark was possibly dried<sup>298</sup>
- 2) The bark was collected<sup>299</sup>
- 1) The bark was recognised as useful

Based on the reverse chaîne opératoire, it is possible to set up a multitude of experiments with tar distillation. However, whether it is possible to connect experimental results to either of the Palaeolithic tar samples is questionable and will be discussed together with the experiment results below.

### 8.2.3. Experiments series 1: summary

The first set of experiments took place in 2009 as an exploration of the use of pit structures for birch bark tar production (Groom et al. 2015; Groom et al. 2015; Schenck 2011). The attempt was unsuccessful, but yielded an interesting trend towards a functionality of raised structures that was pursued in a second set of experiments. In actuality, the pit-experiments served as an introduction to pyrolysis without retorts, and consequently provided experience and guidance for the subsequent experiments.

Experiment: *Aceramic production of birch bark tar – pit structures.*

Experiment type: Field

Variable control: None

Period: August 2009

Location: Land of Legends, Lejre, Denmark

Participants: Tine Schenck (experimental archaeologist), Peter Groom (environmentalist, experimental archaeologist), Grethe Moéll Pedersen (archaeologist)

Participant skill level/technology: Double-pot method (Tine Schenck, Grethe

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297. This structure could be formed by the bark itself, as demonstrated by Meijer and Pomstra 2011

298. Drying bark is a typical step of modern-day birch bark extraction, but it is unclear whether this is strictly necessary and what it achieves in terms of functionality, composition, and resulting tar. Osipowicz (2005) has showed that this is not a necessary step to produce useful, tar-like substance. He did, however, recount that the output was a compacted, heavily polluted lump, rather than the normal viscous tar (or pitch in a singular-vessel process).

299. On dead birches, which typically degrade from the inside out as the bark is so resistant to decay, this may consist of simply picking off some soft pieces of wood. On live trees, it is better to collect the bark in spring due to the excessive sap levels that renders the bark flexible. However, personal experience has shown that even during Norwegian winters, when the bark clings tighter to the tree trunk, it is not impossible to collect live bark.

Moéll Pedersen). Academic knowledge of plants and plant technologies (Peter Groom).

Research focus: To create a non-ceramic, airtight pyrolysis pit.<sup>300</sup>

Hypothesis: Birch bark tar can be produced in simple pit structures.

The initial round of experiments was carried out in August 2009 at Land of Legends, Lejre,<sup>301</sup> Denmark. The overall idea was to create an airtight chamber without the aid of a container, and small pits were selected as a starting point, as they were thought to reflect a cognitively simple idea for production, as well as an undiagnostic feature that could have been obliterated through time. A sequential experiment design was intended to respond to provisional results. This led to the use of internal rather than external parameters as a basis for succeeding structures, as no external parameters exist. This also meant that the initial structures were very different from the final firings<sup>302</sup> (Figure 39), and all were subjective.



Figure 38: Pit experiment S1 (Eric). From left: initial construction with run-off channel. Bark fill caked together after firing; the partly pyrolysed top and only slightly affected bottom.

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300. The first set of experiments were initially designed after a tar-producing pit structure found at Biskupin site 6, Poland (pit IIIa), dating to the early Medieval Period. The structure was a conical pit; D: 85 cm, Ø: c. 80 cm, and was found with burnt remains of birch bark tar in the infill. The pit was interpreted to be used with pottery, as was later demonstrated in experiments to work very well for birch bark tar production (Piotrowski 1999). However, no pottery was found in association with the structure, and it was thought that this might provide a beginning point for birch bark tar production in pits

301. Formerly *Lejre Experimental Centre*

302. This was also why we elected to publish the experiments under the heading of 'explorations.'

From the initial, exploratory experiments, none seemed successful. However, throughout the experiments, a strong tar odour could be detected, which was ascribed to the separation of volatiles and pyrolysable products that occurs in the process. In hindsight, and after the second (successful) sequence of experiments, some of the charred remnants found upon excavation in series 1 had been probably indicative that pyrolysis had occurred in parts of the bark. No tar was recovered, but as no receptacles were placed in the structures, any limited amount may have drained into the subsoil.

A total of 14 experiments with 8 types of structures were executed in pairs, to allow for as similar weather conditions as possible between two exemplars of roughly the same structure. Each pair was meant to include variations of one structure type.<sup>303</sup> As a comparison to the consequently unsuccessful pits, the two last structures were raised above ground as a potential way forward. Unsurprisingly, the small heaps of sand with birch bark inside were much better exposed to sufficient heat, and the bark clearly showed signs of a beginning pyrolysis in the form of a glossy, tarry substance on the bark. We had already deduced that this transformation of the bark was directly related to heat exposure time from three experiments designed to isolate this variable specifically,<sup>304</sup> but had problems with heat penetration into the pits. The standing structures seemed to provide a better solution to the problem, and we decided to continue with such structures in series 2.

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303. However, in one instance (S7), a singular, raised structure was tested independently, and S6-S8 were tested as a trio under the same circumstances.

304. Experiments S8-S10, see (Groom et al. 2015). Although the exposure time was isolated, as these were field experiments, true, scientific isolation was not possible, and we did not consider this hypothetical-deductive.



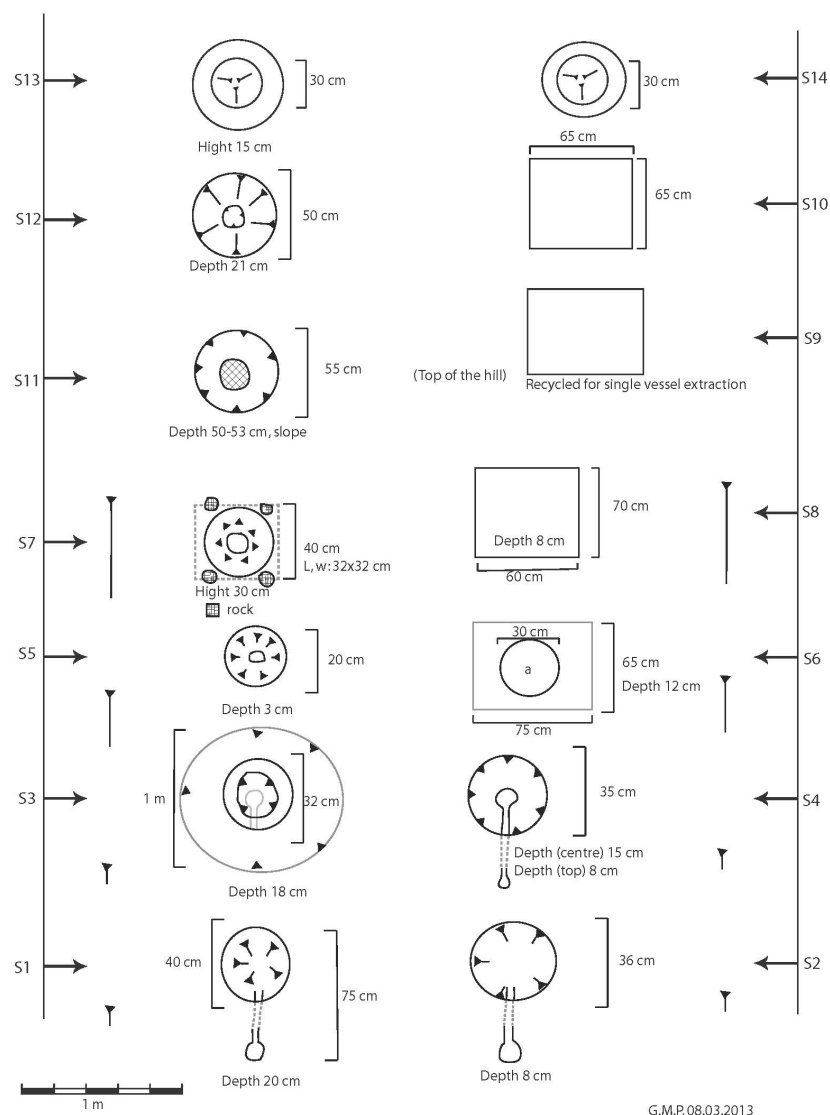


Figure 39: Plan sketch of exploratory, first sequence of experiments. Illustration by author and Grethe Moëll Pedersen, courtesy of Groom et al 2015: Fig 2).

## 8.2.4. Pyrolysis in standing structures

### 8.2.4.1. Preparations series 2

Series 2 was wholly dedicated to standing structures, based on the tendencies observed with S13 and S14 in series 1 (Figure 39). Since pits were deemed unsuccessful<sup>305</sup> due to insufficient heat penetration of the soil cover and the lack

305. However, other authors, notably Roel Meijer and Diederik Pomstra (2011) have been successful with tar production in a shallow pit with ash as cover. It is unclear whether the pit included charcoals, or it they were placed on top of the structure. As we were trying to work with 'untraceable' structures, we did not consider to work with charcoal *in* the pits, as charcoal is often well preserved and visible in structures. Nevertheless, charcoal filled pits are easily (mis)conceived as cooking pits, and we cannot exclude that such structures may have been birch bark tar production pits in the past.

of an enveloping heat source, the idea was to create heaped structures in order to surround the bark with as much heat as possible. The structures were designed as small sand<sup>306</sup> mounds with an encompassing fire. The chosen construction material was building sand,<sup>307</sup> as sand had previously set hard on its own in the duration of the firing, and no plasticising agent would therefore be necessary to maintain the structures. Previously, ours and others' (Osipowicz 2005) attempts have relied on clay, but as clay may come up to sintering thresholds when sitting directly in a fire<sup>308</sup> and would hence leave archaeological trace, we decided to omit it along with any other material that would not completely disintegrate after firing. As the experiments proceeded, the structure size was decreased to encourage full fire coverage. A larger heap could possibly be more actualistic in terms of output since the tar yield was very low in the small, experimental structures. Nevertheless, we decided to focus on the actual pyrolysis procedure rather than actualistic capacity, which is equally unknown.

The research aim was to create the necessary heat penetration and airtight conditions by the simplest means possible, and the only materials used would be birch bark, sand, and firewood. A small, mounded structure of sand would ideally create approximately the same conditions as a single-vessel structure, although the structures were not designed with vents as is often done to encourage volatile evaporation (e.g. Weiner 2005: 26). In one sense, a mound of sand is the simplest form of a kiln. The structure works along the same principles of enclosing a space to control the temperature by using an uncontrollable heat source such as a bonfire. As the temperature of a bonfire can vary with several hundred degrees within the same fire and thereby creates a difficulty with control (Gosselain 1992a), a kiln-like structure assures that a certain temperature range can be withheld over time. As it is also necessary to create an atmosphere devoid of air for the duration of the process, an enclosed chamber is possibly the most reliable option. Sunken features seemed to lack

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306. Based on the geological definition of sand, defined by particle size  $\varnothing$  0,063-2 mm rather than mineral composition (*Geoleksi*, online geological encyclopedia from the University of Oslo. <http://www.nhm.uio.no/fakta/geologi/geoleksi/> (accessed 5/8/3015)

307. Dially's building sand with grits

308. See section 6.2.4..

some penetration capacity,<sup>309</sup> and the kiln-like raised mound both seemed like a necessary and simple design. Series 2 was designed as field experiments, this time with minimal change in structure, to fully understand the process before we proceeded into a new phase. The series was designed to execute one experiment daily, with subsequent excavation and evaluation. One day was set aside for planning and discussion; to detail the methodology and documentation procedure, and to set up the experimental area. In addition to Tine Schenck and Peter Groom from series 1, Sharon Hartwell, horticulturalist, also participated in the experiments.

For measurement, a Jenway 220 Temp Meter Thermocouple and a handheld Anemometer with integrated thermometer from LaCrosse Technologies were used for 10-minute interval readings of chamber temperature and windspeed. Outside temperatures were measured every 30 minutes, and a standard low-tech, dial hygrometer were used at the beginning and end of each experiment. The experiments were documented with a Canon EOS500D DSLR camera, and the structures were drawn after excavation.

#### **8.2.4.2. Experiments series 2<sup>310</sup>**

The experiments were conducted in October 2013 in Stafford, UK. A private, fenced off garden area was chosen for the experiments, and sheltered from rain with a tarpaulin. Although not particularly exposed to wind, a good breeze was present on most days, which helped raise the bonfire temperature. To form matching foundations for all experiments, a c. 5 cm deep layer of builder's sand was added to the underlying gravel before any structures were set up. This layer would build up during the course of the experiments, due to the disintegration of previous experimental structures.

The first two experiments were designed as pilot experiments to explore two possible structure types. Experiment 3-1 – a simple, mounded heap of damp sand with a horizontal bark cylinder<sup>311</sup> in the centre (Figure 40, for details, see

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309. Even after 6 hours of high intensity firing, a singular sheet of bark was only beginning to pyrolyse (Groom et al. 2015: Fig. 7).

310. For full temperature profiles and windspeed, see Appendix F.

311. L 10 cm,  $\varnothing$  5cm.

Table 6) – showed most potential and displayed signs of having achieved pyrolysis.



Figure 40: Constructing the heap of sand that was used in experiments 3-1 and 3-3 to 3-5. This consisted in placing the tightly rolled bark cylinder flat on the sand, and simply heaping damp sand around it. The structure was built up with a view to cover thickness, and we took measurements consistently to ensure the correct thickness.

Temperatures were not monitored in this exploratory phase. This allowed for the minimising of potential variables, e.g. accidental disturbance of the structure by use of the thermocouple. Tar odour could be detected after 20 minutes of extensive fire coverage, but as tar odour was a feature we encountered consistently in series 1, we decided to let the fire go for a few hours more. This was the only experiment in which we terminated the experiment based on intuition, as the remainder were concluded after the appropriate temperatures had been achieved long enough for pyrolysis to have completed.<sup>312</sup>

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312. This was deduced from previous experience with double-pot firings; albeit without thermocouples.



Figure 41: Initiation of experiment 3-1.

Upon excavation, it became evident that reduction had been achieved through the 5 cm sand cover, as the sand around the bark roll was black and hardened. Although effectively encapsulating the bark, the sand nevertheless disintegrated completely upon excavation. The bark cylinder was almost exclusively affected on the upward facing surface, where the fire had been burning throughout. Upon opening the cylinder, several layers were fused together by a glossy, burnt substance, presumably tar. However, a response to insufficient heat distribution, the structure size was reduced in subsequent experiments to allow for full envelopment of the structure while maintaining bonfire size.<sup>313</sup>

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313. Due to health and safety risks, we were not able to expand the bonfire size.





Figure 42: Bark with tar after experiment 3-1.

Experiment 3-2 was a pilot attempt to introduce a flat stone slab at the bottom of the structure to reflect heat upwards into the bark chamber while at the same time collecting the tar (Figure 43, see also Pawlik and Thissen 2011b: Fig 3). The thermocouple was positioned in a copper tube for protection and inserted between layers of bark. However, as the sandstone slab cracked severely due to thermal stress at a chamber temperature of c. 120 °C an hour after ignition, it was decided to abandon this method in favour of the successful, simpler first structure, with fewer elements to control. As the primary aim remained to produce tar in the simplest way possible, we decided to uphold the original idea of a structure with only three components: sand, bark and fire.<sup>314</sup>

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314. Even if abandoned, 3-2 produced a faintly tar smelling, discoloured bark that was still more or less in place, and this structure should be further explored with appropriate materials. Meijer and Pomstra 2011 reiterates several successful experiments with this type of structure, constructed with a quartzite slab.



Figure 43: Experiment 3-2, internal structure.

Experiment 3-3 was the first double firing (Figure 44), and structure 3a was the first simple sand mound in which temperature data was gathered. Since heat penetration was evidently the primary obstacle during the pit firings, 3-3 was also designed to explore cover thickness. The thinnest cover possible would presumably be the most economical choice in terms of fuel efficiency, but it was thought that a very thin cover would be difficult to control with a friable and delicate sand structure that would also need to withstand firewood movement and potential thermal stress.





Figure 44: Experiment 3-3. From right: 3--3a (5 cm cover T), 3-3b (3 cm cover T).

Resulting from our observations in series 1 and the pilots, 3-3a was set up with 5 cm and 3-3b with 3 cm cover thickness. The difference was to ensure one structure would survive. Ideally, both would be sufficiently controllable to produce the necessary temperature span. The thermocouple was inserted into the 5 cm mound (3-3a) through a copper tube, with a gap of <1 cm to prevent tar pollution. This structure would function as an assumed minimum temperature gauge as the heat penetration was expected to be more effective through the 3 cm layer. A single fire was built around the two structures.

The temperature rose steadily, and although with occasional plateaus, the experiment was terminated after 4 hours when liquid tar accidentally started trickling through the thermocouple tube at a temperature of 320 °C (Figure 45). However, as the sensor was placed at the bark chamber floor, this can only be considered a minimum temperature for the pyrolysis chamber.





Figure 45: Tar bubbles on the thermocouple tube, experiment 3-3a (5 cm cover T).

Upon excavation and examination of the bark remains, it became clear that structure 3-3b had likely also produced tar as the bark remains were covered in a glossy, but burnt, matter. However, no tar residue was present within the structure. We assumed it had burnt to destruction due to the thinner cover and hence higher temperature of the interior chamber. It was observed that the copper tube had in effect created a double-vessel situation, and it is therefore also possible that the tar in structure 3-3b had drained into the sand.<sup>315</sup> Both structures had clearly undergone reduced atmosphere, visible in the blackened sand layer. The chamber walls were solidified, probably due to sintering (Henderson 2000: 132-133). This is interesting, as it can be assumed that once sand has sintered it is close to mimicking the protective environment of a pot.

Experiment 3-4 was designed as a closer investigation of the 3 cm cover thickness that was introduced with 3-3b, as it was not yet confirmed that this experiment had in fact produced any tar. The idea was to observe the temperature and general performance of a thinner cover. Based on the experience of tar spill in 3-3a, a copper run-off tube was introduced to provide a means of catchment and hence confirm tar production. This was deemed necessary as the experiments were not focused on catchment methods, and

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315. As the conditions for pyrolysis are the same in single- and double-vessel structures, the fact that this was a double-vessel firing is not relevant to the research question, which were only concerned with achieving those conditions in a sand heap. The ad hoc double-vessel structure is therefore of no consequence to the results of the experiment.

the tar seemed to be running off into the sand or burning to destruction due to high temperatures (321 °C max). Although this would again create a double vessel situation, the experimental focus was strictly maintained; to create an airtight pyrolysis chamber by the simplest means possible. Catchment methods were therefore not introduced as an experimental parameter. To prevent a run-off into the thermocouple tube, this was placed at a slight incline from the bottom of the pyrolysis chamber, whereas the catchment tube was declining away from the chamber. As a receptacle, a sheet of lime bark was placed at the outlet, and covered with gravel and sand to seal off the opening.



Figure 46: Experiment 3-4 after firing.

Experiment 3-4 (Figure 46) was the slowest firing of the standing structures. The experiment was actively fuelled for 4 hours and 55 mins, and there were several plateaus during which the bonfire was manually fanned. This was ascribed to weather conditions, as there was a high level of humidity combined with very little wind. It was contemplated whether the fresh bed of humid sand was adding to the humidity as it dried and released steam within the structure, but this could not be observed and was only noted. After 4 hours and 10 mins,



the fuel was changed to thinner sticks of seasoned softwood, which resulted in a rapid increase in degrees. The experiment was terminated at 317 °C as, even with the use of two fans, the temperature had plateaued, and it was deduced that the measurement must again be noted as a minimum chamber temperature due to the position of the thermocouple at the base of the chamber.

Upon excavation, it was confirmed that tar had been collecting in the catchment tube as remnants of the oily substance were clear, but had soaked through the insufficient bark receptacle and into the soil. The structure otherwise showed great similarity to experiment 3-3a and 3-3b with blackened, reduced sand and sintered walls encasing the bark roll (Figure 47).



Figure 47: Central pyrolysis chamber after firing and excavation, Exp 3-4. Bark char has been removed to produce a roll-shaped chamber with sintered walls. The reducing atmosphere has also produced a near black sediment to the centre of the structure, which lessens with increasing distance. Scale in mm.

Experiment 3-5 was designed as a double firing after the template of 3-3, but as opposed to 3-3, the thermocouple was inserted into the 3 cm cover structure (3-5b) to provide an interpretive gauge for the 3 cm cover efficiency. This time, proper receptacles were introduced in the form of glass jars, with copper outlet tubes declining from the chamber. Because the glass had to be protected from thermal stress, they were covered with a substantial layer of sand, and a bank

was set up between the receptacles and the bonfire (Figure 48). This meant that the fire would not be as effectively surrounded by flames as in the previous experiments, but did not appear to influence the temperatures upon firing.<sup>316</sup> The thermocouple tube was again placed at a slight incline from the bottom of the pyrolysis chamber as had proved successful in the previous experiment.



Figure 48: Experiment 3-5. From top: 3-5a (5 cm cover T), 3-5b (3 cm cover T).

The temperature increased rapidly in experiment 3-5, most likely due to less humid conditions and an average wind speed of 0,5 m/s (see Table 6). The interior chamber reached 321 °C after only 2 hours and 10 mins, at which the feeding of the fire stopped and die-down was initiated. Upon excavation, tar had collected in both jars (Figure 49) and so as a concluding experiment, experiment 3-5 was deemed successful. Again, the black and sintered chambers showed strong similarity with all other experiments apart from experiment 3-2. Measurements continued until 5 hours had passed, to observe the declining temperature curves in a successful experiment (Figure 50).

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316. This highly unactualistic receptacle construction was deliberately not designed by the simplest means possible. At this point, we were already confident that we had produced tar, and collected tar for further analysis.



Figure 49: Tar from structure 3-5a (5 cm cover thickness). The star-shaped wall pattern is the outlet point of the collection tube and resulted from the jar's horizontal position in the ground. This photo was taken two weeks after the experiments, and some of the viscous pitch still clings to the vessel wall.

As can be seen in Figure 50, all confirmed, tar-yielding experiments (EXPs 3-3a to 3-5b) reached a minimum temperature in the vicinity of 320 °C. The bark roll chamber in all of these experiments had an internal height of approximately 5 cm, but the temperature difference between chamber ceiling and floor is unknown, as the thermocouple was consistently put at the bottom of the chamber, and therefore probably measures minimum temperatures only. All mounded experiments with thermocouple<sup>317</sup> could technically be classed as double-vessel type, due to catchment tubes and external receptacles (in experiment 3-3a the thermocouple tube itself), and the tar produced was unpolluted by char and sediment. However, in experiment 3-5 it proved impossible to lift the jars away from the sand without trapping sediment within, which resulted in sand contamination.

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317. Experiments 3-3a, 3-4, 3-5a and 3-5b.

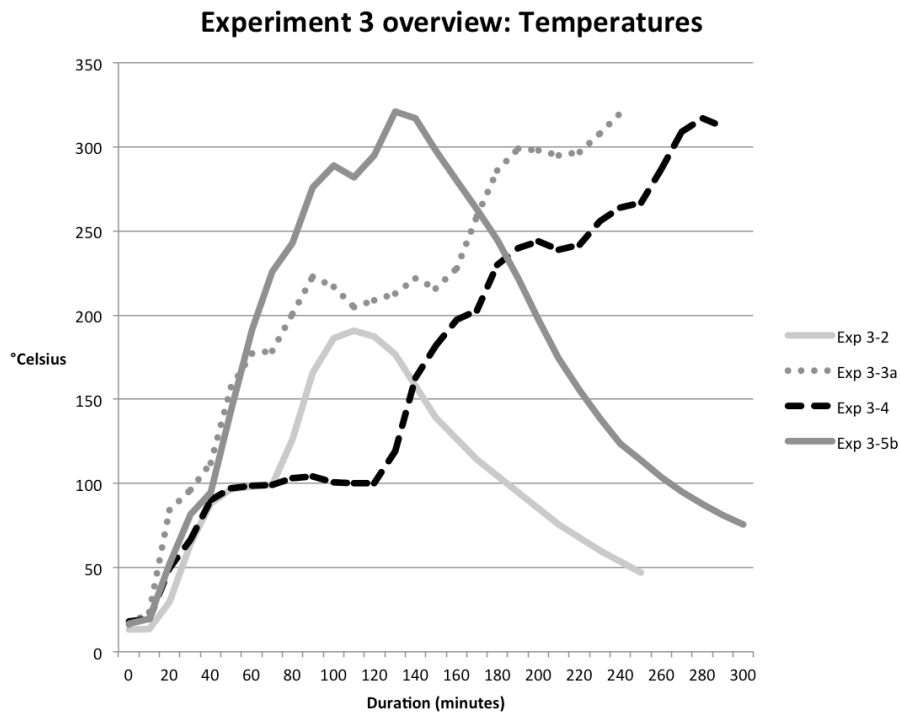


Figure 50: Temperature measurements for EXP 3-2 to 3-5, 10 minute intervals.

The temperature in the friable sand mounds easily climbed high enough to produce a clean tar in a double-vessel situation. Once the design was determined, this procedure (without catchment tubes) is a simple process that did not take much practice before mastery. To speculate further as to the level of Palaeolithic technological skill is beyond the scope of these experiments, which were foremost designed to explore the feasibility of pyrolysis of birch bark tar in the simplest way imaginable;<sup>318</sup> and to pile damp sand around a bark roll is indeed very simple. The feasibility of such an operation was confirmed, and the experiments were deemed successful. For a summary of recorded data, see Table 6. For additional information about the experiments, see Appendix F.

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318. We realise that this is our imagination as researchers, and that for others, other procedures may seem more simplistic. We urge more discourse on this topic.

Table 6: Summary of experiments with raised mounds.

	EXP 3-1	EXP 3-2	EXP 3-3a	EXP 3-3b	EXP 3-4	EXP 3-5a	EXP 3-5b
<b>Date</b>	15/10/13	16/10/13	17/10/13	17/10/13	18/10/13	20/10/13	20/10/13
<b>Start (fire)</b>	17:30	12:00	11:15	11:15	12:05	11:45	11:45
<b>End</b>	20:30	16:10	15:15	15:15	17:15	16:45	16:45
<b>Air temp °C</b>	15,5 – 13,4	14 – 17,6	17,5 – 20,9	17,5 – 20,9	13,7 – 16,4	14,7 – 18,7	14,7 – 18,7
<b>Humidity %</b>	80-85	88	69	69	87	72	72
<b>Weather</b>	Cloudy	Rainy	Sunny intervals	Sunny intervals	Mist/Rainy	Light showers/sunny intervals	Light showers/sunny intervals
<b>Average wind (m/s)</b>	0	0,6	0,5	0,5	0,1	0,5	0,5
<b>Structure type</b>	Raised mound	Stone slab w/ raised mound	Raised mound w/ TC tube	Raised mound	Raised mound w/ TC and catchment tubes	Raised mound w/ catchment tube	Raised mound w/ TC and catchment tubes
<b>Structure dim</b>	H: 12 cm ∅: 35 cm (base) ∅: 17 cm (top)	30 x 30 x 4 cm	H: 10 cm L: 20 cm W: 15 cm	H: 8 cm L: 15 cm W: 11 cm	H: 8 cm L: 15 cm W: 11	H: 10 cm L: 18 cm W: 15 cm	H: 8 cm L: 13 cm W: 11 cm
<b>Start temp °C</b>	--	13,2	16,2	--	18	--	16,7
<b>Max temp °C</b>	--	177	320	--	317	--	321
<b>Fire die-down</b>	20:00	13:20	15:15	15:15	17:00	13:55	13:55
<b>Firewood type</b>	Softwood, logs	Softwood, logs	Softwood, logs	Softwood, logs	Softwood, logs/sticks	Softwood, sticks	Softwood, sticks
<b>Firewood vol (l)</b>	21	21	42	42	42	28	28
<b>Bark vol (mm<sup>3</sup>)</b>	282,6	c. 200	157	109,9	141,3	125,6	
<b>Cover thickness (cm)</b>	5	5	5	3	3	5	3
<b>Tar odour occurrence</b>	17:50	13:10	12:40	12:40	--	14:05	14:05

Result	Burnt tar in cylinder	Indet.	Tar	Burnt tar	Tar	Tar	Tar
Notes	Exploratory. Structure pre-experiment  Occasional use of bellows 17:40-20:00	Exploratory; alternative structure. Stone fractured at 13:05-10 due to thermal shock; caused exposure to bark sheets.	Experiment with cover T.  Two structures simultaneously in one fire.  Experiment terminated when tar occurred in TC tube.	Experiment with cover T.  Two structures simultaneously in one fire.  Experiment terminated when tar occurred in TC tube of 3-3a.	Experiment to measure temperature at 3 cm cover T.	Double firing with catchment jars.  Attempt to confirm tar in structures with both 5 cm and 3 cm cover T fired simultaneously.	Double firing with catchment jars.  Attempt to confirm tar in structures with both 5 cm and 3 cm cover T fired simultaneously.
	<b>EXP 3-1</b>	<b>EXP 3-2</b>	<b>EXP 3-3a</b>	<b>EXP 3-3b</b>	<b>EXP 3-4</b>	<b>EXP 3-5a</b>	<b>EXP 3-5b</b>

### 8.2.5. Conclusion: we learned a lot about birch bark tar, but...

The experiment design of both experimental series were set up with an awareness that the procedure would not in fact represent any known technology, and would therefore consist of qualified attempts to understand the technology more than reproduce a known, archaeological situation. This is understood by several of the authors in this discourse (notably Meijer and Pomstra 2011; Osipowicz 2005), and at current the list of potential, Palaeolithic tar-yielding procedures is growing. Nevertheless, there are no guarantees that either of the proposed methods are even close, and we may also grossly under- or overestimate the cognitive capability of different Middle Palaeolithic people – which in this case have been grouped under one although separated by up to 200 000 years. We are not aiming to state that mounded sand heaps in a fire was in fact the way it was done. Rather, we wanted to explore whether the structure could be constructed with virtually nothing, so as to leave no archaeological trace aside from general bonfire remains, or leave only a spill of hydrocarbons such as at Abric Romani. Through that argument, we of course still launch this method as one that *might* have taken place.



The experiments were defined within the object-manufacture category,<sup>319</sup> and the evidence level was associated for the tar-yielding structure (E).<sup>320</sup> This level remains associated; we know it did happen, but we have no knowledge of how (yet), and can therefore only associate the actual structure from our own notions of tar production structures. The evidence level for the technological process (T); how the process of the entire production occurred, follows hand in hand: again the only indication is the tar finds. However, the existence of tar does necessitate two factors: airtight conditions and controlled temperature. For these, no analogy is necessary; the reducing atmosphere must be present, and the temperature span must be in the necessary range for the pyrolysis.<sup>321</sup> Series 2 was structured around these two criteria, and the pit experiments were used as a reference for what constitutes insufficient/poor heat penetration. Nevertheless, the high level of uncertainty throughout the experimental process must be taken into consideration, and the evidence for the technological process was still associated: we started from an idea based on what a pot can do; which appears to frequently be the beginning of such experiments (Osipowicz 2005; Palmer 2007). As the actual, Palaeolithic makers have had no notions of ceramic pottery, this is a fictional way of reasoning, and we lose a lot of the procedural understanding this way. Nonetheless, this analogical thinking from modern to past is something an archaeologist can never be unaffected by.

Currently no physical contexts (C) for tar production exist, neither from Campitello quarry, nor Inden-Aldorf, nor Königsau that can shed light on either technological procedure. Even so, the level of evidence in relation to the contextual situation of firings can also be associated, in this case to Abric Romani. The site is significant for the potential position of the tar structure *in* a bonfire – potentially one that was also used as a general heat source, and that was situated in relation to a shelter. It is also significant for the interpretation of size of the structures, which must necessarily be smaller than the bonfire to obtain a well-dispersed temperature throughout the pyrolysis chamber. Additionally, as the Königsau pieces presumably fall within the time span for

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319. Table 3

320. For E, T and C; see p. 157.

321. Roughly between 250 and 400 °C.

Abric Romani, the standing structures were planned and relatable to the limited archaeological material available. Yet, we do not know that Abric Romani is in fact relevant to tar production, and so the evidence level here, is also still associated. Although our experiments were related to known Neanderthal combustion structure sizes (Roebroeks and Villa 2011), this is still only an association from the fact that we know the tar must have been produced in the vicinity of fire to achieve the necessary temperatures, and does not change the evidence level for tar production. It should also be admitted that by demonstrating that birch bark pyrolysis may well occur in small heaps of sand, we have not excluded opportunistic gathering of a naturally produced resource, as can also be argued for the results of Meijer and Pomstra (2011).

Should the structure at Abric Romani, or similar structures, eventually turn out to be connected to birch bark tar production, the evidence level may change to indirect because of series 2 above. If so, the spill of hydrocarbons would indirectly be the evidence of structures that leave no other trace; presumably standing structures that would be indistinguishable against the bonfire remains, as pits; certainly with charcoal, hydrocarbons and/or burnt sediments, may well be traceable for a very long time: shallow, burnt depressions interpreted as hearths are for instance found at Beeches Pit, UK, dated  $414\ 000 \pm 30\ 000$  BP (Preece et al. 2006: 491). As for now, the experiments in this chapter have still not changed the evidence for how birch bark tar was produced in the various stages of the Middle Palaeolithic.

So, given the uncertain evidence situation even after experimentation; was series 2 a viable experiment per discourse criteria?<sup>322</sup> The discussion above shows how we deemed the archaeological primary reference. As no other evidence seems to exist than the tar finds themselves, Neanderthal hearth sizes and potentially Abric Romani hearths, the experiment viability may seem questionable. Aside from the archaeological reference, a discourse criterion for a sound experiment (section 2.3.) was that variables should be singled out and measured. Since the tar experiments were carried out as field experiments, a complete control over variables could not be achieved, or even attempted. This

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322. See section 2.3.

was indeed why the field mode was chosen; to explore the different practical opportunities that arise given the limitations of the selected technological materials sand, fire and bark. According to several authors, this is one of the key strengths of the field mode (Jolie and McBrinn 2010; Rasmussen 2001; Rasmussen 2007b; Whittaker 2010). The chemical process of birch bark tar and the conditions for pyrolysis are well known (e.g. Pollard and Heron 2008: 246ff), and so the isolated variables carry less importance than if this was not the case, or if the experiment was aiming to test other parameters. What was highlighted in the current experiments was the relation between heat penetration efficiency of sand and its potential for creating a reducing atmosphere in an unprocessed state. Various factors relating to heat were measured, but could not be isolated for one-by-one testing under true control. In the end, the research question was simply "can it be done?" and the recording of temperature, tar odour, windspeed, time and cover thickness was mainly to ascertain that approximately similar conditions were upheld during each experiment, and that we would be within the correct temperature span for pyrolysis. The general standard ensured by close monitoring makes it easier to compare the experiments with each other. After this series, where the proposed structure did in fact consistently produce tar, the team is now likely to be sufficiently experienced to produce tar without gauging these parameters, and to begin experimentation with other parameters, larger structure sizes, and catchment solutions.

As stated in the beginning of this chapter, we were aware that we would not be any closer to a definite Palaeolithic methodology for tar production. Even so, the experiments produced several results that are highly relevant for the view of Palaeolithic birch bark tar production:

- Tar production is not difficult once the skill set is learnt. A crucial competence is to assess how long it takes for pyrolysis to produce tar so that the fire can be terminated to prevent the tar from burning. This can be gauged by using the occurrence of tar odour (volatile evaporation).<sup>323</sup> Once the timing skill is learnt, and a general know-how of extraction/processing time is obtained, the fire can be left to its own devices apart from the occasional feed.

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323. As a personal example, the tar odour has become the gauge for the present author when producing tar without measurement equipment.

- It is not necessary to formally build or create a structure to produce tar. Rather, a random heaping of sand only held together by moisture is likely to produce tar just as well as any other method. However, this type of structure is conceivable as an element of the repeated use of fireplaces, as suggested by (Courty et al. 2012).
- The above statement cannot exclude opportunistic gathering of naturally occurring tar as a result of spontaneous fires. However, the subsequent processing is more likely to be anthropogenic, as naturally occurring tar must be considered to be contaminated with sediments in most cases.

The results of this experiment has highlighted at least two alternatives for potential chaînes opératoires; intentional, human creation of a structure for pyrolysis; or natural production of birch tar which is later gathered and processed by human hands – both for (at a minimum) the purpose of adhesion. Each chaîne opératoire dictates a potential framework for intentions, skill, skill transfer and use. Once the actual production is understood, our results may bear influence on intangible features of being human in the Middle Palaeolithic. For now, our experiments are a valuable contribution to the discourse on the concrete specifications for tar production, but less so to the notions of everyday life and technological practice in the Neanderthal communities that made use of birch bark tar.

## 9. Discussion

The aim of this thesis has been to explore whether there are conceptual and practical obstacles to understanding intangible technological aspects through an experimental research methodology. To promote such analysis, concepts relating to experimental archaeology, the paradigms it rests on and the approach to technology have been investigated. Based on this theoretical examination, three practical experiments into different technologies, periods and materials were set up to provide a practical view of experimental practice. This has subsequently led to the current point of the analysis: a broad view of a multi-stranded experimental archaeology which must be bound together and evaluated.

Summarised, Part I of the thesis started with a presentation of a study sample of 100 selected experiments from 1996 and onwards. This sample was used as a starting point for the analysis of experimental research archaeology, and to gain a broad view of the technological, theoretical, analytical and regional practices that are part of the discourse. Through exploring definitions, codes for conduct and general guidelines regarding experimental modes or circumstances, experimental archaeology was gradually defragmented to form a fuller picture of what the premises for the practice are. Together with a research stay at the Viking Ship Museum in Roskilde, Denmark, this deconstructive view made it possible to gain a broad impression of how experimental archaeology is executed internationally. Through this view, a description of a typical experiment was gained, with the intention to both deconstruct the concepts behind experimental practice, and to reconstruct the method so that it could be explored further through case experiments.

After an analysis of experimental archaeology, the paradigmatic place of experiments in the archaeological discourse was explored and repositioned. The repositioning was done by taking what is very often considered a scientific methodology, but is as often a biased, human practice, out of the positivist-postmodern disharmony, and transplant it into a third paradigm rarely considered in archaeology; philosophical pragmatism. By moving it from positivist/postmodernism to pragmatism, experimental archaeology was allowed

to contain all its practical and theoretical diversity, whether or not an experiment was explicitly discussed in a theoretical regard. Philosophical pragmatism allows a researcher to stay concerned with the consequence of a research procedure, instead of retreating to paradigmatic quarrels about dichotomies. The latter occurs more in the general archaeological discourse than in the experimental, but the important factor was that experimental archaeology, which has recently changed from a solely scientific to a scientific/interpretive multitude, would be preserved with its diversity without normative judgement for the evaluation to come. In the pragmatic sense, it does not matter what is right or wrong, as much as what the outcome is and how the outcome is discussed. Our sub-disciplinary discourse is diverse, but allows for this plurality both by the publication of non-scientific experiments in *Journal of Archaeological Science* (e.g. Liedgren and Östlund 2011), and the presentation of science in the public terrain (e.g. Comis 2010). The outcome is mixed, and it was decided to look at its backdrop in a way that would not force it into a fixed pattern.

The mixed outcome of the experimental methodology, from zooarchaeological taphonomy studies to ethnoarchaeological fieldwork, from bronze swords to wool fibres, was also why the choice fell on applying a typified methodology for the case experiments rather than maintaining a standardisation, as experimental archaeology is too diversified for a standard to be a rightful representation. To typify instead of standardise would be to omit an isolated subscription to the most cited theoreticians alone (Coles 1967; Coles 1973; Kelterborn 2005; Mathieu 2002b; Reynolds 1999; Saraydar and Shimada 1973), of which only one published an original paper in the 2000's.<sup>324</sup> The intention behind the method evaluation was to take a view of experimental archaeology that would be representative for how it is achieved *today*, largely by looking at it in practice. By looking at the present practice, a broader range of viewpoints could be sought rather than only those cited most often. Part of this was for instance obtained by investigating a sample of 100 experiments from the full breadth of the experimental discourse.<sup>325</sup> Philosophical pragmatism allowed for the plurality that is experimental archaeology today, whereas the other two paradigms would be mutually exclusive. To apply either of these would cause a

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324. Kelterborn's 2005-article is a collation of three articles from the 1980's.

325. Appendix A.

strained juxtaposition between the simultaneously scientific and interpretive contributions that the discourse presently contains, and therefore not particularly suitable for an evaluation of the method in its academic entirety.

To conceptualise what intangible aspects of a technology can be, practice theory was presented as a theory that could describe the scope of a technological practice. Although there is only scant usage for practice theory in general experimental discourse, largely atheoretical beyond the experimental methodology, there is solid application of the chaîne opératoire for the conceptualisation of technology. Practice theory seems to be the theory most often connected to the chaîne opératoire approach in archaeology in general, and when its concepts were considered, it was deemed to be a good representative for how experimentalists perceive technology, or maybe rather the world of crafts. Through combining concepts from practice theory and chaîne opératoire, the methodology could access intangible aspects of technological practice through structured fields, that are guided by doxa and ruled by individual agency.

The construction of the typified methodology was the last part of the deconstructive process. The construction was approached by using pieces of the deconstructed experimental praxis and investigating them before reconstituting them into a procedure. The methodology included a close consideration of the archaeological primary reference, one of the most – if not *the* most – important part of an experiment. The archaeological reference usually functions as the indicator of interpretational success. As experimental archaeology is often claimed to relate to deductive experimental reasoning in terms of procedural knowledge generation, it was decided to include the scope of logical inference in the evaluation of such success. However, after an evaluation of the reality of deductive and inductive reasoning in experimental archaeology, it was rather determined to apply the pragmatic inference form abduction.

By building on the principles of abduction instead of the logically stringent deduction or induction, increased likelihood rather than validation or falsification was seen as the desired outcome of experimental inference. To concretise



whether or not an experiment had increased the likelihood of its interpretive result, a categorisation of experiments by isolation of their proximity to the archaeological evidence was created. The plan was to apply this proximity as a gauge for experimental success: if the interpretation was backed by an increased proximity to the archaeological evidence, the experiment could be considered successful in providing a more likely interpretation. The purpose of the categorisation scheme was to present an experiment result in a format that could be understood by several experimenters. The guidelines brought forward in the discussion of experimental procedure in Chapter 2 were combined with the categorisation scheme for evidence level. This integration was decisive for the case experiment protocol.

Part II contained the reconstructed, typified experiments, which were intended to provide the practical baseline for the assessment of experimental potential for accessing intangible information about past technologies. Experiment 1 was an exploration of Bucket-shaped pottery, predominantly recovered from burials from the Norwegian Migration Period (400-600 CE). This pottery has an abnormal proportion of temper (80-90 %) which consists of either soapstone or asbestos. At the same time, it often displays a low firing temperature – sometimes below 500 °C (Hulthén 1986). It was decided to take a view of soapstone in isolation, and its often proposed relation to stabilising abilities in the production process, including protecting the pot from thermal stress. The focus on such regular temper abilities has been fairly consistent in the discourse, but such capabilities in the rest of Norwegian history presumably only require around 15-25 % temper addition, also in other types of pottery from this period. The mystery of the high proportion of temper therefore seemed to go beyond thermal properties. Nevertheless, it was considered a good place to begin experimentation, and the connection between temper proportion and thermal properties was therefore explored through three experiments each with pottery containing 25 %, 50 %, and 75 % soapstone. The experiments consisted of one field execution of a bonfire firing, one lab-structured experiment that complemented the information from the field experiment through firing test tiles in controlled conditions,<sup>326</sup> and one controlled lab

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326. This was however not a true lab experiment, as the test tiles were hand-made.



experiment where kiln fired test tiles were exposed to increasing amounts force through a three-point bending test. The latter experiment aimed to highlight the potential relation between temper proportion and temperature in relation to durability and hence stabilisation properties.

The bonfire experiment showed that all test pots survived the firing equally well, and it was not possible to highlight any discernible difference in thermal property between proportions. However, all the pots changed from a matted clay-colour to a golden metallic sheen, more so with the increasing amount of temper. The kiln firings concluded that this colour change was a gradual process which was initiated after 500 °C but before 750 °C. The three-point bending of test tiles of each temper proportion, kiln fired to 750 °C and 1000 °C, showed clustering of data according to firing temperatures, regardless of temper proportion. This experiment indicated that the fire temperature of the clay was the determining factor in the resistance to applied force. This can therefore be seen as an indication that durability is determined by the firing temperature of the *clay*, rather than related to the soapstone content proportion.

Altogether, the results indicate that soapstone temper in large amounts neither increases or decreases thermal resistance nor durability, and is therefore not likely to have been added for such functional purposes. This was within the aim and scope of the research question. However, the change in appearance from matted silvery to shining golden was not considered within the research question. This came as a pleasant surprise which not only excluded prior interpretations, but made the temper much more likely as an aesthetic additive. Recent analyses have revealed that there are minute traces of gold particles in certain specimens of this pottery, likely to have functioned as a representation of its presence rather than have held any functional purpose. It is therefore conceivable that the golden colour was intentional, and related to the status of gold, functioning either as a sought-after appearance, or as a proxy for the precious metal. The findings related to previously unknown properties of the archaeological pots directly. Furthermore, the results were indicative of intangible aesthetics relatable to death, fire and transformation. The evidence levels had therefore increased, and the experiment was deemed a success according to the categorisation scheme.

The second experiment concerned the functionality of a Mesolithic/Neolithic ground stone tool category that is occasionally found on hunter/gatherer settlement sites around coastal Norway. The tool presents as palm-sized beach pebbles with one or two pecked facets, but its function is unknown, and it is frequently misclassified as a grinding stone, hammer stone, or anvil stone. It is also occasionally interpreted as an unspecified crushing stone. The experiment aimed to determine its function to discuss its context on two separate sites in relation to site organisation, task management and what such results could say about the society that used the stones. The experiment was set up as a classical use-wear study. Tasks included in the production of the reference collection were based on the most normal interpretations, and included cracking and grinding hazelnuts, separation of sinew through repeated pounding, and a production of unused surfaces. A use-wear analysis was performed based on the investigation of 11 archaeological specimens from Eastern Norway and general tribochemical characteristics (Adams 2002; Adams 2010; Adams 2014). The stones resulted in a possible identification as sinew pounding stones, but it was realised that the list of reference tasks was too short and should have contained a wider spread of wear patterns – for instance hide processing, which may bear similar characteristics (Adams 1988). As the functionality of the stones could not be determined, it could neither be determined whether intangible aspects of the respective technology could be arrived at through this methodology. The evidence levels were therefore not augmented after the experiment.

The third experiment concerned the much pondered Neanderthal birch bark tar production. The only recovered evidence of this production consists of a limited number of tar samples, and the aim of the experiment became to use this information to present an alternative production method. It was expected that a view towards the production technology could inform on the surrounding procedures, whether it entailed systematic production, or could be easily procured through a simple procedure. The experiment was part of a series that the experimenters had started a few years prior, and where experiences from unsuccessful pit firings had led to a consideration of simplistic standing structures that would leave no archaeological trace. The team decided to work

towards achieving necessary pyrolysis of the birch bark through the simplest possible means, which was for the time being considered to be a small heap of sand. All four experiments with this structure type were successful in producing tar, although a receptacle was not included. Conclusively, the experiments showed one possible way of procuring tar. However, as there is no archaeological evidence relating to such process apart from tar remains themselves, and as the finds in question span 150 000 years, it has to be maintained that the results gave no information about a specific society, and rather only informed on a fictive situation where two modern researchers decided to see if they could produce tar in a – for them – exceedingly simplistic structure. The evidence levels were therefore not increased by the experiment.

In total; two of three experiments resulted in negative findings relating to the intangible. Is this a representative result for the experimental discourse? The case experiments were performed by discourse guidelines, collated to address several, unrelated technologies and raw materials,<sup>327</sup> different periods, different societal structures and differing scope. Nevertheless, to evaluate an entire method based on the personal experiments of one author is a meagre ground for conclusion, and so the experimental discourse has been involved as much as possible, and the final method evaluation refers as much to the analysis of the broad discourse as to the case experiments. In this way, as so many experimenters influenced the assessment, the project is thought to have assessed an adequate data sample for the analysis. The combined view of case experiments and discourse shows that some experiments are clearly very useful to gain greater insight in what was part of the intangible, technological field of practice, but it is unclear whether experimental success could have been increased if the faceted stones experiment was successful. It may of course be discussed if a reference collection is ever completed. Nevertheless, what seemed increasingly evident was that not all experiments can achieve a view of intangible aspects of technology, even when this is the overall aim of the experiment. Whether this is due to the technological limitations, preservation bias, experimenter bias, format of research question, raw materials or ideal, is not clear. Whether the experiment execution was in fact representative of the

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327. Although pyrotechnology may be a common denominator for the experiments with Bucket-shaped pots and birch bark tar procurement.

**entire methodology is in the last instance open to be critiqued by others.**

Not only must the entirety of the discussion now be amalgamated, but an evaluation of the adequacy of the chosen methodology must also take place. A key assessment becomes the viability of the perspectives, guidelines, and theories as a basis for an analysis of the practice of experimental archaeology. Before such questions are assessed, results of the three case experiments will be referred and compared to provide an entry point for the discussion to come.

### **9.1. Results of experiments and intangible worlds**

The results achieved through the experiments in Chapter 6 – Chapter 8 span wide, both in periods, technologies, and results. In the experiments with Bucket-shaped pots it became clear that the high proportion of temper did not necessarily have a functional motive. The only discernible factor that came forward was the fact that the pots changed appearance when fired over a certain temperature threshold. As cooking and food processing pots often have maximum half the temper material of the Bucket-shaped pots, throughout the world and regardless of period, there is no particular reason to maintain that the temper proportion of soapstone to clay was related to heated food-processing. The earlier interpretation that it was used to stabilise the paste during manufacture and firing was also unfounded. Instead, it appears that aesthetic perception, or the desire for the pots to change to metallic appearance for other reasons, for instance metal representation, were the primary motivation of the use of 80-90 % soapstone additions in the vessels (Kleppe and Rueslatten 1993; Kleppe and Simonsen 1983: 18). Why should be further analysed, but it does fit nicely with the current research on Bucket-shaped pots, (gold)smiths, metals and funerary environments. However, what should be particularly noted about this experiment is that the research question was formed so that any positive result could possibly be conceptualised into a contextual, situated technological practice. The research question itself was not structured to accommodate the aesthetic appearance of the pots directly. Rather it was constructed to exclude the 'normal' reasons for adding temper to pottery, so that it would be highlighted that it was potentially *not* a purely utilitarian choice (this

is apparently quite typical, see Pélegrin 2006: 38). By exploring the lack of utilitarianism, negative results were expected to give good grounds to discuss potential other reasons for the high temper proportion. Instead, the experiment gave positive, tangible results to indicate an intangible idea. The scope of the intangible indication was therefore unexpected (for a discussion of similar experiences, see Narmo 2011).

A less investigated situation related to the context of Late Mesolithic/Neolithic faceted beach pebbles from marine/coastal environments. These experiments were built on the general Mesolithic discourse. However, there was no prior research of the artefacts themselves. To find out more about the stones, first and foremost their function, the experiments were informed by classic use-wear studies where an experimental reference collection is traditionally compared with archaeological specimens. In the same instance, the experiments were conducted with a view towards functionality, and the processing of sinew and hazelnuts in various formats informed on final interpretation of results.

A decisive function could not be determined until further tests have been undertaken, particularly concerning hide-processing, and the references may benefit from an even wider span of technologies. At this point, the stones stay connected to the tangible realm of actual use-motion (vertical) and surface reaction (rounded crystals, surface debris and pitting). Even so, the experiments did lessen the likelihood of prior interpretations of function, which were connected to grinding, bipolar knapping technique, use as hammer stone, or as nut-cracking implement. Significantly, the research question was constructed to achieve such exclusion. Although there is a substantial amount of research on contemporary technology even from the sites in question, together with excavation reports and ecology from both the Late Mesolithic and Early to Middle Neolithic, there was no specific research on the stones themselves which could inform on their context and use. This proved to cause difficulty when the step from tangible to the intangible was considered, and with the current information, no such interpretation could as of yet be made. To put an experiment as the first systematic research is a good way to gain insight into tangible aspects such as function, but possibly less ideal to attempt to gain an understanding of *notions* of use. To reach this point is likely to take more



research on the stones in connection with site and techno-complex, both experimental and non-experimental.

The third case experiment entered a realm where barely anything is known about the surrounds of the people in question. European Middle Palaeolithic Neanderthals are biologically well researched, but in relation to their use of technology, research is sparse, even on a European level. Additionally, research spans hundreds of millennia, and so a given community can hardly even be identified. The experiment in this thesis attempted to add a perspective of possibility, rather than a regular discussion of function or procedure. The research question was designed to evaluate a simple practice pattern rather than provide an interpretation of an archaeological situation.

The experiment showed that birch bark tar; one of the few substances we know that people had and used in the Middle Palaeolithic, can be made in a simple mound of sand. Previous experiments by the same team have shown that it can be complicated to produce tar in a variety of pit structures, neither of which were successful attempts (Groom et al. 2015). Combined, the experiments showed that an advanced chemical process can be very simple if one knows a good procedure. Nevertheless, as there is very little, if any, trace of the actual process, the sand mound is only one of many possible processing structures. Also, since the **case** societies that demonstrably made use of the tar are separated by up to 150 000 years, it is quite likely that they used different methods for tar procurement. However, the structures for this purpose can be exceptionally simple – in their most basic form, one could simply heap some sand together over a little bark. The experiments may therefore have contributed to the discussion of the minimum cognition requirement to be able to achieve tar on an intentional, regular basis. Nevertheless, although the experiments can be said to have contributed **significantly to the discourse**, they have not led from actual tangible evidence to intangible surrounds of the Neanderthals in question, as the process remains on a hypothetical level still. Nor was it our intention to conclude with any aspects of cognition.

To sum up, one out of three experiments was successful in providing non-tangible insights, while still staying close to the material evidence as illustrated

by the experiment categorisation scheme set up for the purpose of the evaluation (Table 3). The two remaining experiments remained on the same evidence levels. The function of faceted stones is likely to be determinable by additional experiments. To find out what the stones were for will be the beginning of a view to the surrounding practice – one that possibly and most likely needs further complementary research to become conceivable.

The way in which Neanderthals over a span of 150 000 years procured their tar is less likely to be determined unless archaeological evidence for procurement circumstances are recovered with similar dates as the tar finds themselves. Without evidence, the experiment cannot proceed into the intangible part of the practice(s) that may have been anything from a day-to-day task to a big event. It could be that pyrolysis was simplistically achieved in a heap of sand, but it could also be that it was undertaken by the construction of an advanced, airtight structure of which we have no indications. It may have been opportunistically harvested, and subsequently and intentionally refined. It could be performed by women, men, children, or all of them, and there is currently no way we will know, neither how often, nor how much they procured. However, we do have an inkling of why, as specimens from each site show use for hafting purposes. The high amount of implements with tar from Inden-Altdorf tells us its inhabitants may have preferred tar to other adhesive substances, but this lack of other substances such as skin glue, could also be due to their decay. More significantly, this is not information that the experimental results did in any way inform on. All in all, it seems to first and foremost be the lack of archaeological evidence that is in the way of accessing the intangible aspects of birch bark tar technology through experimental research.

So; why is it that experimental archaeology is not necessarily successful in achieving information about intangible backdrops of technologies? The Bucket-shaped pots experiment can safely be said to have moved from the material, tangible artefact and its use-life, into the discussion of intangible concepts of taste, aesthetics, and fire transformation of Migration Period people of coastal Norway. This was an experiment related to a much-researched technological tradition, and there were substantial investigations and analyses of the pots to lean on. The two other research situations had no particular backing of research

for their results, and so the experiments alone were demanded to fulfil a view to the intangible. Could experiments alone obtain such broad scope? In the following, the possible conceptual and/or practical obstacles that have become clear through this thesis and possibly played into the experiment results, will be discussed in the order they were proposed in the introductory chapter.

## 9.2. Questions, questions...

Many questions arise from the work undertaken for this project. In the beginning of this thesis, the question was posed whether a focus on technology complicates a study of the intangible. The question came about because although archaeologists frequently study intangible, immaterial aspects of being a human, such as systems for economy, trade alliances, war, professionalisation, rites and religion, taste, fashion, and significance of symbols, experimental archaeologists do so at a much lower rate. Of the 79 % of a study sample of 100 published works<sup>328</sup> that focus on technology, 24,1 % (19 articles) – less than a quarter – give attention to intangible aspects of the respective technologies. This is not likely to be comparable to the general, archaeological discourse, where to the author's knowledge, the elements just mentioned are substantially represented.

The survey may indicate several issues with regards to intangible technologies:

- 1) It is complicated to address intangible questions through a focus on technology.
- 2) It is complicated to address intangible questions through experimental archaeology.
- 3) It is complicated to address intangible questions through experimental archaeology that concerns technology.
- 4) There is a tradition for addressing tangible matters in experimental archaeology.
- 5) Experimental archaeologists have preferential reasons for researching the tangible in technology.

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328. See Appendix A for full list of publications in the sample and results of analysis.



Technology is not only a popular issue to approach with experiments; the archaeological discourse is on the whole quite attached to the idea of technology. We typologise by technological function (scraper, grinding stone), and name periods by the use of new technologies (transverse arrowhead period, Bronze Age), but more significantly; we discuss the connection between technology and cultural belonging (Fuglestad 2007; Gosselain 1992b), assign cognitive levels based on technological accomplishment (Bril et al. 2012; Speth 2015; Wadley 2013) and discuss gender roles expressed through technologies (Dobres 1995; McClure 2007; Soffer et al. 2000; Spector 1993). We raise questions of technology and symbolism (Haaland 2004; Hayden 1995), discuss social strategies expressed through technological action (Armit and Finlayson 1995; Haaland 2004; Hayden 1995; Rankama et al. 2006; Sinclair 1995), and establish social relations through technology. These are only a few examples. We archaeologists essentially rely so heavily on technology for discussing the intangible that our discourse would probably be sparse without it. Question 1) above will therefore not be subject to an extensive discussion. The archaeological discourse shows that it is currently not considered complicated to study intangible aspects through a focus on technology, which should suffice for the purpose of this thesis.

If so, is it rather the experimental methodology that provides a general obstacle for researching intangible elements to society? There are numerous examples of experiments that do indeed focus on something intangible, such as experiments with music and acoustics (Blake and Cross 2008; Cross et al. 2002; Lawson 1999; Marshall 2011), learning and cognition (Bril et al. 2012; Eigeland and Sternke 2011; Eren et al. 2011; Khreisheh 2013), skill (Apel 2008; Knutsson 2006; Morin 2012; Whittaker and Kamp 2006), bodily gestures (Geribàs et al. 2010; Pélegrin 2006; Stefanakis and Vlavoilakis 2014; Štěpán 2004), spatial perception (Clarke and Renwick 2013; Kaltsogianni 2011; Vranich 2002), maritime travel (Edberg 2009; Englert 2012b; Wharram and Boon 2006) and a host of ethnoarchaeological experiments with social foci (to mention just a few: Barndon 2011; Fredriksen 2009; Gosselain and Livingstone-Smith 1995; Kory Cooper et al. 2015). The experimental discourse is rife with examples of research that directly target the intangible premises and expressions of a

society. It is therefore unlikely that it is the experimental methodology alone that complicates the research or non-tangible elements of the past. Again, the general archaeological discourse provides an answer and the question will not be explored further.

Although some of the examples above concern technology, the lack of technological experiments on intangible topics are demonstrably significant outside of certain themes. Especially skill and learning aspects of technology have gained a large body of experimental research in the last few years.<sup>329</sup> Apart from that, it seems to be that it is within maritime archaeology and ethnoarchaeology we see the majorities of non-tangibly focussed technological experiments. The general status of discussions of the intangible in the technological, experimental discourse is meagre, as already demonstrated by the analysis of the study sample. This is the reason behind the main questions of the thesis: whether there are conceptual or practical barriers to understanding intangible aspects of technology through the experimental research methodology.

### **9.3. Research question 1: Conceptual limitations to accessing intangible technologies**

#### **9.3.1. The problem of theory**

Chapter 4 demonstrated that to approach the intangible of a technological practice can provide meaningful insights into how a society lives, expresses themselves, and reasons. In other words, the circumstances of technological practice can illuminate the fields of societies past and their worldviews – their *doxa*. The extent of this illumination is unquantifiable, but it is clear that to understand previous societies *beyond* their things is one of the main undertakings of archaeologists. Theories that connect technological or other practices to mechanisms in society abound in the social sciences, but one of

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329. These experiments are largely focussed on lithics, and the potential connection to the gestural chaîne opératoire; predominant in lithic studies, cannot be overlooked.

the more established theory sets across the social sciences is practice theory.

Practice theory is currently well adjusted to archaeology, amongst others through the chaîne opératoire approach. With its established history and suitability for bodily expressive practices, practice theory can provide eligible concepts of expressive practice for research in experimental archaeology. In this endeavour it is not alone, as there are several other theory complexes about the intangible applied in archaeology; for instance Actor-Network Theory, feminist and queer theories, phenomenology and semiotics/semiotics. However, practice theory is explicitly about practice; arguably the priority of experimental research. In this regard, it yields concepts that can be adapted to experimental study through the chaîne opératoire, as was demonstrated through the experiments in Chapter 6 to Chapter 8.

The application of practice theory to experimental archaeology provides a universe in which to discuss practice as meaning. That makers added a high proportion of soapstone in a Bucket-shaped pot so that it would change appearance makes sense when seen against the backdrop of their contemporary society, where the pots were predominantly used as symbolic burial furnishings. It also makes sense when it is considered together with the other elements that fundamentally changed the chaîne opératoire of production. The pots were moulded instead of coiled as was the earlier standard. They were of a shape and patterning that was not insinuated in the archaeological record until it appeared. The nature of the constituents was also different, as soapstone was not a typical temper before this point, and asbestos had been used in earlier times, but was not in use immediately before the Migration Period (Jørgensen 1988). Additionally, the pots were fired on unexpectedly low temperatures, although this was the case for some of the earlier pottery as well (Hulthén 1986). In sum; most of the chaîne opératoire changed with the initiation of these pots, while simultaneously existing in tandem with classical pottery making. The change in chaîne opératoire must necessarily bear a meaning, and the experiments have brought us closer to understand what that meaning may have been. Also, the meaningful practice becomes increasingly expressive when compared to other contemporary practices. The experiments also contributed to a view of the Bucket-shaped pottery practice in juxtaposition

with the classical pottery practice of coiled vessels with 15-25 % granite temper, that treads forward in sharp relief as an expression of the traditional. Although we are not yet at the point where we understand why the chaîne opératoire changed, practice theory helps to conceptualise the practice field as one entity and as an expression of a changed doxa. By applying practice theory we can see the change from the tradition of thousands of years to the new way of dividing the regular (continuation of the classical tradition) and the strange (Bucket-shaped pots) as active and expressive agency within the same field.

As exemplified by this experiment, practice theory, **selected for reasons of typification**, has the necessary concepts in place to generally make sense of intangible expressions and meaning embedded in past practice. As such, to address the intangible in technology should not be considered limited by theoretical and interpretational concepts of technology. Nevertheless, it should be noted that practice theory was only meaningful after the somewhat surprising results became apparent. It did not particularly contribute to either the construction of the research question or the set-up of the experiment.

Practice theory was also less meaningful when the function, action and purpose of a technological action was unknown, such as became evident through the experiments with faceted pebbles and the processing of birch bark tar. Although the experiments silenced certain old interpretations, they did not contribute actual new functions or suggestions for the implements that could shed light on its surrounding practice. In this way, until the practice is known, it is hard to see it as an expression of anything. This is not a result of the theory itself, rather we should acknowledge that theory cannot solve practical issues we face as experimental archaeologists. In many examples, we set out to explore what a practice actually was, as was the case with two experiments in this project, and which is generally a common practice in the experimental approaches to technology (e.g. Blake and Cross 2008; Cunningham 2011; Gansum 2004; Law de Lauriston et al. 2015; Lobisser 2004; Mithen et al. 2008; Piotrowski 1999; Rifkin 2012; Steguweit 2015; Tsur et al. 2015). Before the practice is established, practice theory has little or no point of reference for experimental results.

Certain approaches to technology are very much able to get closer to a technology's intangible premises with the use of experimental archaeology – as is seen in the discourse of skill. The skill discourse therefore shows that intangible facets are not inherently inaccessible. However, the fact that these parts of the discourse are largely concerned with certain technologies (lithics) or centred in certain research environments (Scandinavia and the US) is significant (e.g. Apel and Knutsson 2006a), and express connections to particular theoretical climes for discussion. Such climes make certain foci more or less popular regionally, or relating to technology, and show how theory can act as both an obstacle and a facilitation to our own practice, and to how we steer it to achieve our goals. **The problem of theory seems therefore to be related to the lack of use, rather than the form of the specific theory sets.**

### **9.3.2. The problem of science**

As was discussed in Chapter 2; there are two trends in how we approach experimental practice – as a scientific method (amongst others Blair 2002; Callahan 1999; Magnani et al. 2014; Nami 2010; Outram 2008; Reynolds 1999) and/or as an interpretive method (e.g. Apel 2006; Bakas 2012; Bánffy 2012; Crothers 2008; Drews 2012; Gheorghiu 2011; Rasmussen 2007).

The latter trend expresses itself in the uses of perspectives that are influenced by for instance practice theory, phenomenology, and public archaeology approaches. This is generally research that takes the hermeneutic route into experimental results, and it is this trend that dominates the intangible perspectives found in the study sample. There seems to be little in terms of conceptual obstacles blocking the way into posing intangible questions, as interpretive theories are generally concerned with such. Such experiments may take form as more of an exploration of practice than as a strict test. In the words of Apel and Kjel Knutsson (Apel and Knutsson 2006b: 12), experimental archaeology is like "a continuous motion between a sensually based description and theoretical analysis and reflection."

The other direction may pose more of an obstacle: the widespread assignation of experimental archaeology as a science, and the promotion of scientific,

positivist ideals is one factor that may influence the way we approach the intangible. To illustrate, Knutsson (2006) highlights that the scientific Enlightenment ideal of disembodiedness can cause existential crises when we always have to approach a problem in a causative manner, such as often seen in experimental archaeology. To specify; we cannot always find a quantifiable cause-effect mechanism between intangible and tangible factors of a technology; such as when Tanzanian iron-smelters do not allow women on a smelt because they could disturb the liminal transgression; the birth, of the iron (Barndon 2012). This is beyond scientific perception, but it is still a very prominent factor in the smelts of the *Pangwa* and the *Fipa*. Not just that, it is deemed to actually have an effect on the quality of the iron by the performers of the smelt, who must be assumed to know and have known their technology intimately. Such an example must be taken as an indication that cause-effect is not always the appropriate designation of involved factors. As present-day archaeologists, we already have the tools to access the general, physical laws that describe events like melting points and structural physics. However, as researchers of people, scientific facts are not always what we are aiming to illuminate with our research. It is as a prolongation to this that the analysis of conceptual obstacles will turn to factors that relate to the scientific ideal and that may directly oppose intangible questions and answers.

As seen in the study sample and Table 2, the view of experimental archaeology as a scientific discipline is generally expressed through a focus on tests, hypotheses, variables, measurement, statistical representation and significance, and a prominent part of this discipline is written according to the IMRAD model for scientific writing and presentation.<sup>330</sup> Examples are seen in the theoretical parts of the discourse (Callahan 1999; Cunningham et al. 2008; Domínguez-Rodrigo 2008; Kelterborn 2005; Outram 2008; Reynolds 1999), but elements of these ideals are almost omnipresent in the worldwide presentation of experimental results, with only a few exceptions ; largely from Scandinavia (e.g. Narmo 2011; Planke and Stålegård 2014; Crumlin-Pedersen 1995) or from the ethnoarchaeological discourse (Barndon 2012; Ellen and Muthana 2013; Gosselain 2011).

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330. Model for outlining publications as follows: Introduction, Methods, Results, Analysis, Discussion.

The problem of modelling archaeological inferences on the logics of science was discussed in Chapter 3. It was demonstrated that even if we idealise the hypothetico-deductive method (Boëda 1994; Nami 2010; Reynolds 1998b), and sometimes also claim that we use inductive reasoning (Boëda 1994; Coates et al. 1995; Groom et al. 2015), the actual reasoning performed in archaeological interpretation is neither, because archaeological explanations are just that – they are interpretive, not logically coherent. But regardless what our thought operations are in practice, the HDM has a large and established following in experimental archaeology, often repeated without necessarily addressing what it logically entails (Domínguez-Rodrigo 2008; Kamp and Whittaker 2014; Zipkin et al. 2014). In a similar vein, induction is also brought forward as an unspecified and generalising way of thinking, without at the same time taking its statistical character on board (Vickers 2014).<sup>331</sup> Both logical methods are normal thought operations in regular scientific experiments, so actual lab experiments can apply them. The instant the lab experiment is used to interpret an archaeological problem or situation, when something did not *necessarily* occur, the form of inference shifts to non-necessary abduction (Douven 2011). This is also the case for inductive predictions, which do not on their own entail or include any form explanation in the conclusions (Vickers 2014).

It may not be important which ideal form of inference archaeological reasoning generally applies. However, there is one thing that we do adopt with the scientific ideals of HDM (and IM): the criterion that everything must be quantifiable to be tested. This is natural in the hard sciences, within which formalised experiments were established. In the natural sciences, alternatives to HDM and IM are probably not even considered, as the natural laws that govern all experiments are generally mathematical in character and can therefore conform to both necessary and statistical inference forms (Barrow 1996: 57). This is not a problem in sciences that are built on quantitative modelling or formulae, but the question remains whether it is descriptive for the actual, non-mathematical world (*Ibid.*; Casti and Karlqvist 1996: 29; Rescher 1999: 34f). Archaeology attempts to describe this world as seen by humans,

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331. **For discussion of these characteristics of inductive reasoning, see p. 76f, p. 108ff.**

and while humans discovered (the numerical conversion of the world to) the natural laws, such laws are not able to describe and analyse emotions, morals, taste, home, and countless other elements to human life that we live with every day. If we attempt to uncover what such aspects were in the past, most of us apply other tools than quantification in the form of natural laws.

To quantify is one of the most fundamental ideals of experimental archaeology, and although it is cited (Kelterborn 2005; Reynolds 1999), it does not get the same attention that other criteria do. However, an almost **united** experimental discourse practices quantification as an unyielding criterion for a good experiment, in experiments of all technologies and periods. Quantification is also a decisive factor when discourse participants distinguish between experimental and experiential archaeology<sup>332</sup> (Comis 2010; Heeb and Ottaway 2014; Jeffery 2004; Reynolds 1999). This leaves experimental archaeology with a very difficult ideal to live up to, indeed. The numbers or outputs have to be descriptive to something that also occurred in the past, for us to call it experimental *archaeology*. At the same time, the thoughts [bias] the experimenter puts into the experiment should not. As already discussed in section 3.2.2., as experiment bias is actually prepared for and admitted through the existence of the criterion for actualistic behaviour, it is understood that the person/proxy/experimenter cannot *not* be a human of their own time, including the understanding, knowledge and skill levels that result. It should just count as little as possible. How little this in actuality is, is less important. Although the data output is quantified and cited, the human input and experience that went into performing the experiment gets very little attention (but see Bradley 2013; Hammersmith 2011; Nami 2006). This lack of personal information is very similar to how human performance is (not) reported in the natural sciences. As opposed to in the natural sciences, however, we more often include human interaction and active influence in the operation of our actual experimental procedure. The actualistic criterion for field experiments shows that we are aware of the 'dangers' of the human factor. Even so, the fact that one decided to do *a* or *b* based on a hunch, a guess, a headache, a misunderstanding, due to

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332. However, only a few define the latter of the two, e.g. Barry Molloy (2008: 130), who labels it "physical interpretative methodologies," and Daniel Jeffery (2004: 13) as "concerned with realistically performing tasks in the manner in which they were performed in the past."



time restraints, or because one had little money, is rarely put into a publication. However, that unquantifiable element of human behaviour may of course have been decisive for the experimental [quantified] results – also true in the past. The disproportionate dynamics between sizeable input first through the experiment in practice, and slim output at a later time in publication, results in what we may call a disembodied experiment: an experiment represented by numbers and graphs, which gives a clinical view of the result that corresponds little with the way in which the results occurred: dirtily, odorously, messily and humanly. In this disembodied experiment, there is little room for the intangible of practice, but much room for intangible quantified representations of results.

Quantification may be very useful for certain experiments, but as mentioned, it lacks real use in others. Sometimes it seems like experiments are quantified so that archaeologists can put a number on the results. If true, deductive hypothesis testing was the ideal, a simple Yes/No would be a perfectly valid result. For instance, a set-up could be imagined to be something like this:

Hypothesis: We can make tar in this erect sand structure. Test: experiment.  
Result: Yes.

Although this set-up could be applied for the erect structures (as was in the case of the pit-experiments), we chose to quantify several variables "just in case." One intention was to keep some level of certainty that the structures held the correct temperatures internally, but this would matter little if the other conditions for pyrolysis were not met. The outcome of the experiment would then be "no," which would presumably gain less attention in the discourse than the affirmative.<sup>333</sup> Another intention was to succumb to discourse criteria for subsequent acceptance, further publication and general attention. In the publication of the pit-experiments (Groom et al. 2015), we decided to label our experiments "explorations," as we felt they did not fully stand up to the discourse ideal of published experiments because of the lack of quantified data.

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333. However, the quantification would be meaningful if the outcome was negative; for instance that the structures *did* hold the correct temperature span and that the problem **would** therefore **be** air-flow. In any regard, it would have little to do with intangible factors to the birch bark tar technology, as this error would only be descriptive to our unsuccessful, and as such fictive, structures and bear no relevance to past communities.

For archaeology, this Yes/No binary output is often all that matters. Whether it is a HDM, IM, AM or flight of fancy, is not really relevant to the application of the results. Quantified experiments get published, but often the value of an experiment lies in the fact that it achieved something. – a manufacture or wear pattern similar to the archaeological pattern; for instance use-wear or breakage pattern (Adams 1988; Steguweit 2015), processed a material in a previously untested manner (Juleff 2009; Meijer and Pomstra 2011), mimicked formation patterns in a certain way (Mallol et al. 2013) and similar questions where researchers search for Yes/No answers. As a bronze caster once said; you do not need to gauge temperature when you know it is or is not hot enough. Such knowledge, is of course, intangible, and even though individual, presumably one that is transferrable between past and present through a range of representational markers.<sup>334</sup> A repeated Yes-result, building up a knowledge base, could therefore possibly be used to reach parts of the intangible premises of a technology more efficiently than the quantified data for the same experiment. This brings the experimental over in the experiential, which is often discredited for its lack of (accepted) method (Nami 2010;; Reynolds 1999). However, in the words of Julia Heeb and Barbara Ottoway (Heeb and Ottoway 2014: 163): "As long as an activity is trying to answer a specific question, and the results are related back to the archaeological record, they are part of the experimental process, even if they cannot be described as an experiment." In sum and pragmatically speaking, the experiment can be accepted as an experiment even if it is not a true, quantified test of hypothesis. This is a reality that is displayed numerous times by the publication in accepted media (e.g. Groom et al. 2015; Liedgren and Östlund 2011; Narmo 2011; Vranich 2002; Whittaker and Kamp 2006).

Although unquantified experiments probably warrant more attention regarding the intangible, quantification achieved in experiment can still be very important to archaeological interpretation, such as when research questions concern rates or values directly (Gosselain 1992a; Smith 2001). Such numerical output can

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334. For instance which senses are applied and what one sensory perceives as important. With regards to the tar experiments, some of those could be the acrid smell resulting from evaporation of volatiles, time and heat perception: (not) hot enough.

also make intangible aspects decipherable; for instance indoor climate (Christensen and Ryhl-Svendesen 2014), firewood heat efficiency in very cold winters (Liedgren and Östlund 2011), or travel speed (Englert 2006); all of which create premises for and rely on technological practices: for building, pyrotechnology, indoor crafts, **ship-building, navigation**, and more. This was also visible in the only experiment of the three case studies that reached an actual intangible connection. In the experiment with Bucket-shaped pots (Chapter 6), the result was presented and understood in the form of sensory experience, but was put in relation to direct quantified information of temperature and proportion fraction. The quantification made the quantities of temper understandable.

Nevertheless, in the vast amount of experimental questions asked and trialled, the quantified answer has little or no direct bearing on intangible matters of technological practice, apart from in the skill and cognition discourse. Indirectly, quantified results can report on conditions for practice, which is also true for unquantified yes/no results. However, often when intangible questions are concerned, there is less focus on quantification, and more focus on description and possibility (Elburg et al. 2015; Høgseth 2012; Morin 2012; Narmo 2011).

The lack of experiments on intangible aspects may be due to three issues related to quantification:

- 1) We as a group are not interested in the intangible aspects of technological practice.
- 2) Intangible aspects of technological practice cannot be quantified.
- 3) (Some) Intangible aspects of technological practice can be quantified, but there is at current no established tradition for doing so.

The first issue is not likely to be true, given the nature of archaeology in general, and the recent growth of the skill/cognition, ethnoarchaeological and at some levels the archaeoacoustics, discourses. In either case, archaeological preference is largely individual and will not be speculated on here. Both the remaining statements may, however, be true. A lot of intangible, technological matters cannot be quantifiably tested or reported, possibly most of them. How should one for instance quantify the meaning and importance of traditions, style,

know-how, relationships or prestige for a technological practice?

Even so, in some regards, quantification has been attempted and achieved. **For example**, we, the discourse, agree that there are now many ways to measure gestures and create statistical representations of skill levels (Apel 2000; Eren et al. 2011; Khreishah et al. 2013). This is true, even if skill is truly unquantifiable in itself. I know, you know. Do you know more than me? Very possibly, but can we know, and how can we tell? How much influence did someone's parents have on their skill levels? How important is a tradition? If at all quantifiable, such aspects are quantifiable by representation, just as in the question of skill. Like soundscapes, they can sometimes be measured (Bradley 2013; Díaz-Andreu and García 2012; Marshall 2011), but the interpretation is unquantifiable and reliant on consensus to gain attention.

There can be no clear answer to whether intangible, technological aspects can be quantified. Some can, some cannot, some can be binarily quantified (Yes/No). The reason for why we experiment less with intangible than tangible aspects is not something that can be defined with ease. Firstly, some intangible factors are most certainly lost to modern-day archaeologists. This includes factors that relate directly to individuality, such as thought, preference, and in certain regards skills (Eren et al. 2011). Some other past intangible facets that cannot fully be cogitable to us even with our hardest efforts are soundscapes and their importance to perception of surroundings,<sup>335</sup> the meaning content that symbols or symbolic practices evoke (e.g. Barndon 1999; Gosselain and Livingstone-Smith 1995), and the normality or logics of a connection between self-evident concepts/doxa, such as why the smelting furnaces of the *Pangwa* are female and must be treated as such – although at present archaeologists can map and discuss such logics to some extent (e.g. Barndon 2012).

The empirical origins of the ideal of quantification stems from the same notions that restrain us from a lot of the understandings above; that we have to be able to sense it to know it. This is a reasonable ideal from a discipline based on

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335. This is true also for today. From personal experience as a foreigner to the UK, it can be reiterated that a stranger does not always react the same/"normally" to local soundscapes.

empirical observation of tangible aspects that exist in the form of facts today.<sup>336</sup> Pragmatically speaking, this is how archaeology works and we cannot simply singularly refute it to change it; it will not easily gain the democratic consensus needed. Other disciplines take more care of other parts of the intangible in the past, while archaeology looks at *material* (empirically experienceable) culture and some of the intangibility that precludes and results. In the same way, to quantify the empirical is by far the way we understand material culture in academia worldwide. It is part of our language, and it cannot be expected to change in an instant. Rather, it is more likely to change with the change of paradigms that occurs from time to time, which we can see in the increased non-empirical, abstracted ideas that entered the archaeological discourse after the onset of post-processual archaeology.

As brought forward in Chapter 4, tradition is often considered influential to practice, and archaeological practice is no exception. The status of quantification of the empirical is currently reflected in the tradition of international archaeological publication (and see Kristiansen 2012). At the time of writing this thesis, two out of the five top journals on the world ranking by impact factor<sup>337</sup> are scientific archaeology journals.<sup>338</sup> The strive for scientific formats in archaeology is perhaps popular because it is a format that can suit all archaeologies that have data; independently of local artefact types, regional knowledge of cultures, and culture of presentation. Also, scientific formats, such as the IMRAD, is presented as neutral and detached; which may be convenient for communication between cultures. Quantification can be seen as a key factor in both this universal language and the disembodiedness that results.

In experimental practice specifically, quantification is one of the defining, empirical ideals, and a concept that binds experimental archaeologists as a group to procedure. Experimental archaeology is more material than many other archaeologies, and may possibly rely on a more material framework to

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336. The (structured) notion of 'facts' will not be disputed here, however hermeneutic the standpoint of the author is.

337. According to Thomson Reuters' *Journal Citation Reports (JCR)*.

338. *Journal of Archaeological Science* (#3) was registered with 2.196, and the *Journal of Archaeological and Anthropological Science* (#5) was ranked with 1.878. The *Journal of Archaeological Research* was listed with the highest impact factor for archaeology at 2.500. [Accessed 28/8/2015]

function as wanted and needed by archaeologists. For experimental purposes, quantification currently serves a critical function; for the *general*, consensual form to be fulfilled; for other people to understand our findings regardless of cultural background; and sometimes as a yardstick to guide us in what we can do better. Perhaps quantification is not ideal for understanding a circumstantial context, but the question remains whether we can discredit it entirely for the purpose of the intangible. After all, it is currently part of our tradition, and as such, an intangible aspect of our technological practice, not to be overlooked.

### **9.3.3. Research problems, the tangible and the intangible**

Traditions can take many forms in archaeology. Part of the current tradition(s) in experimental archaeology is for instance related to quantification, but the scientific aspect is not the only factor in our conceptualisation of the experimental process. As was already demonstrated above, the somewhat surprising results of the experiments with Bucket-shaped pots were concluded in the prolongation of the current, regional research discourse about death, burial, fire mediation and metallurgy. Without this position in the discourse, the results would still have been indicating an intangible concept of aesthetics, but the interpretation would most likely have been left suspended until further, contextual research could be undertaken.

Two questions result from this situation: 1) Is the strong backing of a research tradition the reason this particular intangible idea relating to Bucket-shaped pots could come forward? 2) Could an intangible result be promoted or exacted through formatting the research question in a particular manner? The answer to the first question is likely affirmative. The connection to precious metals and heat transformation would not have been brought forward if earlier results (Fredriksen 2006; Fredriksen et al. 2014) had not promoted it. The dynamic discourse made it possible to take a surprising result further in terms of the intangible. Regarding the second question, this is a more complex problem. The discourse tradition shows a mixture of research questions that both address or do not address the intangible aspects of the researched technology. An interesting situation is found in the discourse of skill, which is established within a tradition of researching the intangible circumstances of technologies. In the

skill discourse, research questions are very often constructed to investigate such aspects directly (e.g. Apel 2008; Khreisheh et al. 2013; Knutsson 2006; Whittaker and Kamp 2006; Liardet 2013), and the results generally pertain to the aspects raised in the research question and subsequent investigation. In the present project, neither of the research questions were particularly directed towards the intangible, but were rather left open and unspecified.

The results of the questions raised in the skill discourse are very often achieved through concrete means – through investigating a tangible part of the technology. This is a salient point for how research is being framed, approached and argued. In the present case experiments in Chapter 6 – 8, two of the three experiments were conducted with principal reference to tangible archaeological material. Although all experiments were either successful or moderately so in fulfilling the research question, only one was successful in providing a strong argument for the findings concerning the intangible surrounds of that technology. The experiment that neither framed the research question in terms of intangible technological aspects, nor provided a solid and tangible reference for the results, was unsuccessful in providing even an inkling of insight into the intangible field of practice.<sup>339</sup>

The three case studies provided three completely different variations on the relation between research question, tangible reference and results. The tangible reference is a criterion for good argumentation in most of archaeological discourse, not only the experimental. Because such reference must be considered the norm in a discipline that studies material culture, the situation between moderately unsuccessful case experiments, vaguely formed research questions qua intangible aspect, and the diametrically opposite success seen in the skill discourse. This opens the question of whether there is an increased chance of accessing aspects of intangible technological practice if the *research question* is framed explicitly towards such aspects. To conclude in any direction is at this point not possible to justify for lack of practical, experimental research structured towards exactly this problem. The skill discourse shows that there may be an increased chance of approaching the intangible through a strong

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339. **See Chapter 8.**

relation between the research question, the tangible analysis, and the results. However, the Bucket-shaped pottery experiment shows that this correspondence is not an obligatory criterion to achieve results related to intangible surrounds, as is also discussed by Narmo (2011). It would be interesting to take such research further, based on the experiences and results from the present project.

#### **9.4. Research question 2: Practical limitations to accessing intangible technologies**

As part of the analysis conducted for this thesis, the study of the experimental discourse has brought forward some interesting perspectives on the structure of archaeological experiments and their outcome. As touched upon earlier, there are other parts of experimental practice that may be structured to reach the intangible, such as the experiential input discussed previously, which result in experiments that are not traditionally produced, but still provide interesting and valuable contributions (Heeb and Ottaway 2014; Narmo 2011). One way that has been gaining more of a following recently is seen in the more or less phenomenological trend that uses experiential methods to inform on the intangible, largely, but not exclusively, situated in the UK (Brück 2005; Clarke and Renwick 2013; Hamilton et al. 2006; Harris 2008; Kaltsogianni 2011; Marshall 2011; **Skeates 2011**; Stefanakis and Vlavgilakis 2014; Šálková et al. 2011). What binds these attempts together is often to use first-person experience to inform on how understandings may take shape; in archaeology predominantly of something material (Brück 2005). **While the first-person experience (of the experimenter) is part of any experiment, it is rarely brought forward. This is probably due to the excessive focus on quantifiability, based on positivist ideals, that causes the entire discrepancy between the unmeasurable and uncontrolled and the wish to present something as both measured and controlled. However, just as in phenomenological and sensory experiments, the present person in regular field experiments becomes the human proxy for the past person, and although this is neither quantifiable, objective or corroborative, nonetheless the researcher becomes able to make statements about the experimental experience. This must be considered the opposite of the ideally**



disembodied experiment. In a way, the human experience of the material is exactly intangible, and experimenting the **sensory**, phenomenological way becomes an intangible, qualitative and embodied experimentation.

The value of experiential experiments is easily discredited by criticising of the phenomenological viewpoint altogether, especially by citing a lack of connection to the past. The critique often takes the form of claiming that phenomenology cannot maintain a generalised, cross-cultural understanding of surroundings (Barrett and Ko 2009: 276). This seems like a normal objection from a hermeneutic standpoint, which goes against **generalisation**. More interestingly, this critique is especially strong in landscape archaeology, and comes from the generalising, disembodied tradition of processualism, where it is maintained that a lack of method and an unscientific, uncritical manner makes phenomenological experiments counterproductive (e.g Fleming 1999, 2006). The response has been that phenomenological landscape methodology has been put forward in the form of mapping and experience experiments (Brück 2005; Clarke and Renwick 2013; Hamilton et al. 2006). In a way, to map surroundings in the subjective is what we always do when documenting in the field, and in principle, this is no different in a phenomenological experiment. However, the critique is first and foremost reliant on the interpretive connection to the past, something that is not claimed for regular documentation procedures which are generally trying to make maps legible in and representative for the present.

**Ten experiments in the study sample went into detail about intangible matters of technology, and the phenomenological or experiential experiments made up six of these. But why, then, do phenomenological experiments seem to display a somewhat higher prevalence of discussing intangible aspects of past technologies than 'normal' experiments? In more ways than one, such sensory, first person experimentation does stretch further than just the subjective experience as experiment.** The baseline for the ability to experience 'as if in' the past is that the surroundings or experiences must be (considered) representative. There are strong similarities between honest reconstructions, created for reenactment purposes – explicitly experimental or not – and phenomenological experiments. Reconstructed buildings (with or without fire

extinguisher) provide an experience that can be connected to the intangible; the experience of the technology, the surroundings and the spatial premises; how light falls from the central bonfire, the quality of the indoor climate (Christensen and Ryhl-Svendsen 2014; Liedgren and Östlund 2011; Melis et al. 2011). Reconstructed boats and their reconstructed trials provide very clear premises for various sailing crews within which to experience the technology of the boat. The material end result of the technology; the boat, is not the **ultimate** motive for the technological building of the boat, and this becomes very clear to a participant once they board for weeks on end.<sup>340</sup> In the same way, one can say that a reconstructed house can give the necessary experiences of space that can conceptualise that the house was not only built for dwelling and living, but also for matters of norm, out of necessity in relation to the toolkit, for the sense of home, and that its structure would provide a sense of indoors that can be very different from today. Neither reconstructions nor experiential, phenomenological experiments can provide a sense of how an individual experienced **something** in the past. This is the yardstick for much of the past intangible in any which way; what such experiments *can and do* provide is a sense of how the intangible is more important in the (perception of a) technology than just the physical end result – the object – itself. In that way, phenomenological, experiential, reconstructive experiences and experiments may be giving more information about the intangible than the quantified experiment, as opposed to the traditional view of their lacking knowledge value, probably due to this very characteristic (Forrest 2008; Petersson 2011). **This is an important perspective to consider when discussing future study of intangible aspects of technology through archaeological experiments.**

**Nonetheless, non-phenomenological experiments do also exhibit success with accessing the intangible every so often. As part of the analysis of the practical limitations for gaining access to non-tangible parts of past lives through experimentation, the four of the ten intangible experiments that were not phenomenological in character will be discussed in the search for an answer to the second research question. These four experiments had one thing in common: their research questions all dealt with intangible matters from the**

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340. Interview with sailor on the Sea Stallion, VSM, 19/6/2013.

**beginning:** learning (Eigeland 2011b), music (Lawson 1999); sustenance and mobility (Cunningham 2011) and rituals (Gheorghiu 2011). All the experiments tackled their research questions through different methodological and technological approaches. Eigeland (2011b), as already described on p. # taught beginners to knap and observed the remaining manufacture patterning – a common binary question in experimental archaeology, as discussed in the previous section. By achieving a certain pattern, or lack thereof, she could take her results into a discussion of how the observed archaeological pattern may have been achieved – in this case through a discussion of learning and flint-using strategies due to a native lack of flint in the relevant region.

Graeme Lawson (1999) experimented with Medieval music and tonality in his research on Medieval bone flutes. By examining use-wear and establishing the wear patterns that had formed around certain finger holes more than around others, Lawson could highlight which notes were played more often. Thereafter, he played those tones on epoxy casts of the flutes, both to replicate the notes, but also to investigate the function of the flute design itself. Amongst other things, Lawson discovered that flutes were discarded when the holes were spaced wrongly, just as is the case today. However, the preferential tonality of the time was not for the same heptatonic scales that are preferred in the West today, which is a definite finding that relates to the intangible.

Penny Cunningham (2011) explored medium-term storage properties of small pits in relation to hazelnuts and acorns in her experiment. There are several archaeological finds of storage pits for nuts and seeds around Mesolithic Europe, and Cunningham experimented with six different ways to store nuts in similar small pits over a duration of 20-32 weeks. Her results showed that pits are useful for such storage in a variety of ways, and may indicate that the variety of pit types found were used for different types of stored resource, different type of conditions, or both. She highlights that the difference may be social, but what she counters, more importantly than anything, is the old thesis that hunter/gatherers did not use storage media, as they were not sedentary. Cunningham explores through discussion how storage of nuts would allow a reliable reserve for hunter/gatherers, and that pit storage should instead be interpreted as a risk-buffer to counter hunger in a cyclically mobile group.

The last in-depth intangibly-focussed experiment in this sample is Dragoş Gheorghiu's experiment with technological rituals (2011). Gheorghiu's experiment consists of a replication of the chaîne opératoire of production, of a presumably symbolic ceramic figurine from the Chalcolithic Cucuteni-Tripolye culture, as evidenced by features in archaeological finds. By following the same procedure as the original makers, he gains a sense of the technical premises for and the materiality involved in what he considers must have been a ritual production – an obscure ritual, only available to the makers themselves.

**Conclusive to the discourse study of experimenting with the intangible**, these four experiments are very different in both technology and experimental process. However, both Eigeland and Cunningham explored binarily structured research questions that resulted in Yes/No answers, whereas Gheorghiu and Lawson explored the chaînes opératoires of manufacture and use, respectively. **The sample is too small to conclude with the relevance of these similarities, but what binds all these researchers together is their subsequent discussion of their results in context, instead of leaving the isolated, quantified results to stand alone as is the case with the majority of the sample. After the analysis of the discourse combined with the case experiments performed by the author, it seems clear that if the intangible context is *not* considered, the results will also fall short of shedding light on such aspects of the relevant societies.** Not only are the results of the ten experiments mentioned in this section relevant for insights into the intangible – these experiments are also explicitly made to be so through the interpretation. This is a common archaeological interpretational mode. Nevertheless, this is largely a missing element in the rest of the technological discourse in the study sample<sup>2,341</sup> where these ten articles are the only ones to approach the interpretation of intangible aspects in-depth. The remaining articles generally run only summarily through the contextualised discussion. As 12,7 % of the study sample *were* successful in accessing some degree of intangible information, the lack of such perspectives in the discourse cannot be due to specific practical obstacles within the method of experimental archaeology. When combined with the discussion in section 9.3.3.

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341. 79 articles in total focus on technology in the sample. See Appendix A.2.

On some level, it is as if experimental archaeologists writing for the experimental discourse keep primarily to the experimental, and somewhat less to the archaeological. Perhaps it should be considered, like suggested by Aldenderfer for archaeological interpretation of symbolic behaviour, to focus on a pragmatic, multi-stranded corroboration of the interpretation (Aldenderfer 2012). An experiment could be performed as one part of a project – as parts of an experimental *programme* as mentioned by many (Aubry et al. 2008; Blair 2002; Ollich-Castanyer et al. 2014; Schiffer and Skibo 1987). However, we should not forget that we are not only experimentalists, but also *archaeologists, educated to interpret a variety of aspects, both tangible and intangible*. This entails that we should be able to take our interpretations beyond experiment results, for instance with reference to the finds themselves, site contexts, and other research. This is what Eigeland, Lawson, Cunningham and Gheorghiu do (p. 309), and which seems to make their results so conceivable.<sup>342</sup>

**So: are there practical obstacles to accessing the intangible aspects of technology through experimental archaeology? Through the practical approaches in the experiments detailed above,** the archaeologists create their understanding of the intangible. The same is true for the experiential experiments in the sample, who all focus on practical experience. This may indicate that other experiment structures with primary archaeological references may be designed to experience or materially connect to the intangible in the past. **The experiments in chapters 6 to 8 in this thesis only show that this is fully possible, but may not always be the case.** In these experiments, discussions on the intangible could certainly be made by the use of other archaeological means, but not necessarily the material, practical and experimental. In the end, the material does bind experimentalists to primarily focus in the tangible realm, which was also the reasoning behind the categorisation scheme that conceptualises the relationship between the tangible and the research question (and see Gosselain 2011: 212). Nevertheless, intangible information may occur, such as became evident in the pottery experiment in this thesis.

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342. To base an argument on a variety of perspectives is considered by Baert to create meaning in the pragmatic sense by being democratic, consensus driven and diverse (Baert 2005: 160-163).

The problem seems to be to know when the respective and relevant information will occur. Is it up to us as archaeologists to structure the research questions just right, is it coincidental, is it subjective-personal and reliant on the eyes that see? In the end, none of these questions can be decisively answered by the case studies performed for this thesis, **as the sample size was too small for a definite answer**. Even if it is our primary intention to reach an intangible aspect with a concise research question, **it may not always contribute the information we seek**. In a way, it is reassuring that our method can provide us with surprising results still, even when we predict outcomes (**wrongly**) beforehand. **The answer to the question of practical obstacles is probably twofold: Generally speaking, practical obstacles to the method cannot be said to be the reason why we so often fail to investigate the intangible aspects of technology. Yet, in the specific experiment, there may be several practical obstacles to finding such information: lack of research previously undertaken on the matter, the practical setup or experiment structure, budgetary constraints that will not allow a full exploration of a particular matter, or other practical issues that arise, and which cannot not been planned for, such as weather. However, nor the tangibility of an experiment procedure, nor that of a technological procedure, nor the tangible primary references, are obstacles to accessing the intangible aspects of technology.**

#### **9.5. Self-assessment as a proxy for experimental archaeology**

The evaluative part of an experiment has proven useful to me many times. In a sense, it should be part of any research project, although perhaps not in writing or publication so much as constituting an honest look inwards in the team and in the individual researcher. However, this study includes three experiments, and they should be evaluated as part of this thesis to represent the full scope of the methodology. The evaluation will therefore be iterated here in explicit writing. A self-assessment also relates to the criterion of bias awareness, both set forward by experimental discourse criteria and postulated by hermeneutics, to which I subscribe.

The method of this thesis was based on deconstruction and representation through typification. But can a single person represent the incredible diversity that is inherent in experimental archaeology? This was one of my main concerns. From the onset, experimental archaeology was represented through the discourse; both from definitions, guidelines and circumstances, and it was a conscious choice to do so. The fieldwork in a multidisciplinary environment at the Viking Ship Museum was included to represent the breadth of experimental work, and observations of and discussions with different professional groups were intended to show different viewpoints of and in the methodology. Although it should be clear from the start that the author is leaning towards hermeneutic ideals, attempts were made to stay – if not neutral – then at least as informed as possible of a diversity of perspectives. Of course; to evaluate an entire methodology by oneself is going to fall out to the preference of the author, and I admit that this has indeed happened. Nevertheless, I cannot identify a manner in which this could foreseeably be prevented. Therefore, I let it do so, but at the same time attempted to structure a typified practical methodology for the case experiments that would encompass more aspects than just my own preferential ways.

The typification of experiments could have been executed in many ways. I chose to apply to philosophical pragmatism because it allows and promotes diversity and democracy, but also discourse consensus, likelihood and consequence. That meant that an experiment could be both my way (allowing diversity), and according to a generalised procedure from the written discourse (democratic consensus). It could be used to augment an argument (increase likelihood) rather than validate or logically justify, which is infinitely harder to achieve. And it would be a gratifying paradigm for this evaluation as every once in a while, after a method is introduced and normalised, there should be an assessment of the methodology and its consequences. Philosophical pragmatism would not promote a moral distinction between approaches, theoretical or thematic inclinations, and in this way I found it more suitable than the two alternative paradigms, which most definitely do pass strong value judgements on each other.

There are many ways in which a method assessment can be achieved, and



there is usually room for improvement. In this project, the analysis was largely reliant on practice theory and the chaîne opératoire for the conceptualisation of technologies. Perhaps this prevented a fruitful view of the intangible that could have been obtained through other theoretical foundations, or perhaps theory clouded a clear view that could have been obtained without any theoretical selection at all. In hindsight, the research questions were rather unspecified, especially in light of the discussion above where the connection between research question, tangible reference and intangible results is highlighted.

One element that should be evaluated specifically is the categorisation scheme. Did it attain its goals? Was it at all helpful? It would surely have been possible to say whether or not the experiments had relevance to the intangible without it. However, what the categorisation was meant to do did not function to its full potential in the case experiments, as these more often than not failed to highlight intangible aspects of the respective technologies. It did work as intended in the Bucket-shaped pottery experiment, where the evidence level for technology (and thereby likelihood) was augmented from associated to indirect. The scheme allowed a quantification of success as well as failure, and decisive wording and a fixed conceptualisation about evidence and likelihood levels; all factors that are sought after in much of the experimental discourse. I will maintain that the categorisation scheme may be useful for the conceptualisation of an experiment's proximity to archaeological reference, but it needs further experimentation to assess its total functionality and to be adjusted to experimental practice. It should also be evaluated by external experimenters.

Regarding the experiments themselves, there are several things that could have been improved. The pottery experiment could have included a cooking phase, to determine the functionality of the soapstone proportion in relation to food and heat functionality, as suggested by the discourse (Fredriksen 2006; Kristoffersen and Magnus 2010; Rødsrud 2012). The experiment could also have been designed to include the element of asbestos temper. However, this was consciously excluded due to health and safety considerations. The faceted stones experiment could have involved more tasks as well, such as hide processing. The tar experiment could have included an experimentation phase with receptacles. Nevertheless, it is rarely not possibly or beneficial to expand



an experiment beyond its final form, and in this sense, all three experiments can be considered representative of typical experimental archaeology.

**This thesis is a evaluation of a method that is pragmatic, practice theory oriented, hermeneutic and much more.** Not all technologies could be represented, and not all methodological structures, analysis methods or countries are included in the process of typification. There are probably too few ethnoarchaeological experiments, and too many lithic experiments for some, but the other way around for someone else. As the experimental method led to two negative and one positive outcome, it can be discussed whether or not there is a result to this project. Notwithstanding, at least I feel safe that I raised both strengths and issues that the method holds. After writing this thesis, I know experimental archaeology is wider and more versatile than I could have ever imagined, and that there is no such thing as binary answers. Experimental archaeology is a method that generates experiences, throws up dirt, smells bad and gets attention. I do maintain that whether or not we can access the intangible, other archaeologists know what we do, and they want us to keep doing it.

## 10. Conclusion

As discussed throughout this thesis, there may be several limitations in the way of experiments leading to discussions of intangible surrounds of technological practices. The research questions of this thesis were focussed on two types of limitations: conceptual or practical. Out of conceptual obstacles to discussing the intangible, four factors in particular came forward.

One obstacle revolves around the application of archaeological theory. Not only practice theory, but theory in general, can be decisive for how an experiment is structured and executed. More so than anything else, this relates to theoretical considerations on a paradigmatic level. In experimental archaeology, the widespread idea that the experimental process is formed as a hypothetico-deductive procedure does influence a variety of set-ups. Even more significantly, this notion appears to lead to a consideration of control as something achievable in all experiments, including the scientifically uncontrolled field experiments. This idealisation is dubbed "the modified control theory" by Petersson and Narmo (Petersson and Narmo 2011a: 31). In itself, modified control in an experiment is a paradox, as control works with a very specific purpose: to keep a variable entirely in stasis. In the regular, scientific manner, control is a mechanism that assures that an experimenter knows exactly which variable is influencing the result. When control does not work like this, the experiment is not controlled, and to then give an experiment this denomination is inconsistent with the general application of the label. This may cause both unclear/dishonest reporting and confused peers, and results in wrongful perception of the HDM and what it does. As the HDM is a strict logical inference type with only one possible format, this is also a paradigmatic discrepancy if it is applied due to an idealisation of the scientific.

The first conceptual obstacle to experimenting with the intangible leads directly into the second and widespread, conceptual obstacle, which is likely to be found in scientific ideals that promote the publication of the disembodied experiments. Experiments are often presented as scientific procedures through the use of the IMRAD model, and a visual representation of selected aspects and a clear concealment of others, such as glossing over what a field experiment often

looks and feels like – the intangible circumstances of the field experiment. The IMRAD format is not universally applied, but is currently the form of publication that gives a researcher the highest status on the international scene of experimental archaeology. When experiments are published in books, they seem to have a lower range of influence, and hence those that are published in international, peer-reviewed journals in the above format are highly influential. This is in line with the previously mentioned *Strong Programme* that considers academia a social field of power negotiation between influential groups and individuals, rather than a field of neutral production of knowledge (Barnes et al. 1996).

The scientific ideal results in a quantified representation of the world that may not be ideal for discussing something unquantifiable, such as social aspects, environmental circumstances, or other premises that are intangible, but still very decisive to any technological practice. Although the application of this ideal differs regionally, it has a strong influence in the highest ranked discourses on the global scene of experimental archaeology, such as in the UK, in Italy, and in the US. It is conceivable that experimental archaeology remains focussed on the tangible because the international discourse is the most attractive and prestigious to participate in. Its inclination towards the scientific format may therefore steer the field of experimental archaeology as a doxa.

As indicated through this argument, the discourse structure is reliant on tradition, which is another important conceptual obstacle in the strive for the intangible. Currently the scientific tradition is strong, possibly as it focusses on empirical facts in an empirically founded discipline such as archaeology. Facts are legible between regional traditions, between languages and between theoretic conviction, and most or all archaeologists have to work with primary archaeological references. that are definitely tangible today. The opposite of the scientific discourse; the postmodern discourse, is often unclear and comes forward as unstructured. It presents interpretations that may be hard to understand if you have not read the correct primary sources, and it is positioned far away from the hard facts that we have all been taught to work with. Postmodern interpretation may seem even further away from a material, physical and practical experiment standpoint, than what is currently seen in

other sub-disciplines in archaeology.

One reason that increases this distance is likely to be the ideal of quantification that we often apply to make sense of and communicate our results. We measure variables – it is supposed to be so according to the discourse<sup>343</sup> – and we put the numbers neatly in tables. Although the numbers are concepts and intangible, abstracted representations of the world we live in, it appears from the discourse that we are less concerned with the fact that the distance between the abstracted numbers and the dirty field experiment is as great as that between the past tangible to the intangible. This tradition of automatised, numerical representation of what we do may be part of the problem. However, currently this is our language to talk together, and the lack of experimentation with intangible aspects of technological practice may not change substantially any time soon. Nevertheless, according to the definitions of experimental archaeology, it is changing somewhat, and may have to do with the introduction of these facets of society in general archaeology with the introduction of post-processual paradigm.

Of course, the present thesis is trying to conclude with regards to experimental archaeology on the whole. Certain traditions, like in Scandinavia, are more closely connected to the intangible aspects of technological practice. This is both visible in the regional, small discourse of experimental archaeology and in the general, Scandinavian discourse of archaeology. Because tradition is highly influential to general archaeological and specific experimental formats, the conceptual premises for experimenting is reliant on where people are educated as much as on the general international discourse for experimentation. To conclude an evaluation of experimental archaeology on a general scale is therefore ultimately nothing more than another abstraction.

One indication that comes forward from a review of the discourse of skill, learning and cognition, combined with the case experiments, is the probable connection to the form of the research question. In this discourse, which is generally successful in discussing the intangible through tangible, experimental

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343. See section 2.3.

work, studies often operate with research questions that target the intangible directly, whereas the case experiments in this thesis did not specifically do so. This may be one reason why the success of the case experiments was limited in accessing the intangible surrounds of the respective technologies. Further research should be undertaken to conclude in either direction; including both practical with specifically designed research questions, and theoretical reviews of a larger sample of the experimental discourse successful in achieving such information. Nevertheless, the indicated connection may be significant for future success and broadening of the scope of experimental research.

In practice, many of the same experiment conditions and obstacles apply when an experiment is executed. For instance, we apply the quantification ideal rigorously, which may literally keep us too focussed on displays at regular intervals rather than promoting a free discussion of observations made during the process. The scientific ideal is also likely to resound through the practical concerns of getting published. The vast majority of archaeologists in the international discourse want to get published, and to get an experiment internationally published, we need to format our experiment thereafter – also in practice. That means editing: we make the experiment look proper, including (sometimes unnecessary) tables full of measurements, and sound proper, through a distanced language and correct, disembodied terminology, much like the one applied in this thesis. In practice, an author needs to have access to the correct toolkit to get published, both regarding format, and linguistically.

But the largest practical obstacle to an experimentation with the intangible in technology is probably the practical nature of experimental archaeology itself. As experimental archaeologists, we are practically connected to the physical, to the material, and although on a subconscious, informal level we do probably debate and discuss the intangible and technological practice fields as much as any archaeologist, we still generally maintain a focus on the material right in front of us. We are trained to scrutinise physical material, amongst others through our criterion of archaeological primary reference. This is what we do, and it is the way experimental archaeology has obtained its present form. It is possible that some experimental archaeologists may simply avoid to ask questions regarding intangible matters due to the difficulty they represent for a

material method in a material discipline, but this may be more due to automated subscription to form than as a wilful act.

All this does not mean that it is impossible to access the intangible aspects of a technology, as shown by the experiment with Bucket-shaped pots, and by numerous examples cited throughout this thesis. Questions concerning intangible elements of practice are getting addressed from time to time, and sometimes it gets into a trend, as visible in the questions of skill, learning and cognition. Nevertheless: although most of any technology is probably related to the intangible, only a quarter of technological experiments are concerned with such elements, and only a few of those again in any depth.<sup>344</sup> It should be remembered that archaeology worldwide is not primarily concerned with only the intangible, but instead applies the tangible to inform on the intangible. In experimental archaeology, we are, after all, archaeologists too, and should not be excepted from this process.

There *are* ways to reach the intangible, as demonstrated several times in this thesis, also by one of the three case experiments. One way that springs forward for experimental archaeology is exactly that mentioned for a pragmatic archaeology: to use our skills as archaeologists to juggle not only experimental results, but also to put them together with other perspectives on the same archaeological references. We can experiment, but nothing stops us from also using our general interpretational skills to supply experimental results with additional pieces of information and interpretive narratives. This is what those experimental archaeologists do that are successful in achieving good, in-depth perspectives on the intangible premises and outcomes of archaeological practice. The fact that they do, may be a reminder to the rest of us that we have more skills than just running bellows, carrying heavy loads, maintaining logbooks and taking numbers off displays – although we do that so well.

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344. Appendix A.2.



# Appendices





## **Appendix A. Study sample: experimental publications revised for questions concerning methodology and intangible aspects of technology: publication details and analysis results**

### **A.1. Selection criteria**

The selection of literature was largely based on the size of linguistically available material (English/Scandinavian/Dutch/French/German) for the author, in an *internationally available*<sup>345</sup> medium. In addition, the size of the *published* discourse in a country is considered, which steered the selection so that it is distributed accordingly. Care was also taken to spread the sample across as many technological traditions as possible.<sup>346</sup> In total, 100 published experiments have been revised for this analysis.

The sample is considered generally representative of the experimental discourse, but is of course open to criticism. For instance, a large part of what is normally considered experimental archaeology, which occurs in museum contexts but is not published, is not included. For the purpose of this thesis, which tackles *academic* experimental archaeology, this was intentional. However, it may result in a skewed sample, where for instance the Danish and Polish discourses are not represented in line with the actual practice of what may be considered experimental archaeology. Also, the author is aware that substantial, national discourses may exist that have not been allowed to influence this analysis, such as is the case in France.

The criteria the sample was evaluated after translate as follows:

*Intangible*: that which forms the social and human backdrop for the technological end-product, irrespective of the present day existence of materials or not. This definition excludes unpreserved materials, which are sometimes

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345. Here defined as participation of two countries or more in the publication/publication series.

346. For instance lithic research, metallurgy, maritime archaeology, etc.

considered intangible.

*Deterministic*: generalising statements that do not consider situated circumstances of a given society, for instance the use of social models without reservation.

*Methodological focus*: whether the publication presents a new methodology, or an evaluation/improvement of one, or otherwise focus on the use of a specific methodology.

*Country*: countries of author affiliation at the time of publication. If this is not presented in the publication, present country of affiliation or discourse (July 2015) is applied.

## A.2. Results of analysis

### RESULTS OF PUBLICATION ANALYSIS

Properties	Amount of publications	of Intangible	of technological	of methodological	of total
Intangible	43	100,0 %			43,0 %
Deterministic	27	(9) 20,9 %			27,0 %
Intangible, non-deterministic	34	79,1 %			34,0 %
Technological	79		100,0 %		79,0 %
Intangible and technological	19	44,2 %			19,0 %
Methodological	67			100,0 %	67,0 %
Technological and methodological	21		26,6 %	31,3 %	21,0 %
Technological and methodological; deterministic	5		6,3 %	7,5 %	5,0 %
Intangible, technological and methodological, non-deterministic	6	14,0 %	7,6 %	9,0 %	6,0 %

Table i. Results of analysis of 100 experimental archaeological publications. Results are skewed according to relative size of the different discourses.

### A.3. Analysed publications

APEL, J.

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2007. Grinding flour in Upper Palaeolithic Europe (25 000 years bp). *Antiquity* 81 (314): 845–855.

ARRIGHI, S.

2004. Use-wear analysis on lithic artefacts of Paglicci Cave (Italy): experiments for the recognition of technological traces. *EXAR Bilanz* 3: 51–56.

BLAIR, C.

2002. SMELT: Economies of Scale. In *Experimental Archaeology: Replicating past objects, behaviors, and processes*, edited by J. R. Mathieu. Oxford: Archaeopress.

BLAKE, E. C., and I. CROSS.

2008. Flint Tools as Portable Sound-Producing Objects in the Upper Palaeolithic Context: An Experimental Study. In *Experiencing Archaeology by Experiment*, edited by P. Cunningham, J. Heeb, and R. Paardekooper. Oxford: Oxbow Books.

BORGIA, V.

2004. Functional analysis of the backed tools from the Gravettian layers 23 and 22 of Paglicci cave (Foggia - Italy): the experimental program. *Experimentelle Archäologie in Europa EXAR Bilanz* 3 43–49.

BÖTTCHER, G.

2004. Koptische Nadelbindungstextilien in Museum der Kulturen Basel. *Experimentelle Archäologie in Europa EXAR Bilanz* 3: 211–214.

CAROSCIO, M.

2011. Storage Vessels in the Mediterranean Area in the Late Middle Ages: Interpreting Technical Devices by Ethnographic and Historical Sources. In *Experiments with Past Materialities*, edited by D. Gheorghiu, and G. Children. Oxford: Archaeopress.

CEREZER, J. F.

2014. Ceramic Technology: Fragments of an Experimental Process. In *Technology and Experimentation in Archaeology*, edited by S. Cura, J. Cerezer, M. Gurova, B. Santander, L. Oosterbeek, and J. Cristóvão. Oxford: Archaeopress.

CHRISTENSEN, J. M., and M. RYHL-SVENDSEN.

2014. Household air pollution from wood burning in two reconstructed houses from the Danish Viking Age. *Indoor Air* 2014: 1–12.

CLARKE, S., and E. RENWICK.

2013. A Processional but not Processual Approach to Stanydale Neolithic Temple. In *Experimental Archaeology and Theory. Recent approaches to testing archaeological hypotheses*, edited by F. Foulds. Oxford: Oxbow Books.

CORY-LOPEZ, E.

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CRUMLIN-PEDERSEN, O.

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CUNNINGHAM, P.

2011. Cache or Carry: Food Storage in Prehistoric Europe. In *Experimentation and Interpretation*, edited by D. C. E. Millson. Oxford: Oxbow Books.

DAIRE, M.-Y., C. BIZIEN-JAGLIN, and A. BAUDRY.

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DINELEY, M.

2011. Experiment or Demonstration? Making Fermentable Malt Sugars from the Grain and a Discussion of some of the Evidence for this Activity in the British Neolithic. In *Experiment and Interpretation*, edited by D. C. E. Millson. Oxford: Oxbow Books.

DRISCOLL, K.

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2015. Organic inclusions in Middle and Late Iron Age (5th–12th century) hand-built pottery in present-day Latvia. *Journal of Archaeological Science* 57: 239–247.

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## Appendix B. Bucket-shaped pots: catalogue

CATALOGUE OF BUCKET SHAPED POTS IN CHAPTER 6

Museum catalogue no	Context	Site	Period	Type	Temper material	Comment	Investigated
B10890	Grave find	Vikse Austre, Sveio, Hordaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Tempered with talc and mica (catalogue).	
B10893	Grave find	Vikse Austre, Sveio, Hordaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Tempered with talc and mica (catalogue).	
B14441	Rock shelter	Nordvik, Samnanger, Hordaland, NO	Migration Period	Likely Bucket-Shaped pot	Soapstone		
C5872	Grave find	Aak, Møre og Romsdal, NO	Migration Period	Bucket-Shaped pot	Asbestos		
C12321a	Grave find	Hafsøy, Eigersund, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Interior crust	X
C18425a	Grave find	Vasshus, Suldal, Rogaland, NO	Migration Period	Handled vessel	Undetermined		X
C18425b	Grave find	Vasshus, Suldal, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Painted with black (?) slip	X
C18814a	Grave find	Førland, Suldal, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone and asbestos	Painted with black (?) slip	X
C18814b	Grave find	Førland, Suldal, Rogaland, NO	Migration Period	Handled vessel, ?	Undetermined		X
C18839a	Grave find	Nærheim, Suldal, Rogaland, NO	Migration Period	Handled vessel	Undetermined (granite?)		X
C18839b	Grave find	Nærheim, Suldal, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone and asbestos, (plus other?)	Painted with black and red slip	X
C18842a	Grave find	Nærheim, Suldal, Rogaland, NO	Migration Period	Handled vessel	Undetermined (plus soapstone?)		X
C18842b	Grave find	Nærheim, Suldal, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Painted with black (?) slip	X
C18863	Grave find	Skeie, Suldal, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Painted with black (?) slip	X
C18863	Grave find	Skeie, Suldal, Rogaland, NO	Migration Period	Handled vessels (?)	Undetermined (granite?)		X
C19363	Grave find	Løland, Lindesnes, Vest-Agder, NO	Migration Period	Bucket-Shaped pot	Mica/soapstone	Silvery sheen, interior crust	X

Museum catalogue no	Context	Site	Period	Type	Temper material	Comment	Investigated
S1440	Grave find	Ytre/Indre Lima, Gjesdal, Rogaland, NO	Migration Period	Bucket-Shaped pot	Undetermined	Patched with drilled and pegged repair: Repair slabs were fashioned with pegs to fit prepared holes in the vessel wall.	
S2652a	Grave find	Helliesen, Sola, Rogaland, NO	Roman/Migration Period	Handled vessel			
S2720a	Grave find	Helliesen, Sola, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone		
S2720b	Grave find	Helliesen, Sola, Rogaland, NO	Migration Period	Handled vessel		Burnished, fired in controlled reduction (Stout and Hurst 1985)	
S2720g	Grave find	Helliesen, Sola, Rogaland, NO	Migration Period	Spinning wheel	Soapstone		
S5046g	Grave find	Slettabø, Bjerkreim, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone and mica		
S5403	Grave find	Ogna, Hå, Rogaland, NO	Migration Period	Bucket-Shaped pot	Asbestos	Patched with drilled and pegged repair: Repair slabs were fashioned with pegs to fit prepared holes in the vessel wall.	
S5560	Grave find	Birkjeland, Bjerkreim, Rogaland, NO	Migration Period	Bucket-Shaped pot	Quartz	Food crust	
S5562b	Settlement/house	Birkjeland, Bjerkreim, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone and mica		
S6299	Settlement/house	Hogstad Store, Eigersund, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Food crust	
S6396	Grave find (cremation)	Steinsnes, Haugesund, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Repaired by attaching slab with iron fittings. Also has remnants of iron band.	
S11759	Settlement/house	Gausel, Stavanger, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Silvery sheen	
S11762	Settlement/house	Gausel, Stavanger, Rogaland, NO	Migration Period	Bucket-Shaped pot	Soapstone	Silvery sheen	
Ts 11179.30	Settlement/house	Borge, Vestvågøy, Nordland, NO	Viking Age	Soapstone cooking pot	-		
Ts 7245ee	Settlement/house	Toften, Andøy, Nordland, NO	Migration Period	Bucket-Shaped pot	Asbestos	Food crust	



## Appendix C. Bucket-shaped pots: experiment data

### C.1. Manufacture and preparations of experiments



Figure i: Coil test of the tree clay pastes used in the experiment. This test shows illustrates unsuitability of the paste to be used for coiling due to lack of adhesion in archaeological soapstone blends. From left: 25 % soapstone, 50 % soapstone, 75 % soapstone. Clay: Etrurian marl, Red Earthenware Powdered Clay 1135/2, Bath Potter's Supplies, UK. Soapstone: Vargavågen, Os, Hordaland, Norway. This mix was used for all experiments apart from pilot experiment, where the latter was used with a commercially bought, wet terracotta clay. Photos: Author



Figure ii: Soapstone temper used in experiments, with provenance Vargavågen, Os, Hordaland, Norway.





Figure iii: Preparation of 75 % soapstone slab for vessel wall. Note the covering of powdered soapstone to prevent sticking. Upon firing this becomes a loose powder that can be brushed off the interior wall if necessary.



Figure iv: Preparation of 75 % soapstone slab for vessel bottom. The rolling pin guides (5 mm) were used consistently throughout experiment for all slabs used to manufacture the experimental vessels.





Figure v: Mounting of 75 % soapstone vessel bottom. The clay body was rested on the wall slab before subsequently being cut to shape after the bottom slab was fixed.



Figure vi: Mounting of 75 % soapstone bottom slab. Note the cracking that results from lack of adhesion in the paste. Due to this, the slabs were consequently laborious to adhere, and caution had to be undertaken in the process.



Figure vii: Experimental vessels drying after burnish and decorations are applied.



Figure viii: Manufacture of test-tiles. The paste was rolled to 5 mm thickness using rolling pin guides, and pressed out with a 45x45x50 mm plastic cookie cutter.





Figure ix: Successful pilot bonfire firing, undertaken as part of the ARK2130 course at the Department of Archaeology, Conservation and History at the University of Oslo, May 2014. Vessels were made with 25 % and 50 % soapstone by students, generally with little or no prior experience in pottery manufacture. All vessels survived the experiment.

## **C.2. Experiment 1-1: bonfire firing of Bucket-shaped pottery**

## EXPERIMENT 1-1 BONFIRE FIRING METRICS

Minutes	Temperature °C	Windspeed m/s
0	86,5	
5	421	
10	717	0,9
15	871	
20	897	0
25	861	
30	860	0
35	839	
40	774	0,6
45	699	
50	531	0
55	498	
60	440	
65	412	
70	369	
75	336	
80	285	

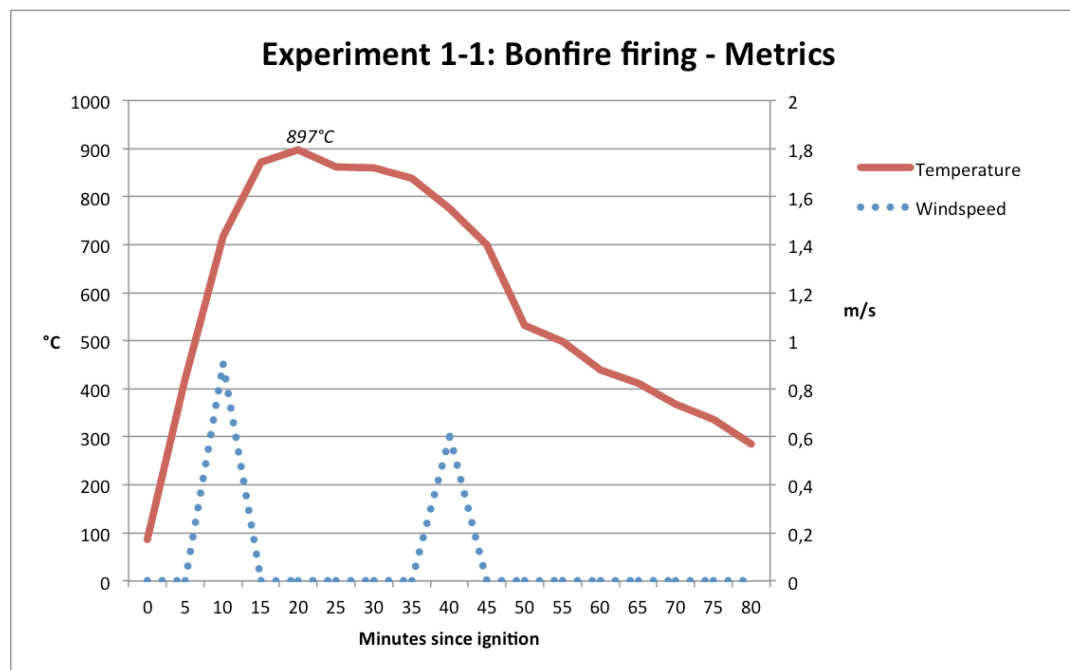


Table ii. /Figure x: Exp 3-1, Bonfire firing: 5 minute temperature interval measured with thermocouple. Max temperature of 906 was not recorded occurred outside of the 5-minute interval. 10 min windspeed intervals were measured during active fuelling of bonfire.



Figure xi: Water-smoking during experiment 3-1. The procedure releases part of the chemically bond water, which evaporates at 100 °C.



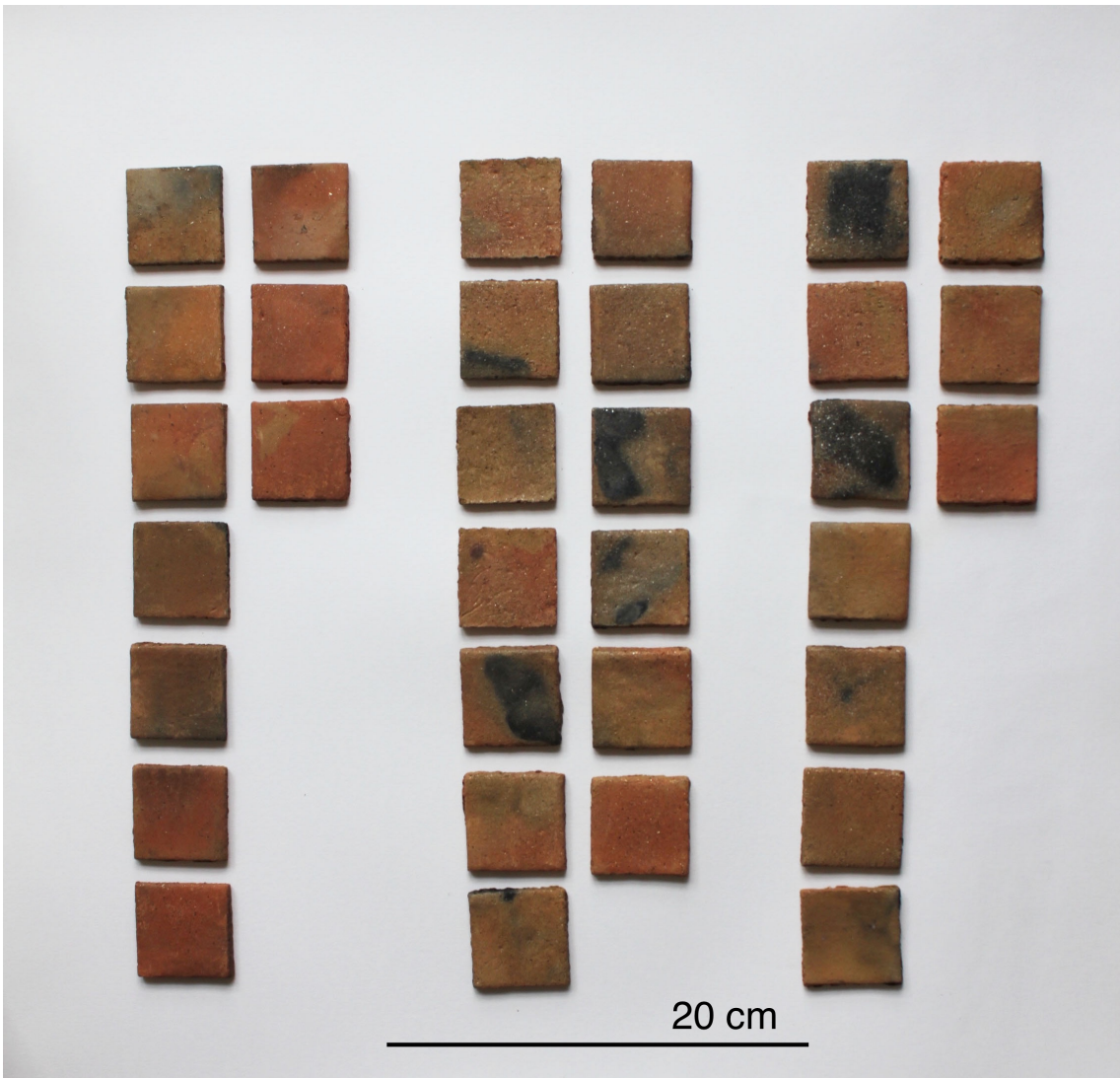


Figure xii: Test-tiles after bonfire firing. From left: 25 %, 50 % and 75 % soapstone. Certain specimen from the original 45 tiles fired could not be retrieved after firing due to fracture and spalling.

### C.3. Experiment 3-2: kiln-firing of soapstone additions

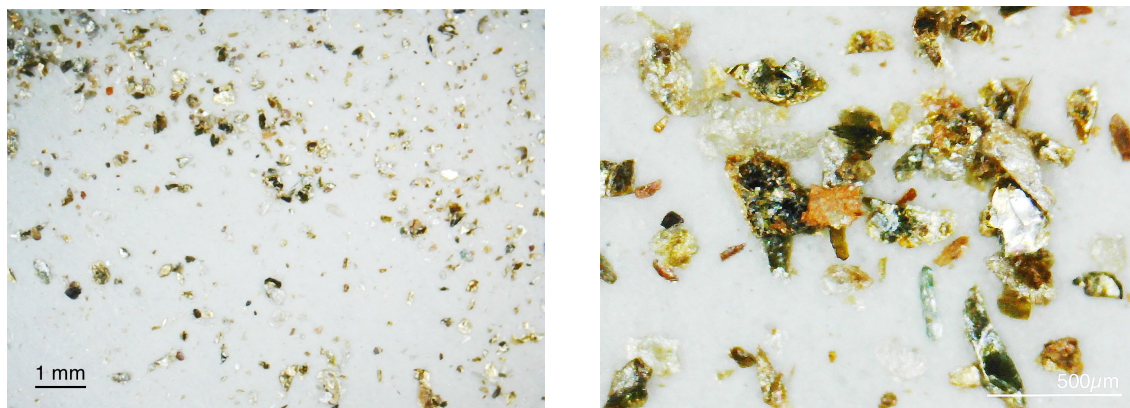


Figure xiii: Soapstone from Vargavågen, Os municipality, Hordaland county, Norway, repeatedly fired in kiln-experiments at 500 °C, 750 °C, and 1000 °C. Sample was taken from the soapstone prepared for the experimental vessels and tiles. The sample exhibit an overall golden appearance, which in detail is shown to include a range of golden, red, and silvery tones.

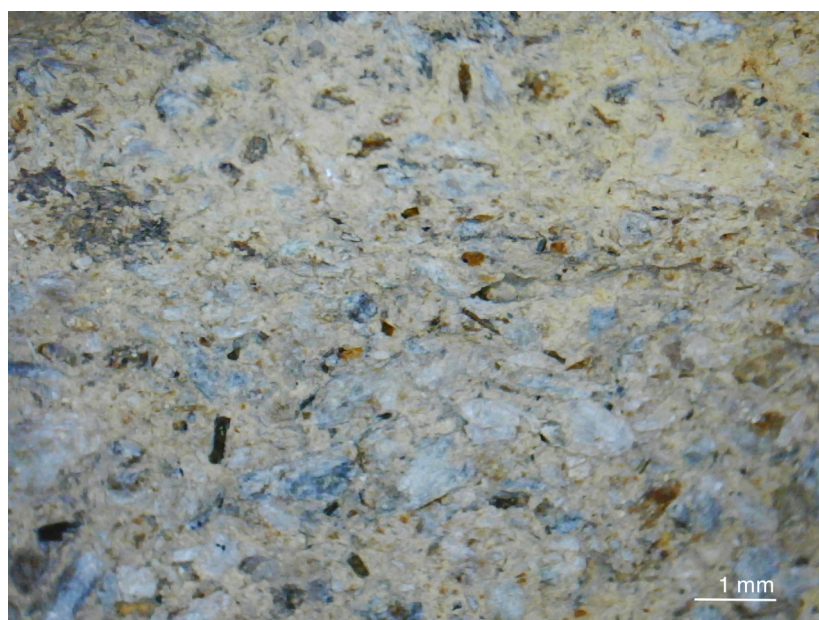


Figure xiv: Temper particles in C18425b, Vasshus, Suldal municipality, Rogaland county, Norway. Grave find. Wall sherd exhibiting a high density of temper. It is unknown whether this pot contains geologically determined soapstone. The catalogue entry (from 1896) details it as "glimmer" – the Norwegian term for mica. This was a common denomination for ceramics later perceived to contain soapstone.

#### C.4. Experiment 1-3: three point bending test results

##### EXPERIMENT 1-3: RESULTS

Tile no	Max. Load (N)	Deflection at Max. Load (mm)	Firing temperature (°C)	Soapstone proportion (%)
25A	118,26	0,52646	750	25
25B	100,30	0,61629	750	25
25C	74,29	1,41120	750	25
25D	82,98	0,45255	750	25
25E	121,24	0,75761	750	25
25F	88,61	0,96400	750	25
25G	102,16	0,32682	750	25
25H	275,68	0,34149	1000	25
25I	270,02	0,77619	1000	25
25J	234,00	0,42569	1000	25
25L	292,39	0,45029	1000	25
25M	234,65	0,59389	1000	25
25N	224,70	0,69644	1000	25
50A	92,57	0,50144	750	50
50B	61,56	0,70391	750	50
50C	77,06	0,35597	750	50
50D	77,41	0,35038	750	50
50E	83,84	0,99878	750	50
50F	92,84	0,47733	750	50
50G	68,07	0,42787	750	50
50I	258,38	0,50263	1000	50
50J	244,22	0,51062	1000	50
50K	200,37	0,58046	1000	50
50M	180,89	0,41489	1000	50
50N	226,42	1,22400	1000	50
50?(H)	88,61	0,54186	1000	50
75B	119,88	0,85968	750	75
75C	82,99	0,60599	750	75
75D	108,81	0,47591	750	75
75E	119,02	0,49346	750	75
75F	89,51	0,51516	750	75
75G	106,91	1,12640	750	75
75H	203,10	0,81910	1000	75
75I	227,78	0,81589	1000	75
75J	179,19	0,39367	1000	75
75K	211,26	0,57493	1000	75
75L	256,36	0,47404	1000	75
75M	169,99	0,56268	1000	75

Table iii. Experiment 1-3. Three-point bending tests of handmade, kiln-fired test-tiles. Resistance to force of load in Newton with deflection in mm. In all three temper proportions (25 %, 50 % and 75 %) there is a pronounced increase in resistance related to firing temperature, rather than temper proportion. As the results are similar for each group, the tests indicate that the firing temperature of the clay (ceramic) is the significant factor.

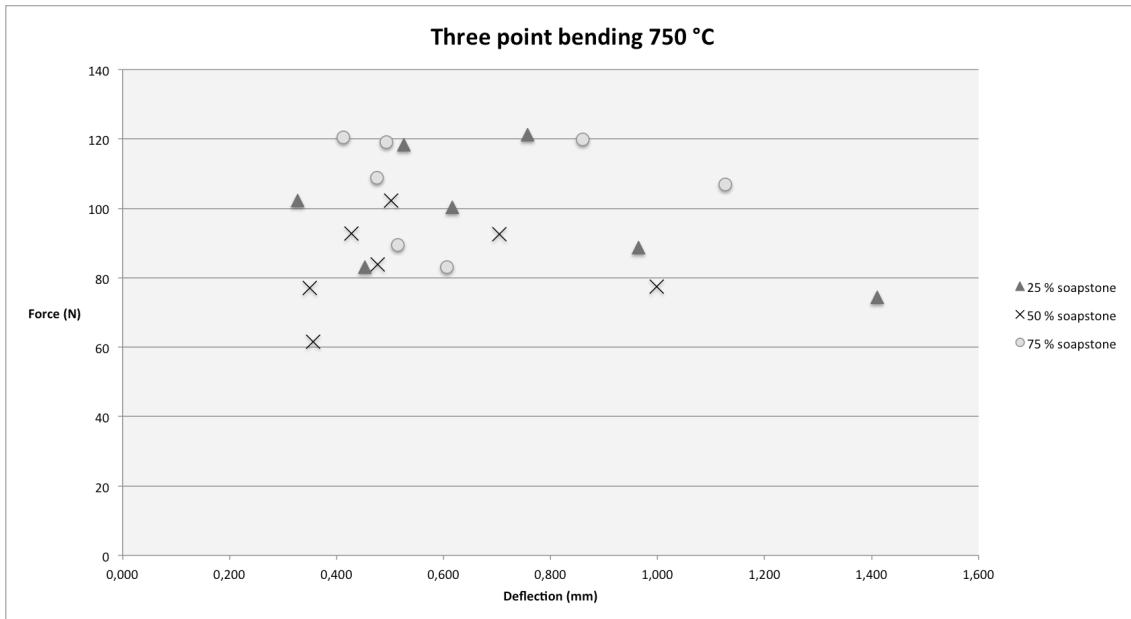


Figure xv: Results of three point bending of handmade test-tiles kiln-fired to 750 °C. The results show a wide dispersion across the X axis, indicating a variety of deflections from 0,33 to 1,41 (both 25 % soapstone samples). The tiles show a closer clustering along the Y-axis, which indicates the amount of force in Newton applied to the tiles upon fracture. All three groups are widely dispersed, and in total, results indicate that neither of the groups are significantly tougher than others.

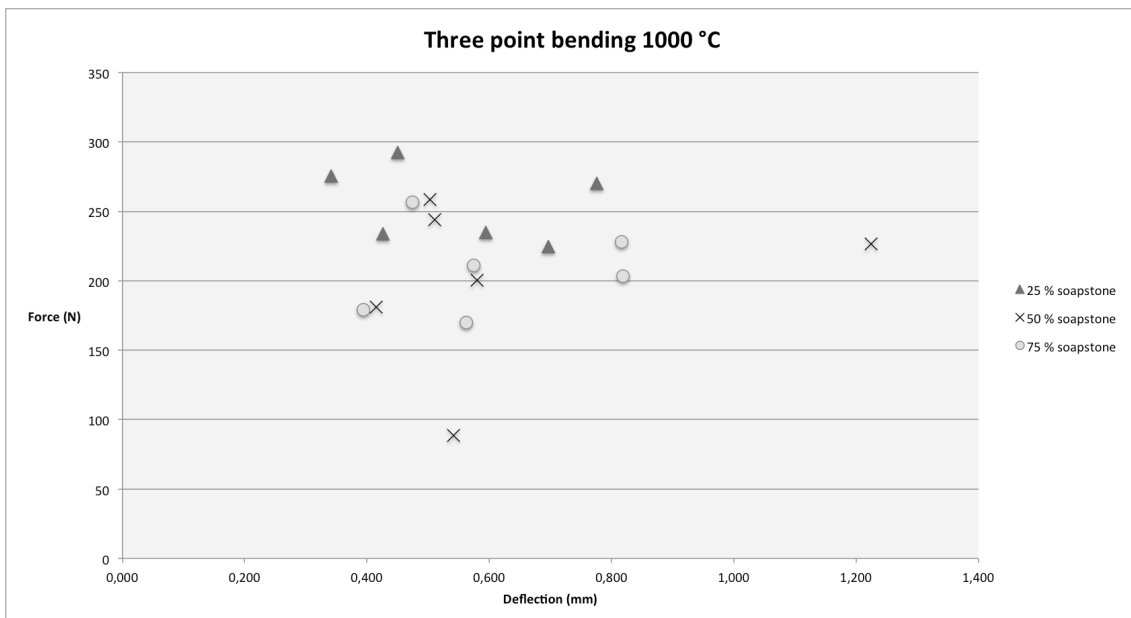


Figure xvi: Results of three point bending of handmade test-tiles kiln-fired to 1000 °C, indicating a similar result as the lower-fired samples. Neither of the groups are significantly tougher than others when considering the specific soapstone proportion. However, all samples are notably stronger when fired to 1000 °C, which attests to the toughness of the ceramic body of the earthenware rather than the soapstone.



## **C.5. Bucket-shaped pots: evidence levels before/after experimentation**

Chaîne opératoire/ Object Biography ↓	Scale →	Min												Max											
		Object				Behaviour				Process				System											
Evidence type →		E	T	C	E	T	C	E	T	C	E	T	C												
1. Manufacture	Direct	█		█	Direct			Direct			Direct														
	Indirect				Indirect			Indirect			Indirect														
	Associated		█		Associated			Associated			Associated														
	None				None			None			None														
2. (Inter-)use	Direct				Direct			Direct			Direct														
	Indirect				Indirect			Indirect			Indirect														
	Associated				Associated			Associated			Associated														
	None				None			None			None														
3. Abandonment	Direct				Direct			Direct			Direct														
	Indirect				Indirect			Indirect			Indirect														
	Associated				Associated			Associated			Associated														
	None				None			None			None														

Chaîne opératoire/ Object Biography ↓	Scale →	Min												Max											
		Object				Behaviour				Process				System											
Evidence type →		E	T	C	E	T	C	E	T	C	E	T	C												
1. Manufacture	Direct	█		█	Direct			Direct			Direct														
	Indirect				Indirect			Indirect			Indirect														
	Associated		█		Associated			Associated			Associated														
	None				None			None			None														
2. (Inter-)use	Direct				Direct			Direct			Direct														
	Indirect				Indirect			Indirect			Indirect														
	Associated				Associated			Associated			Associated														
	None				None			None			None														
3. Abandonment	Direct				Direct			Direct			Direct														
	Indirect				Indirect			Indirect			Indirect														
	Associated				Associated			Associated			Associated														
	None				None			None			None														

Figure xvii: Experiments with soapstone in Bucket-shaped pots: evidence levels of interpretation of the technology: before and after experimentation. The evidence levels, and therefore presumably the likelihood, of the interpretation has increased.



## **Appendix D. Faceted pebbles: archaeological catalogue**

The selection of archaeological specimens in the following catalogue was sampled from the University Museums collated portal – Universitetsmuseenes samlingsportaler – (UNIMUS) database of archaeological artefacts. Artefact study was undertaken at the Cultural Historical Museum of Oslo (KHM),<sup>347</sup> as the implements from Torpum 13/1 and Rørbekk 1 are stored with this museum. The definition of faceted pebble is therefore based on studied finds from Eastern Norway.

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347. This museum is in charge of the archaeology of the nine counties in the Eastern Norwegian region.



PECKED STONES CATALOGUE

Viewed by author	Corresponding to T13/1, -/2, R1	Catalogue ref no	Context	Database denomination	Site	Photo	Macroscopic appearance	Microscopic appearance	Abrasive	Adhesive	Fatigue	Tribo-chemical	Interpretation	Notes
X	X	C53852/31	Mesolithic Nøstvet/phase III - Kjeøy/phase IV settlement	-	Torpum 13, felt 2, Halden, Østfold, NO.	X	T13/1: Fine-grained, granite beach pebble, with one defined (ø 43 mm) and one smaller facet opposite (ø 31 mm). Both facets show very well defined edges have an evenly round shape, and are coarse but level. The pebble is almost entirely round, and has a high profile for the type. Dimensions: 90 x 91 x 74 mm. Weight: 1172 g.	Compared to the natural surface which displays pits and larger expanses of flat surfaces in between, the facets exhibit closer pitting, surface debris and rounded interstices and asperities. The worn asperities have raised features suggesting that contact material was not hard enough to lower them. A few white, twisted fibres with a transverse structure, and what could be several very fine, black hairs (ø c. 5 µm) are lodged in the surface.	X		X			Found close to contemporary beach on settlement site, near area with a high frequency of flint debris, hammerstones, scrapers and a hearth.
X	X	C53852/31	Mesolithic Nøstvet/phase III - Kjeøy/phase IV settlement	-	Torpum 13, felt 2, Halden, Østfold, NO.	X	T13/2: Medium-grained, unevenly round and flattened granite beach pebble, with one defined facet (ø 61 mm. Deep crack/gouge along one side, possibly parallel striations and a few deep pits, otherwise level surface, moderately coarse. Potential frosted areas indicative of fatigue wear, but these also occur on the natural surface and may not be function-related. Facet covers most of one aspect. Dimensions: 85 x 82 x 48 mm.	As above; compared to the natural surface which displays a low frequency of pits and larger expanses of flat surfaces in between, the facets exhibit closer pitting, surface debris and rounded interstices and asperities. As in T13/1, the worn asperities have raised features suggesting that contact material was not hard enough to lower them. A few white, twisted fibres with a transverse patterning are lodged in the surface. One black hair, c. 12-13 µm, is attached to the surface of one of the facets.	X		X			Found close to contemporary beach on settlement site, near area with a high frequency of flint debris, hammerstones, scrapers and a hearth.
X	X	C53854/22	Mesolithic Nøstvet/phase III - Kjeøy/phase IV settlement	-	Rørbekk 1, Halden, Østfold, NO.	X	Oval, coarse-grained granite beach pebble with one defined facet. The opposite aspect of the facet is a level, natural surface, which results in a flattened profile. The facet is coarse, with potential parallel gouges/rough striations. Dimensions: 78 x 74 x 56 mm. Weight: 612.	The pebble displays pitting, surface debris and rounded crystals; the latter less extreme than T13/1 and 2 (C53852/31), but still pronounced. Potential tribochemical wear in the form of sheen and intersecting striations on a few high elevations. White, knotted fibres with clear transverse structure, and one black hair coinciding with those from T13/1.	X		X	(X)		Found close to contemporary beach on settlement site, in an area with very low find frequency.
X	X	C33904 t	Mesolithic Nøstvet/phase III settlement site	Grinding stone	Frebergsvik, Horten, Vestfold, NO.	X	Oval beach pebble with two parallel, prepared facets, one of which has a possible "child" facet along one edge. The facets are coarse to the touch and entirely level. Both facets have almost entirely removed the respective aspect of the stone. Certain areas display a frosted appearance, indicative of (natural?) fatigue wear. Dimensions: 100 x 76,4 x 62 mm. Wt 585 g.	The facets are pitted, with some crystal rounding with ample debris. Embedded in both facets are the same white fibres as in T13/1 and 2 (C53852/31), and C53854/22, severely tangled. Certain fibres appear to be deeply lodged. A few instances of possible tribochemical wear is visible on higher elevations of crystals, on which the sheen differ from the general crystal reflection.	X		X	(X)		In situ location was not specifically documented (Pers. comm Egil Mikkelsen)
X	X	C33904 t	Mesolithic Nøstvet/phase III settlement site	Grinding stone	Frebergsvik, Horten, Vestfold, NO.	X	One half of fine-grained porphyry, originally almost circular, disc-shaped [beach] pebble. Two parallel, flat surfaces of which one is pecked, and one appears natural. The latter surface is smoother than the natural stone, suggesting use. The pecked surface displays a frosted appearance. Originally 96 x 90 (est) x 48,7 mm. Wt 502 g.	The prepared facet displays general pitting, as well as some crystal rounding and some surface debris. However, microscopically One white fibre was observed on the smooth surface. The prepared surface also has white fibres attached to the surface, of which a few have seemingly picked up surface debris.	X		X			In situ location was not specifically documented (Pers. comm Egil Mikkelsen)
X		C14427	Mesolithic settlement (undated in database)	Mace	Torrestad Nedre/ Torrestad Øvre, Sandefjord, Vestfold, NO.	X	Half, rounded pebble with opposite, deep pits central on pebble aspects. Transverse measurements 97 mm. Could be a "fingerpöpsstein" (finger pit stone), or potentially the initiation of a shafthole for a mace.	Crystal structure in central pits is flattened, so that all asperities are at similar level. Interstices remain and crystals are not visibly rounded. Little surface debris.	X				"Fingerpöpsstein"/ mace	Noted in the textual description as "A half 'hammer stone' from the Stone Age."
X	?	C14851	Stray find	Grinding stone	Bråte Østre, Ås, Akershus, NO.	X	Large, disc-like beach pebble with one distinct use-surface/facet (possibly pecked). This facet is deeper than the remaining matter, and very smooth. Clearly defined edge between facet and natural stone, affected by blows - possibly because this is the most protruding point on the aspect. The opposite aspect has a darker spot that could possibly be a form of adhesive wear from manipulation, and also displays a few areas with shock damage. Dimensions: 133 x 153 x 57 mm. Central pit depth: 3-4 mm. Weight: 2371 g.	Crystal structure in the central pit is flattened, so that all asperities are at similar level. Interstices are also somewhat affected and over certain areas erased. Some surface debris. Also displays what appears to be a black residue on this aspect. The opposite aspect displays no notable features, apart from possibly a white fibre. However, this is at current uncertain.	X	(X)				From catalogue entry: "Hammer stone(?), formed on a reasonably large, oval beach pebble. (Dim: 15,8 x 13,8 x 7,1 cm) on which one side is pecked a fairly large concave surface, but which otherwise does not show signs of wear."

Viewed by author	Corresponding to T13/1, -/2, R1	Catalogue ref no	Context	Database denomination	Site	Photo	Macroscopic appearance	Microscopic appearance	Abrasive	Adhesive	Fatigue	Tribo-chemical	Interpretation	Notes
X	X/?	C24118b	Stray find	Grinding stone/runner	Raastad, Vestby, Akershus, NO.	X	Round beach pebble with two opposite facets, both covering the majority of the respective aspects. Facet (a) displays a deep crack in which is lodged a white/pale residue. (a) is also clearly demarcated, whereas (b) is less so and has potential wear trace along the facet edge. A secondary "child"-facet has formed along (b)'s edge. All three facets are very smooth to the touch. There is some damage to the circumference of the stone on the widest point, but whether cultural or natural cannot be determined without analysis. This stone appears to be a typical specimen of the type, similar to C33904 t syenite pebble from Frebergsvik. Dimensions: 76 x 76 x 58 mm. Facets (a): ø 48 mm (b): ø 45 mm. Weight: 670 g	The facets show a flatter and more even structure than the otherwise pitted, natural surface. Interstices appear to display a degree of clogging, but this may be the natural cementation of the particles. There is little surface debris, however, in the crack on (a), there is a substantial amount, possibly pushed into the crack by abrasion on the smoother surface. White fibres are found in the crack, some have a clear transversal pattern and are 25-28 µm thick. Additionally, the macroscopically visible white residue turns out to be two large pieces of shiny, cream-coloured fibre with clear parallel patterning/structure is lodged in the crack, between 80 and 100 µm thick. This substance is somewhat shredded, and smaller fibres of protrude; 14-19 µm thick and somewhat resemblant of the smaller white fibres. The larger substance bears a resemblance to sinew fibres. Nevertheless, due to its situation in a negative space, it was hard to get a proper photo.	X					Found close to C24418a (rhomboid miniature, polished Neolithic axe) and C24164a (lithic yarn sinker) and b (pecked pebble/"grinding stone")
X	X	C24164b	Stray find	Grinding stone	Roverstad, Vestby, Akershus, NO.	X	Oval, small beach pebble with one level facet, and an otherwise high, and close to circular, profile. A few modern striations, probably from trowel, otherwise an even, coarse facet with a well defined edge. Dimensions: 62 x 47 x 55 mm. Facet: 40 x 32 mm. Weight: 332 g.	Substantial pitting has developed on the surface, which additionally displays rounded crystals, surface debris and what seems to be parallel tribochemical wear. The same tangled white fibres with transverse structure appear lodged the surface, along with what may be very fine, black and short hairs. However, the latter is hard to substantiate without further analysis.	X		X	X		Found close to C24418a (rhomboid miniature, polished Neolithic axe) and b (see above), as well as C24164a (lithic yarn sinker)
X		C33214	Stray find	Mace	Åsgården, Horten, Vestfold, NO.	X	Oblong, classical mace-head with a triangular form and relatively small, opposite and unfinished shaftholes. This was chosen as a comparative specimen as there is one interpretation of the pecked pebbles that they are the beginnings of shafthole formation. Dimensions: 63-48 x 43 x 160 mm. Weight: 715 g.	Substantial pitting, frosted appearance, and a large amount of surface debris, as well as no levelling points to a pecking (fatigue). Crystals maintain some angularity. Potential white fibre lodged in the surface, as well as what appears to be fine, black hairs.			X		Mace	Also described as hammer stone, but this is unlikely.
X	X	C33780	Stray find	Grinding stone	Skaufjeld, Sande, Vestfold, NO.	X	Two opposite facets that cover entire aspects of the pebble. One facet (a) is coloured as remainder of stone, but the opposite (b) is entire blackened. This surface is larger, and possibly smoother than (a). The edge of (a) is well rounded, and at one point used to the extent that a new "child"-facet (c) has been formed. Dimensions: 114 x 112 x 62 mm Facet (a): ø 65 mm. Facet (b): ø 72 x 67 mm. Weight: 1431 g.	Facets (a) and (b): Clear pitting, and well-rounded crystals/asperities and interstices. Certain raised areas are entire flattened and have accumulated sheen (tribochemical wear). Loose crystals on both surfaces, as well as a potential, rust-coloured residue on edge of (a). White, twisted filaments on both facets.	X		X	X		In database interpreted as Nøstvet due to proximity to Frebergsvik, where C33904 t were found, and other local Nøstvet sites.
X	X	C36766h	Stray find	Grinding stone	Gommerud, Lier, Buskerud, NO.	X	Coarse, round beach pebble with flattened/disc-like profile. Two opposite use-facets are prepared. The facets are level, coarse, and generally with a clear demarcation, but with a rounding of the edge on one side (a) which could be use-wear. A few shock injuries are apparent on either facet, probably of natural cause. As with C24118 b, the circumference of the stone has suffered some shock injuries, also probably natural. One facet (b) has clear, modern striations, probably from a trowel. Dimensions: 82 x 73 x 44 mm. Facets: (a): 42 x 50 mm. (b): 58 x 41 mm. Weight: 519 g.	Deep, natural pitting is clearly levelled and smoothed by wear. The crystals are rounded and flattened, and interstices are rounded. Little if any surface debris. Potential rust-coloured residue.	X		X			Found near C36766a-g and i-p. The artefacts consist of various Nøstvet types in addition to a generic lithic assemblage. Collected by local farmer over time.
X		C57934/51	Mesolithic (Nøstvet/phase III) settlement	Hammer stone	Nøstvet, Ås, Akershus, NO.	X	Disc-shaped small stone exhibiting classical knapping (fatigue) wear on opposite edges/aspects. Darker areas of adhesive wear, probably from handling. Dimensions: ø 63 mm.	Deep, irregular pitting resulting from knapping a harder stone. Some crystal fractures and a large amount of surface debris. The latter could be sediments or a result of platform preparation by rubbing.	(X)	X	X		Hammer stone	

Viewed by author	Corresponding to T13/1, -/2, R1	Catalogue ref no	Context	Database denomination	Site	Photo	Macroscopic appearance	Microscopic appearance	Abrasive	Adhesive	Fatigue	Tribo-chemical	Interpretation	Notes
X	X/?	C57934/52	Mesolithic (Nøstvet/phase III) settlement	Unknown	Vinterbrokrysset, Nøstvet, Ås, Akershus, NO.	X	Large, round and flattened beach pebble with two opposite facets. Facet (a) displays large gouges which appear more or less parallel. (a) is further divided into to equally large parts by a ridge, presumably caused by using the stone in two opposite directions - also apparent from the gouges. Facet (b) has a similar division into two parts, with a rounder edge around the periphery. The edge may have been worn into the beginnings of a "child"-facet. The entire form is possibly a result of intensive use, maybe by changing hands whilst using. Dimensions: 120,5 x114,2 x 68 mm. Weight: 1045 g.	Microscopically, the facets show a greater degree of pitting than the natural surface. The crystals are rounded, however, they naturally also exhibit this feature. Certain areas appear to have a small amount of tribochemical wear on higher elevations. There is generally little surface debris apart from what is likely to be sediments lodged in the coarse rock.			X	X		
		C52276	Late Mesolithic settlement.	Hammer stone	Dal Søndre, Frogn, Akershus, NO		From database: "Stone of granite, like a hemisphere. Flat, oval top aspect, 8,8-10,8 cm with wear trace"							
		C50541 a	Stray find	Grinding stone	Haug, Ullensaker, Akershus, NO.		From database: "Grinding stone? Round, flat stone. Diam 9,0 cm T 4,3 cm."							
		C33967 i	Mesolithic Nøstvet/phase 3 settlement.	Stone	Hegg, Søndre, Holmestrand, Vestfold, NO.		From database: "Rounded stone with striations and grinding marks on one surface. Largest dimension 7,2 cm"							
		C38068	Stray find	Runner/ grinding stone	Bolstad Mellelem, Sande, Vestfold, NO.	X	From database: "Top half of quern (mano) of unspecified rock. Round, with one smoothly ground surface ø 70 mm and more smaller, rougher wear/grinding facets around the edges. Largest ø 108 mm." Based on the database picture, this stone appears to have classical, unidirectional grinding striations on its entirely smooth facet, but the shape is otherwise similar to the investigated type.							
	(X)	T19363	Stray find	Grinding stone	Fremstad, Agdenes, Sør-Trøndelag, NO.	X	From database: "Smooth beach pebble, grinding stone, with oval form and to level, oval grinding facets on the broad aspects. Dimensions: 113 x 82 x 54. Weight 770 g." The database picture shows this stone as fine-grained, with a typical, oval facet encircled by a demarcated edge. The edge has suffered some newer fatigue wear. Nothing further can be said without further investigation.							
	(X)	T17216	Stray find	Seam smoother	Nesmo, Osen, Sør-Trøndelag, NO.	X	From database: "Oval beach pebble of white quartz where both broad aspects form level surfaces. It is reasonable to assume that the piece has been in use as a seam smoother. L: 11 cm." The database picture shows that the facet on this stone was most likely pecked, as it is pitted and coarse comparing to the otherwise smooth pebble.							
	(X)	T17641 b	Stray find	Hammer stone	Hansvik, Vikna, Nord-Trøndelag, NO.	X	From database: "A large beach pebble of brown quartzite with two opposite, flattened aspects. Should most likely be considered a hammer stone. Largest diam. 12,2 cm." Based on the available picture, this stone looks to be of the classical, investigated type. The visible facet appears coarser than the natural surface, and also exhibits a very distinct edge which has seemingly suffered some more recent fatigue wear.							

Viewed by author	Corresponding to T13/1, -/2, R1	Catalogue ref no	Context	Database denomination	Site	Photo	Macroscopic appearance	Microscopic appearance	Abrasive	Adhesive	Fatigue	Tribo-chemical	Interpretation	Notes
	(X)	T18689	Stray find	Hammer stone	Misund Indre, Midsund, Møre og Romsdal, NO.	X	From database: "Oval, flat beach pebble which seems to have trace after use as a hammer stone. L. 6,9 cm." This artefact has a clear facet, which appears to be pecked, based on its coarse surface and visible pitting. The edge is less pronounced than in the classical type, and in some areas visibly smoothed. Certain areas on the facet also display an extensive flattening, which probably results from abrasion.							
	(X)	T18988 a	Stray find	Hammer stone	Karlestrand, Bjugn, Sør-Trøndelag, NO.	X	From database: "Hammer stone (?) of unspecified mineral: Oval stone with ground facets along the (two) sides. Pecking marks on the ground facets, but not along the edges. Dimensions: 10,3 x 8,7 cm. Weight 730 g." According to the picture, this pebble looks classical, with clear peck marks centrally on the displayed facet, some possibly a later addition as they have a brighter appearance than the remainder of the facet and stone. The facet has a clear edge, and the type looks otherwise classical.							
	(X)	S12792/99	Middle Neolithic settlement	Grinding stone	Stokkdal, Karmøy, Rogaland, NO		Flattened, oval/round beach pebble of the classical type with pecked facet and a marked edge was uncovered at an excavation where the author took part. It was not further documented for this thesis. From the database: "Oval, well rounded stone with relatively flat broad sides. Clear peck/knapping marks on both sides. Dimensions: L: 8,8 cm."							
		C37175/71 967	Unknown	Unknown	Oslogt 6, Gamlebyen, Oslo, NO.		From database: "Unknown. Pecked, round, flat stone"							
		C38626 41 Reg 99	Middle Neolithic settlement	Mace	Auve, Sandefjord, Vestfold, NO		From database: "Oval mace with unfinished shaftholes, pecked on both sides." A picture of this artefact can be found in Østmo 2008.							
		B13350/10	Unknown	Grinding stone	Osland, Høyanger, Sogn og Fjordane, NO.		From database: A grinding stone. Flat, roundish oval. Pecked around parts of the circumference. A flat area is worn or pecked on both "long sides". Only one aspect is slightly levelled by grinding. Largest dimensions: 10,9 cm."							
		C52162	Stray find	Mace	Haugan, Sandefjord, Vestfold, NO.		From database: "Unfinished, near barrel shaped mace of unidentified mineral with shaft-hole started from both aspects. Diam. 6,7 cm." This must be considered a very small size for a mace-head.							
		C38902	Stray find	Mace/ Hammer stone	Bakke, Sarpsborg, Østfold, NO.		From database: "Pecked mace or hammer stone in grey stone. The artefact is barrel shaped with round cross section and flattened ends. Length: 10 cm, width: 5,4 cm"							
	(X)	B13403	Stray find	Grinding stone	Mjelda Midtre, Osterøy, Hordaland, NO.	X	From database: "One grinding stone of grey sandstone. Oval circumference and flat-oval cross-section. Top and bottom aspects are ground flat. The stone is shaped by pecking. A rounded edge encircles the entire specimen. L: 9,5 cm." The pictures shows that the visible facet is entirely blackened apart from a central patch of wear.							
	(X)	T15335	Stray find	Crushing stone	Strøm, Averøy, Møre og Romsdal, NO.	X	From database: "Crushing stone ('grain crusher') of oblong beach pebble with two opposite, round facets, one smaller and slightly concave. L: 13,1 cm"							
	(X)	C31308	Stray find	Grinding/ crushing stone	Kjellerød, Råde, Østfold, NO.	X	From database: "Crushing stone for push-quern (?) of fine-grained mineral, spheroid with two level facets. Diam. 9,2 cm."							

Viewed by author	Corresponding to T13/1, -/2, R1	Catalogue ref no	Context	Database denomination	Site	Photo	Macroscopic appearance	Microscopic appearance	Abrasive	Adhesive	Fatigue	Tribo-chemical	Interpretation	Notes
	(X)	C22072	Stray find	Grinding stone	Blesa, Fet, Akershus, NO.	X	From database: "Crushing stone of grey granite as Müller: Aarb. 1907 p 153. Largest width 7,3 cm."							
	(X)	B14462	Stray find	Hammer stone	Kvalvik, Flora, Sogn og Fjordane, NO.	X	From database: "Hammer stone, round in shape, but level on two opposite aspects. Wear trace all around. Diam. 8-8,5 cm, height 4,2 cm."							
	(X)	B12879	Mesolithic/ Neolithic settlements	Grinding stone	Flatøy, Meland, Hordaland, NO.	X	Not described in database, but two potential specimen are seen on an image in the UNIMUS photography base. The dating is not provided in the catalogue. The same catalogue reference provides an artefact inventory of both Middle/ Late Mesolithic (keel shaped core) and Neolithic (ceramics) date.							
	(X)	T14685	Stray find	Rubbing/ crushing stone	Hammersøen, Namsos, Nord-Trøndelag, NO.	X	From database: "Beach pebble shaped rubbing or crushing stone, worn level on both sides. Length 11,6 cm."							
	(X)	T13387 g	Stray find	Rubbing stone	Haukebøen, Molde, Møre og Romsdal, NO.	X	From database: "An almost spheroid rubbing stone ('grain crusher') with flattened poles, 7,9 x 6,6 cm. The shape obviously results from pecking." Found in the vicinity of a polished adze, and a megalithic flint dagger T13387 c, both typically Neolithic.							

## **Appendix E. Faceted pebbles: experiment data**

### **E.1. Experimental reference collection catalogue**

The following catalogue is a summary of the experimental conditions and surface features of the experimental reference collection created for the use-wear analysis of the faceted pebbles in Chapter 7.

PECKED STONE EXPERIMENTS

Exp	Status	Activity	Time	Subsurface	Facet type	Stone ID	Facet ID	Control	Macroscopic appearance	Microscopic appearance	Abrasive	Adhesive	Fatigue	Tribo-chemical	Similarity arch	Notes
2-1	Pre-experiment	Pecking/production	30 mins	-	Natural	1	a	-	Crushing	Rough surface, sharp crystal edges, step fractures, cracks, frosted appearance			X		Y	Pecking was maintained until surface extent resembled archaeological specimen
2-1	Experiment	Pecking/production	40 mins	-	Natural	2	a	-	Crushing	Rough surface, sharp crystal edges, step fractures, cracks, frosted appearance			X		Y	Pecking was maintained until surface extent resembled archaeological specimen
2-1	Experiment	Pecking/production	40 mins	-	Natural	3	a	-	Crushing	Rough surface, sharp crystal edges, step fractures, cracks, frosted appearance			X		Y	Pecking was maintained until surface extent resembled archaeological specimen
2-2	Pre-experiment	Cracking hazelnuts	63 mins	Flat stone	Natural	7	a		Brown residue	-					N	
2-2	Pre-experiment	Cracking hazelnuts	63 mins	Flat stone	Pecked	7	b	Y	Brown residue, gloss, smoothing	Levelled crystals, trenches	(X)		X	X	N	
2-2	Experiment	Cracking hazelnuts	63 mins	Flat stone	Pecked	8	a	Y	Brown residue, gloss	Levelled crystals, trenches	(X)		X	X	N	
2-2	Experiment	Cracking hazelnuts	63 mins	Flat stone	Pecked	8	b	Y	Brown residue, gloss, smoothing/levelling	Levelled crystals, trenches	(X)		X	X	N	
2-3	Pre-experiment	Grinding hazelnuts	63 mins	Granite slab	Natural	9	a	N	Striations, oil buildup	-					N	
2-3	Pre-experiment	Grinding hazelnuts	63 mins	Granite slab	Pecked	9	b		Levelled areas, surface clogging	Level surface, (waxy) luster, scattered debris	X			X	N	
2-4	Pre-experiment	Pounding sinew	63 mins	Birch trunk w/bark	Natural	4	a		No discernible trace	Possibly a slight crystal rounding			(X)		N	
2-4	Pre-experiment	Pounding sinew	63 mins	Flat stone	Natural	4	b		Little to no discernible trace	Possibly a slight crystal rounding	X			X	Y	
2-4	Pre-experiment	Pounding sinew	63 mins	Birch trunk w/bark	Pecked	4	d	N	Smoothing of surface	Surface debris, rounded crystals, some gloss	(X)		X	X	Y	
2-4	Pre-experiment	Pounding sinew	63 mins	Flat stone	Pecked	4	e	N	Smoothing of surface	Surface debris, rounded crystals, some gloss	X		X	X	Y	
2-4	Experiment	Pounding sinew	63 mins	Birch trunk w/bark	Pecked	5	a	Y	Discolouration/oily residue, some smoothing of surface	Surface debris, rounded crystals, some gloss	X		X	X	Y	Oily residue seems connected to state of sinew
2-4	Experiment	Pounding sinew	63 mins	Flat stone	Pecked	5	b	Y	Some smoothing of surface.	Surface debris, rounded crystals	X		X		Y	
2-4	Experiment	Pounding sinew	63 mins	Birch trunk w/bark	Pecked	6	a	Y	Little discernible trace	Surface debris, rounded crystals	X		X		Y	
2-4	Experiment	Pounding sinew	63 mins	Flat stone	Pecked	6	b	Y	Smoothing of surface, oily residue	Surface debris, rounded crystals	(X)		X	X	Y	Oily residue seems connected to state of sinew

## **E.2. Faceted pebbles: evidence levels before/after experimentation**



Chaîne opératoire/ Object Biography ↓	Scale →													Min	Max	
	Object	Behaviour			Process			System								
Evidence type →		E	T	C		E	T	C		E	T	C		E	T	C
1. Manufacture	Direct				Direct				Direct				Direct			
	Indirect				Indirect				Indirect				Indirect			
	Associated				Associated				Associated				Associated			
	None				None				None				None			
2. (Inter-)use	Direct				Direct	■		■	Direct				Direct			
	Indirect				Indirect				Indirect				Indirect			
	Associated				Associated		■		Associated				Associated			
	None				None				None				None			
3. Abandonment	Direct				Direct				Direct				Direct			
	Indirect				Indirect				Indirect				Indirect			
	Associated				Associated				Associated				Associated			
	None				None				None				None			

Chaîne opératoire/ Object Biography ↓	Scale →													Min	Max	
	Object	Behaviour			Process			System								
Evidence type →		E	T	C		E	T	C		E	T	C		E	T	C
1. Manufacture	Direct				Direct				Direct				Direct			
	Indirect				Indirect				Indirect				Indirect			
	Associated				Associated				Associated				Associated			
	None				None				None				None			
2. (Inter-)use	Direct				Direct	■		■	Direct				Direct			
	Indirect				Indirect				Indirect				Indirect			
	Associated				Associated		■		Associated				Associated			
	None				None				None				None			
3. Abandonment	Direct				Direct				Direct				Direct			
	Indirect				Indirect				Indirect				Indirect			
	Associated				Associated				Associated				Associated			
	None				None				None				None			

Figure xviii: Experiments with faceted pebbles: evidence levels of interpretation of the technology: before and after experimentation. The evidence levels, and therefore presumably the likelihood, of the interpretation has not changed.

## Appendix F. Birch bark tar experiments

### F.1. General information: experiment conditions

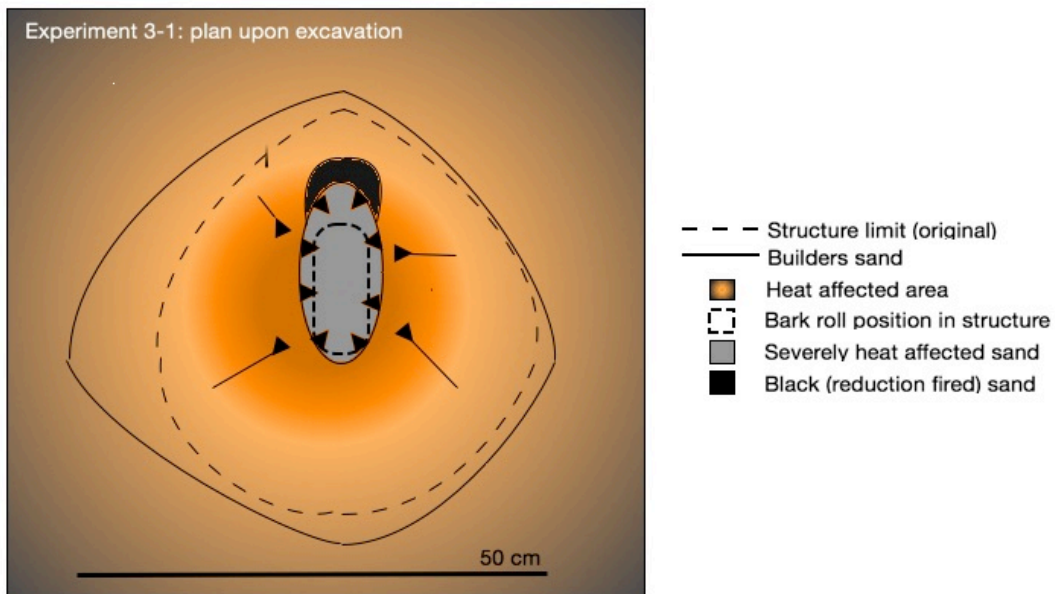


Figure xix: Plan drawing of experiment 3-1 upon excavation and removal of the bark roll. The severely heat affected and reduction fired sand, as well as the tightly encasing, sintered sand around the bark roll, was seen in all experiments apart from 3-2. The subsurface consisted of a deep layer of garden gravel.

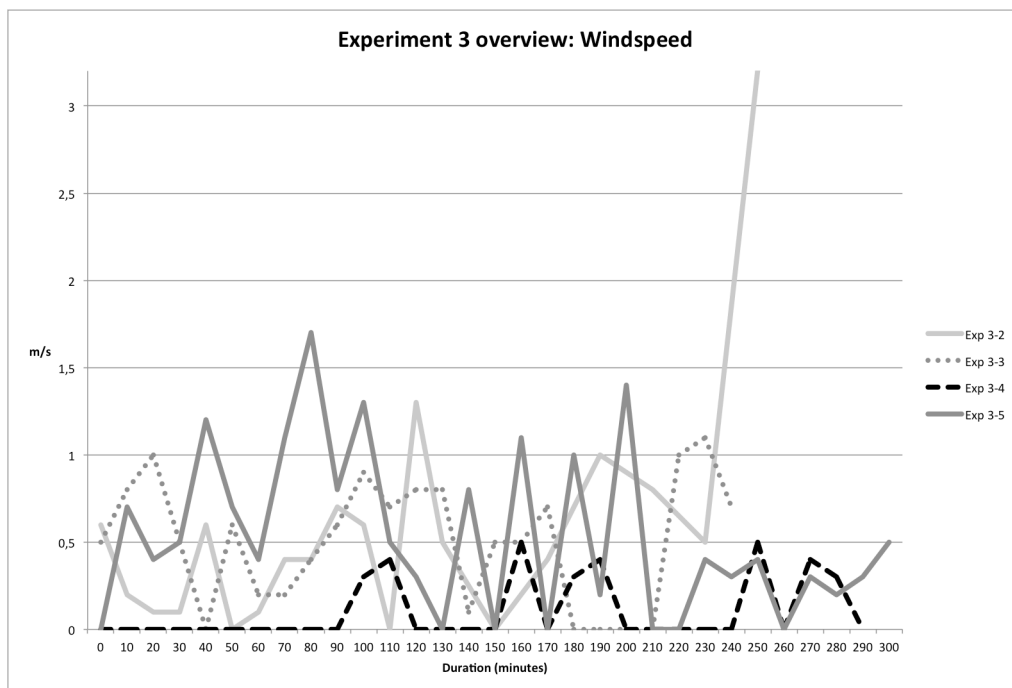


Figure xx: Recorded windspeeds, experiment 3-2 to 3-5, 10 min intervals. Comparison.

## F.2. Experiment 3-2: metrics

### EXPERIMENT 3-2: METRICS

Time	Windspeed	Temperature °C
12:00	0,6	13,2
12:10	0,2	13,7
12:20	0,1	29,7
12:30	0,1	64,2
12:40	0,6	88,0
12:50	0,0	96,7
13:00	0,1	98,0
13:10	0,4	99,2
13:20	0,4	126,0
13:30	0,7	166,0
13:40	0,6	186,5
13:50	0,0	190,8
14:00	1,3	187,1
14:10	0,5	177,0
14:30	0,0	139,2
14:50	0,4	114,2
15:10	1,0	94,9
15:30	0,8	75,4
15:50	0,5	60,3
16:10	3,2	47,3
16:10	1,0	47,3
18:00	-	25,4

Table iv. Experiment 3-2: temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire (20 minutes during die-down). Maximum temperature recorded in pyrolysis chamber was 190,8 °C. However, this temperature is not apparent in the table, as it was observed outside of the intervals.

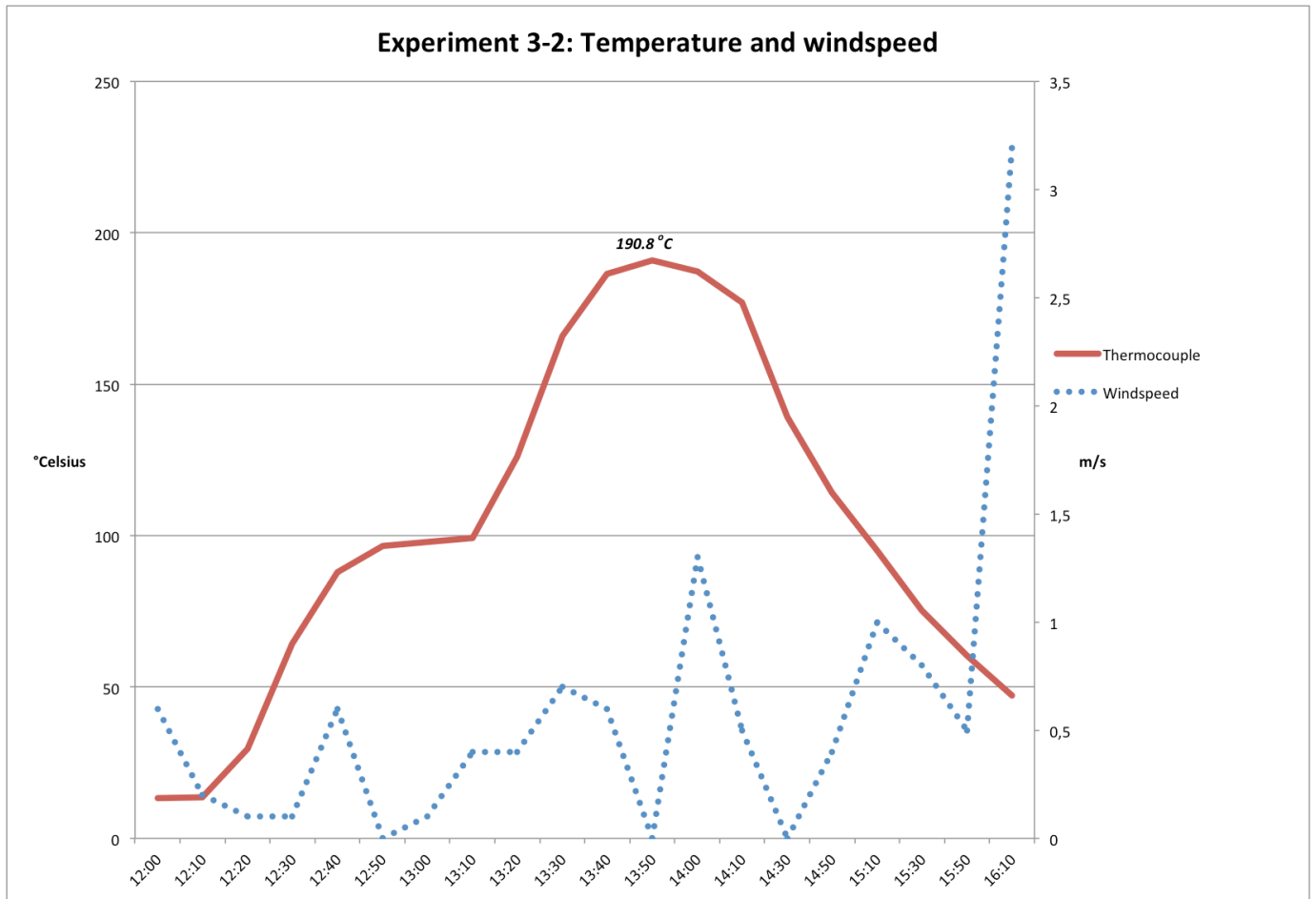


Figure xxi: Experiment 3-2: temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire (20 minutes during die-down). Maximum temperature recorded in pyrolysis chamber was 190,8 °C. However, this temperature is not apparent in the table, as it was observed outside of the intervals.

### F.3. Experiment 3-3a: metrics

#### EXPERIMENT 3-3A: METRICS

Time	Windspeed	Temperature °C
11:15	0,5	16,2
11:25	0,8	23,8
11:35	1,0	84,1
11:45	0,5	96,1
11:55	0,0	112,0
12:05	0,6	157,3
12:15	0,2	177,1
12:25	0,2	179,0
12:35	0,4	201,0
12:45	0,6	223,0
12:55	0,9	217,0
13:05	0,7	205,0
13:15	0,8	209,0
13:25	0,8	213,0
13:35	0,1	222,0
13:45	0,5	216,0
13:55	0,5	228,0
14:05	0,7	259,0
14:15	0,0	286,0
14:25	0,0	299,0
14:35	0,0	298,0
14:45	0,0	295,0
14:55	1,0	297,0
15:05	1,1	308,0
15:15	0,7	320,0

Table v. Experiment 3-3a: temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire. Maximum temperature recorded in pyrolysis chamber was 320 °C.

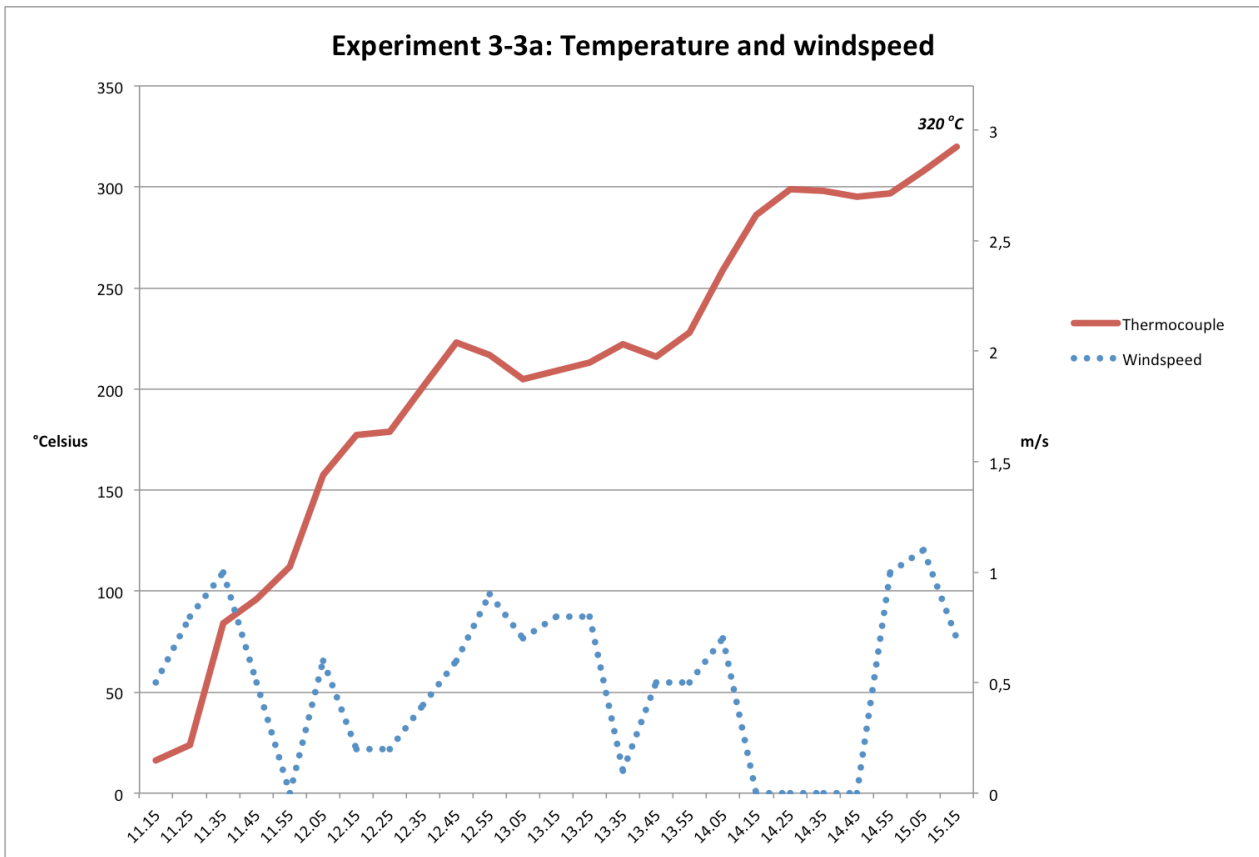


Figure xxii: Experiment 3-3a: temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire. Maximum temperature recorded in pyrolysis chamber was 320 °C.

#### F.4. Experiment 3-4: metrics

##### EXPERIMENT 3-4: METRICS

Time	Windspeed	Temperature °C
12:05	0,0	18,0
12:30	0,0	19,6
12:40	0,0	49,5
12:47	0,0	66,7
12:58	0,0	90,0
13:09	0,0	97,2
13:19	0,0	98,7
13:29	0,0	99,4
13:39	0,0	103,0
13:49	0,0	104,2
14:00	0,3	100,7
14:10	0,4	100,2
14:20	0,0	100,0
14:30	0,0	119,0
14:40	0,0	162,9
14:50	0,0	181,8
15:00	0,5	197,1
15:10	0,0	203,0
15:21	0,3	230,0
15:32	0,4	240,0
15:40	0,0	244,0
15:50	0,0	239,0
16:00	0,0	242,0
16:10	0,0	256,0
16:20	0,0	264,0
16:30	0,5	267,0
16:40	0,0	287,0
16:50	0,4	309,0
17:00	0,3	317,0
17:10	0,0	312,0

Table vi. Experiment 3-4: Temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire. Maximum temperature reached in pyrolysis chamber was 317 °C.

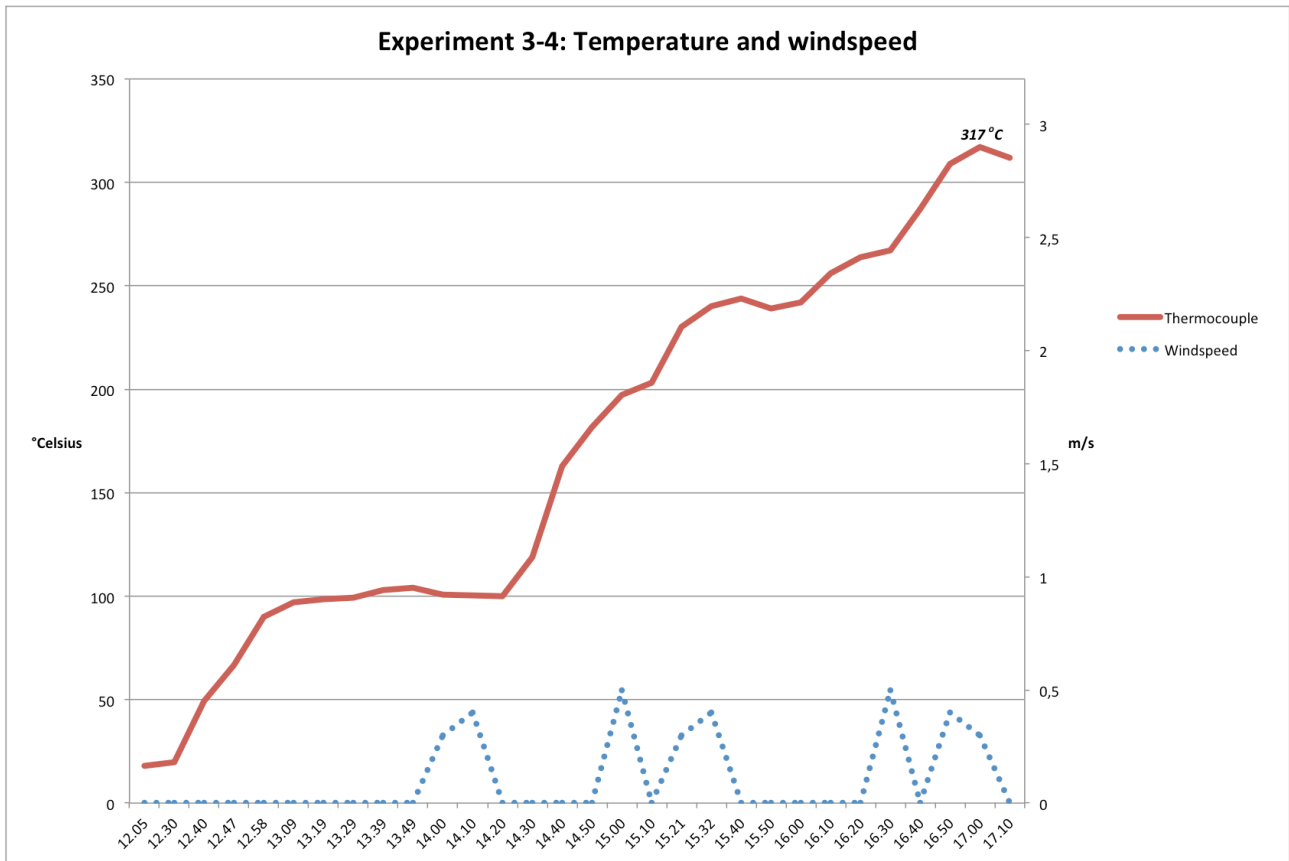


Figure xxiii: Experiment 3-4: Temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire. Maximum temperature reached in pyrolysis chamber was 317 °C.



## F.5. Experiment 3-5b: metrics

### EXPERIMENT 3-5B: METRICS

Time	Windspeed	Temperature °C
11.45	0,0	16,7
11.55	0,7	19,5
12.05	0,4	52,3
12.15	0,5	81,4
12.25	1,2	94,6
12.35	0,7	143,0
12.45	0,4	191,5
12.55	1,1	226,0
13.05	1,7	243,0
13.15	0,8	276,0
13.25	1,3	289,0
13.35	0,5	282,0
13.45	0,3	295,0
13.55	0,0	321,0
14.05	0,8	317,0
14.15	0,0	298,0
14.25	1,1	280,0
14.35	0,0	263,0
14.45	1,0	245,0
14.55	0,2	222,0
15.05	1,4	197,5
15.15	0,0	174,1
15.25	0,0	155,1
15.35	0,4	138,6
15.45	0,3	123,8
15.55	0,4	113,5
16.05	0,0	103,7
16.15	0,3	95,1
16.25	0,2	87,7
16.35	0,3	81,2
16.45	0,5	75,7

Table vii. Experiment 3-5b: Temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire (20 minutes during die-down). Maximum temperature reached in pyrolysis chamber was 321 °C.

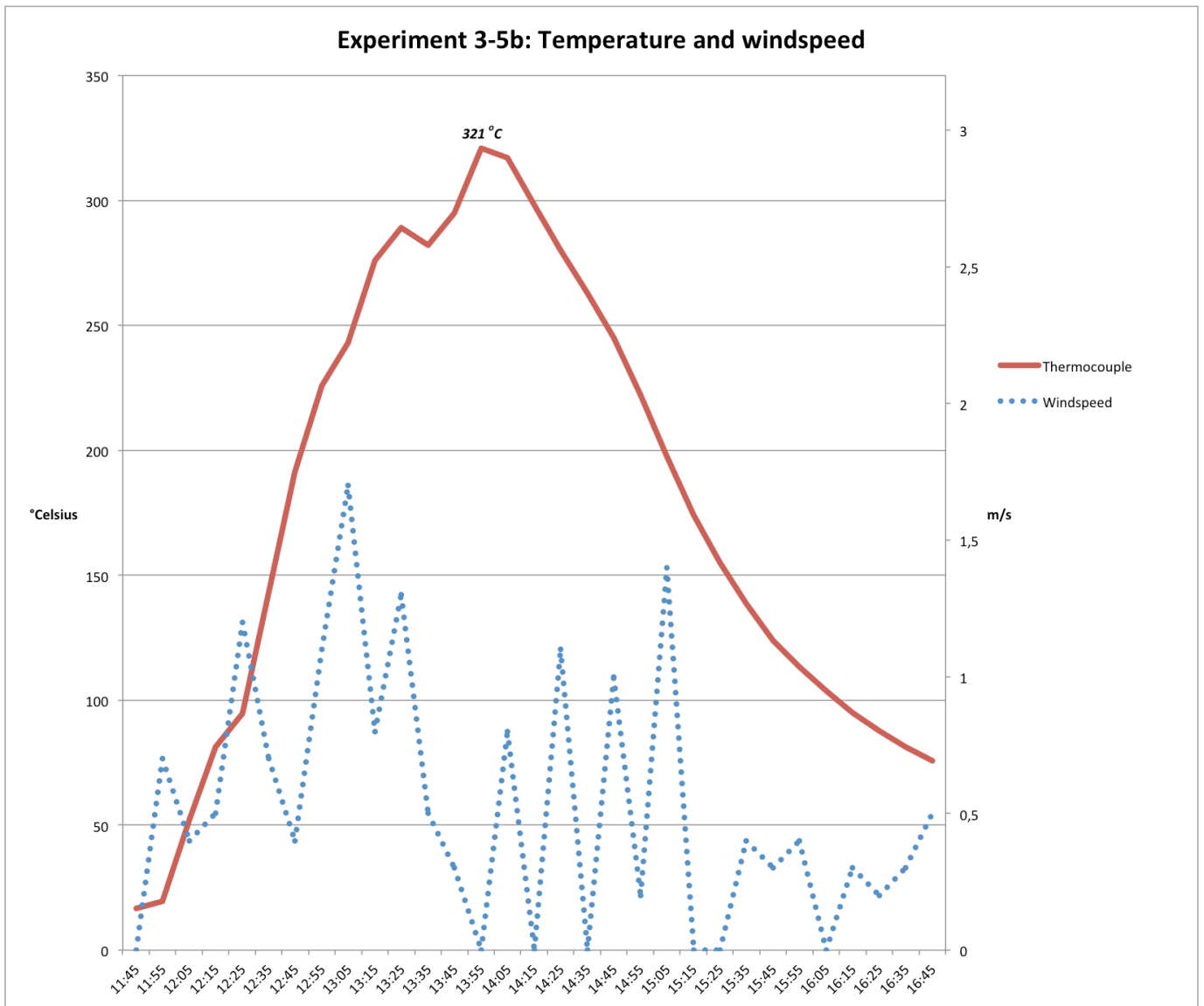


Figure xxiv: Experiment 3-5b: Temperature and windspeed, 10 minute interval measurements on active fuelling of the bonfire (20 minutes during die-down). Maximum temperature reached in pyrolysis chamber was 321 °C.

## **F.6. Birch bark tar experiments: evidence levels before/after experimentation**

Chaîne opératoire/ Object Biography ↓	Scale →	Min												Max											
		Object				Behaviour				Process				System											
Evidence type →		E	T	C	E	T	C	E	T	C	E	T	C												
1. Manufacture	Direct				Direct				Direct				Direct												
	Indirect				Indirect				Indirect				Indirect												
	Associated	■	■	■	Associated				Associated				Associated												
	None				None				None				None												
2. (Inter-)use	Direct				Direct				Direct				Direct												
	Indirect				Indirect				Indirect				Indirect												
	Associated				Associated				Associated				Associated												
	None				None				None				None												
3. Abandonment	Direct				Direct				Direct				Direct												
	Indirect				Indirect				Indirect				Indirect												
	Associated				Associated				Associated				Associated												
	None				None				None				None												

Chaîne opératoire/ Object Biography ↓	Scale →	Min												Max											
		Object				Behaviour				Process				System											
Evidence type →		E	T	C	E	T	C	E	T	C	E	T	C												
1. Manufacture	Direct				Direct				Direct				Direct												
	Indirect				Indirect				Indirect				Indirect												
	Associated	■	■	■	Associated				Associated				Associated												
	None				None				None				None												
2. (Inter-)use	Direct				Direct				Direct				Direct												
	Indirect				Indirect				Indirect				Indirect												
	Associated				Associated				Associated				Associated												
	None				None				None				None												
3. Abandonment	Direct				Direct				Direct				Direct												
	Indirect				Indirect				Indirect				Indirect												
	Associated				Associated				Associated				Associated												
	None				None				None				None												

Figure xxv: Experiments with birch bark tar production: evidence levels of interpretation of the technology: before and after experimentation. The evidence levels, and therefore presumably the likelihood, of the interpretation has increased.



## **Appendix G. Permit for use of illustration**

The drawing of the flint flakes embedded in birch bark tar from Campitello Quarry, in this thesis referred to as Figure 37, is originally found in:

MAZZA, P. P. A., F. MARTINI, B. SALA, M. MAGI, M. P. COLOMBINI, G. GIACHI, F. LANDUCCI, C. LEMORINI, F. MODUGNO, and E. RIBECHINI. 2006. A new Palaeolithic discovery: tar-hafted stone tools in a European Mid-Pleistocene bone-bearing bed. *Journal of Archaeological Science* 33 (9): 1310–1318.

Attached follow the necessary permits, plus e-mail communication between the present candidate and the lead author to gain the necessary permit to re-use this figure.

## G.1. Email communications

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