

Slow Science for Fast Archaeology

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Abstract

This contribution contends that, with the recent genetic revolution, archaeology has reached a new scientism, a development that could lead to fewer opportunities in the epistemology of archaeology to think difference. Drawing from discussions in slow science and the related idea that scientific importance is a matter of concern rather than fact, the contribution proposes that archaeologists start to cultivate methods of deceleration. In particular, as a measure to mitigate the epistemological effects of archaeology's methodological acceleration, the contribution suggests the publishing of personal hunches, failed hypotheses, and so forth in addition to research results, and a cultivation of historical awareness in order to better anticipate possible epistemological effects of pursuing conflicting research interests.

Keywords: anticipation, epistemology of archaeology, history of archaeology, interdisciplinarity, Isabelle Stengers, speculation.

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Prologue: the foundations of fast archaeology

As a modern science, archaeology is marked by nineteenth-century ideals of progress and the accumulation of knowledge. On the one hand, the idea of cumulative knowledge is evident in the sense that, to a great extent, archaeology can be characterized as total history that sees the past as a collection of parts of various size that can ultimately be pieced together for a full picture of the past by careful collecting and sophisticated methodologies. On the other hand, in a more explicitly methodological sense, the history of archaeology can be seen as a series of conceptual and technological advances from the wide-ranging implementation of evolution theory in the course of the nineteenth century to the invention of radiocarbon dating in the twentieth century, and more recently to the increasing interest in matters of provenience and mobility and the use of digital technologies, genetics, and big data towards those ends.

In the Scandinavian context, this development has its roots in the systematization and contextualization initiated by no other than C.J. Thomsen. In reintroducing the three-age system, Thomsen's insight was to emphasize the archaeological context rather than the material of the find. Thomsen felt that 'nothing is more important than to point out that hitherto we have not paid enough attention to what was found together' (Gräslund 1987:23). Because stone and bronze artefacts were more often found together than stone and iron artefacts, and because the relatively younger age of iron artefacts could be established with recourse to written sources, Thomsen inferred that stone artefacts must be older than bronze artefacts.

The implicit evolutionism of Thomsen's contextual-comparative method was further developed in the course of the nineteenth century, most notably by Swedes Oscar Montelius and Hans Hildebrand in their evolutionary typologies. In his *Scientific Archaeology*, Hildebrand (1873:17) argues that if there is one discipline that needs its Darwin, it is comparative archaeology. He then goes on to elaborate on the process by which types emerge, fight for their survival, and die out according to the laws of biological evolution. Later, Montelius (1899:237) put forth his famous adage 'what the species is to the natural scientist, the type is to the archaeologist', elaborating that the task of the archaeologist should no longer be to simply describe and compare the finds and the cultures, but instead, by following the law of evolution, to trace the internal connection which exists between the types, and to show how one type has developed from the other (cf. Montelius 1884). While Thomsen's chronological approach was founded on the idea of context, Montelius and Hildebrand's evolutionary approach came to stress the evolution of the artefact type (Riede 2006).

With the invention of radiocarbon dating in the 1940s, these contextual and evolutionary views became sedimented as the backbone of archaeological chronology as well as the analysis of stylistic variety. Although radiocarbon dating tended to extend the chronology by providing older dates than anticipated, it could more importantly be used to ‘confirm’ the inner sequence of the established relative chronology by reference to the immutable forces of natural laws. Ironically, these laws were the same laws of nature that were thought responsible for giving rise to the studied types in the first place. Importantly, then, archaeological chronology became established as the structure against which evidence could be contextualized and rendered intelligible. More recently, with the introduction of AMS dating, it has become possible to obtain dates from smaller and smaller samples. This has done nothing to the principle of chronology, but it marks a development in which it has become possible to fit smaller and smaller bits of evidence onto the timeline. Similarly, reliance on Bayesian statistics in combining radiocarbon data with prior archaeological evidence has come to provide a further sense of systematicity and objectivity.

Furthermore, although artefact typology in the way imagined by Hildebrand and Montelius is relatively unfashionable today, the underlying concept of evolutionary typology has not gone out of use. When evaluated against this background, it has been relatively straightforward to include and establish genetics as another core scientific method in archaeology. Recent developments in genetics and the possibility to obtain genetic material from ancient samples directly connect with – and even necessitate – the concept of typological evolution (and its abuse): artefact type only has to be replaced with genotype (Müller 2013).

With this brief and selective outline of the history of archaeology’s methodological development, I want to draw attention to some of the fundamental ideas of archaeology as a science. At the core of this history are concepts and methods that have been considered to form the robust core of archaeology’s scientific methodology. Although this type of development is characteristic of all sciences and not only archaeology, it raises a host of worries. The motivation for this article is the particular worry that archaeology has become increasingly fast and that, in the future, archaeology’s methodological development will only accelerate.

One may ask, what is it with acceleration that warrants these worries? Speed is, after all, relative, and it would be foolish to dismiss the advancements of science *in toto*. A certain history of knowledge accumulation, laudable in its own right, is characteristic of the majority of human inquiry from geology to structuralism, and one only has to consider the history of medicine to appreciate technological breakthroughs and the accumulation of knowledge. My worry, then, is more specifically related to the produc-

tion of certain type of knowledge. What I refer to with fast archaeology, specifically, is the increasing tendency in archaeological sciences towards a type of methodological streamlining; a tactic that, through methodological simplification (i.e., increasing emphasis on natural scientific methods, computational methods, statistics, and so forth), could paradoxically lead to a dead end in terms of archaeology's ability to produce knowledge in the broadest sense of the term. My worry is that, as archaeology keeps accelerating, it may become increasingly hard to notice and appreciate difference, that is, the various little manifestations of life which in light of the chosen methodologies might be dismissed as irrelevant.

This is by no means a novel observation or an original critique. The issue was not only the subject of the 'science wars', but probably one of the most common topics that have motivated discussions in philosophy of science. As pertains to archaeology in particular, and in returning to our historical examples above, critique against the positivism inherent to Darwinism, for instance, was already put forward in the nineteenth century. Montelius' reliance on the systematic use of evolution theory was harshly criticized by his contemporary, Sophus Müller (1884) who contended that evolutionary typology would lead to a situation where the archaeological evidence is used to verify the law of stylistic determination that was supposed to explain the observed peculiarities in the evidence in the first place. Müller then argued that typological analysis always depends on preconceived ideas regarding, for instance, the find circumstances. In other words, Müller was concerned about the role of the human individual that produced the artefacts, and he strongly believed in human free will rather than covering explanations (Müller 1884:188).

Arguments similar to Müller's were also put forward by Finnish archaeologist A.M. Tallgren. In a famous article originally published in Finnish, Tallgren (1934, 1937) admits that the introduction of scientific methods in the humanities has introduced in the latter a 'sense' of exactness characteristic of the former. Tallgren (1937:154–155) is worried that the positivism of Darwinism will not lead to more exact forms of representation, but instead to a methodological dead end:

At any rate it appears that archaeology, in spite of its remarkable achievements, has gotten into a cul-de-sac. [...] The whole subject consists merely of a comparison of forms and of systematization. [...] Brilliant systematization, regarded as exact, has not led and does not lead to an elucidation of the organic structure of the whole life of the period studied, to an understanding of social systems, of economic and social history, to the history of religious ideas. In short, forms and types, that is, products, have been regarded as more real and alive than the society which created them and whose needs determined these manifestations of life.

The worry expressed by Tallgren in his paper is that the uniformitarian idea of development will lead to the eradication of difference in the past. Tallgren, writing in the 1930s, was mostly concerned about the political uses of the past and the use of positivism for advancing totalitarian political agendas. However, his critique was also targeted against the kind of cultural essentialism that equated archaeological evidence with particular ethnic groups, an idea that was put forward by Finnish archaeologist J.R. Aspelin (1875) and which then became the leading idea behind much of Finnish migration research until at least the 1950s (cf. Nordman 1915; Meinander 1954). In his criticism, Tallgren, who had also studied in Sweden under Montelius, aimed to distance his position from that of both cultural essentialism and evolutionist typology. Some have suggested that, due to his criticism of cultural essentialism, Tallgren could be seen as an early advocator of processualism (Binford 1968:6–7). Others have noticed that his ideas are similar to those advanced as part of postprocessualism's critique of positivism and Darwinism and that Tallgren's ideas could be considered precursory to post-processualism (Immonen & Taavitsainen 2011:148; Marila 2018:25). However, I suggest that Tallgren's worries reflect and represent a more fundamental opposition between natural sciences and humanities which, regardless of continuing effort, persists today, partly because of archaeology's methodological streamlining.

What I suggest in this article, then, is that, in countering the methodological streamlining characteristic of fast archaeology, and in aiming to notice difference, particular measures are needed to decelerate the processes of archaeological knowledge production. Towards this end, I make use of the concept of slow science and, in keeping with the topic of this thematic issue of *Current Swedish Archaeology*, I discuss the possibility of both methodological and theoretical deceleration in the context of ancient DNA research. More specifically, I will discuss the epistemological challenges introduced by increasing reliance on genetics in the study of population movements, in particular the reductive and simplifying effects that the centralization of a methodology, in this case genetics, can have on the epistemology of archaeology. This is not to say that genetics or archaeology alone should be blamed for creating these challenges and, keeping this in mind, I will also touch on issues of interdisciplinarity.

The genetics revolution

If Darwinism and radiocarbon dating provided revolutionary degrees of exactness in archaeology in the past, in recent years the sense of scientific progress has been attributed to advances in isotope analysis, the use of big

data, and the application of genetics (Kristiansen 2011, 2014). Although genetics undoubtedly provide new kinds of evidence that can lead to discoveries previously unimagined, its effects have also been felt in terms of epistemological oscillation and disciplinary uneasiness (cf. Pluciennik 2006; Lidén & Eriksson 2013; Heyd 2017; Ion 2017; Sørensen 2017 with comments; Nilsson Stutz 2018). As pertains to the epistemological effects of archaeology's increasing reliance on genetics, Kristiansen and his team of scientists (Kristiansen et al. 2017:335) have declared that the new scientific methodology finally lifts the 'interpretative burden from archaeology', and that the new freedom can be invested in 'properly theorising and interpreting' local processes of migration. Kristiansen and his associates leave the meaning of the terms 'interpretative burden' and 'proper interpretation' undefined, but the rhetoric raises suspicions of the possibility of a return to methodological monism *à la* positivism. In this setting, positivism entails that, in search for answers to questions on local processes of migration, the methods of genetics are considered superior to those used in almost 200 years of comparative-contextual stylistic analysis on thousands of artefacts. In other words, genetics are used to verify or falsify the old results of migration studies or provenience analysis. This verification – ironically – is more often than not based on very scarce data, that is, dozens as opposed to thousands, although this pertains more intimately to isotope studies than it does to genetics, as has been convincingly discussed by Sørensen (2017). Clearly the promise of exactitude comes from faith in the rapidly improving methodology rather than from the quality or diversity of analysed evidence (Vander Linden 2018).

As result of this methodological streamlining, past migration tends to be reduced, if only implicitly, to a flow of genetic markers indicated by arrows on a map, rather than cultivated as a highly complex combination of variables and archaeological evidence (Cassel 2000; Hakenbeck 2008; Johannsen et al. 2017; Furholt 2018). The most wide-reaching generalizations on population history and migration are often made by geneticists rather than archaeologists, and the shortcomings of the research usually relate to poor integration of archaeological materials. In this respect, it should be noticed that critique targeted against the straight-forward utilization of genetics in reconstructing past population movements does not only come from concerned humanists, but those archaeologists well versed in genetics and other natural scientific methods (Lidén & Eriksson 2013; Heyd 2017; Johannsen et al. 2017). At times, this has resulted in research that cuts some ethical corners in the race for the acquisition of bone samples (Morris 2017), but nevertheless enjoys a high degree of scientific impact. This, however, is not to say that archaeologists and geneticists should not be held equally responsible in having created the situation. Having one's name included in

the list of authors of a *Nature* article is too easy in proportion to the disciplinary impact and return on investment brought by the publication. One clear downside to joint publishing of this type is that it becomes increasingly hard for any one author to fully grasp all of the processes and lines of reasoning that led to the conclusions presented in the paper.

A case in point on the sociological intricacies and epistemological pitfalls of rushing to publish the findings of aDNA research is the human history of the Pacific Islands. Using genetic evidence, two recent key articles on the topic, Lipson et al. (2018) and Posth et al. (2018), both reach similar conclusions regarding the initial settlement of Near and Remote Oceania, but the evidence used in arriving at the conclusion differs considerably. Lipson et al. (2018) base their analysis solely on genetic material. They suggest that a ‘second wave’ of migrants ‘of almost entirely Papuan ancestry arrived in Vanuatu by around 2300 BP’, and that ‘this second wave forms the primary ancestry of people in Vanuatu today’. The number of skeletal samples analysed in support of this conclusion was a total of one.

Posth et al. (2018), on the other hand, seek to reflect their aDNA results (scarce as they, too, may be) against linguistic and archaeological data, which does not support a wave-like influx of new people in 2300 BP (see discussion in Bedford et al. 2018). Posth et al. (2018) then reach the conclusion that, rather than having been a single event, the population admixture took place as an extended, incremental, and complex process. In other words, while both acknowledge a degree of Papuan ancestry in present-day Near and Remote Oceanian populations, the two approaches provide very different accounts of the processes that led to the situation. While one – and this is the main takeaway of my example – aims to reflect the complexity of the past, the other ends up producing a more simplistic narrative. Importantly, then, the degree of complexity and incrementality that a narrative manages to indicate undoubtedly also has relevance to how the narrative is perceived by present-day local populations and how that narrative shapes their historical and cultural self-identity.

If rushing to publish simplifies the past by obscuring the lines of reasoning behind conclusions, equally hard is the successful anticipation of potentially destructive uses of these narratives by the wider public. When combined with ideas of origin and authenticity, aDNA research directly connects with decades of research tradition in European archaeology (Casel 2000; Niklasson 2014:60; Klejn 2017). Obvious risks in this development include the validation of various political or ideological objectives, mistakes that, undoubtedly, should be left to the history of the discipline rather than repeated by state-of-the-art research (Müller 2013; Heyd 2017; Hakenbeck 2019). In general, then, aDNA research is only feasible in a way that acknowledges the history of archaeology’s disciplinary anxieties, stays

sensitive to its epistemological oscillations, and aims to anticipate the afterlives of scientific conclusions.

If genetics has initiated a return to positivism, a pressing question becomes how to avoid adopting an epistemology that assumes the superior explanatory power of some methods while rendering others unreliable unless corroborated by the results of the superior one, regardless of the amount and quality of the supporting facts. To me, this question is directly related to archaeology's interdisciplinarity, and it is therefore also reasonable to ask what the new positivism does to research that falls outside of the narrow definition of the scientific. Obvious risks include the uneven distribution of research funding, as has been convincingly discussed elsewhere by, for example, González-Ruibal (2014), but I want to steer attention to the epistemological fault lines between sciences and humanities.

While the genetics revolution presents itself as a possibility for an increased mode of interdisciplinary collaboration between the sciences and the humanities (at least to those who choose to pursue a career in archaeological sciences), many have challenged genetics as the hard and fast solution to the problem of interdisciplinarity. In attempting to bridge the conceptual gap between sciences and humanities, Alexandra Ion (2017), for example, argues that, contrary to having provided a means to proper theorising or proper interpretation, genetics necessitates a rethinking of the connecting medium between the sciences and the humanities. Ion then proposes that historical narrative could provide the connecting medium in which the results of genetics are rendered archaeologically meaningful. Ion therefore sees archaeology as a form of storytelling in which the role of genetics is to provide parts of the story rather than concluding facts (cf. Currie & Sterelny 2017).

In a somewhat similar vein, Sørensen (2017) has argued that the increasing reliance on not just genetics but natural scientific methods in general tends to render the methods and results of the humanities unreliable unless scientifically corroborated. Sørensen then proposes that, instead of seeing humanities as a provider of testable hypotheses, the two sub-disciplines' conclusions should be seen as equally 'speculative'. In this way speculation, for Sørensen, is the context through which seemingly disparate realms of evidence can be made to make sense. Sørensen then concludes that, instead of providing answers, the results of archaeological science only unleash further questions (Sørensen 2017:10). The insight embedded in this notion is that archaeological science too contains an element of speculation that is impervious to the verificationism commonly adopted in positivist archaeology.

Recently, Nilsson Stutz (2018) has voiced a concern for the future of interdisciplinarity in archaeology. Nilsson Stutz (2018:52) argues that the recent development that has been presented in the form of a scientific revolution

risks a biased neoliberal conceptualization of interdisciplinarity where the interdisciplinary consists of hypothesis-driven science, or a set of (natural) scientific practices carried out by highly competent researchers who provide clearly ordered scientific knowledge. Nilsson Stutz (2018:52) then proposes that in order to avoid a situation where perspectives that unnecessarily complicate the past are being suppressed, archaeology should more systematically include perspectives from the sciences and the humanities. For Nilsson Stutz (2018), this is especially important when questions of public and collaborative archaeology and multivocality are considered (cf. Nicholas & Markey 2015). Nilsson Stutz (2018:53) then writes that if we approach multivocality not as a political struggle, but as ‘transdisciplinary collaboration’, we can free our political leanings from the factors that influence our disciplinary identities and strategies. Nilsson Stutz contends that archaeology’s relevance in transdisciplinary collaboration is that it provides a way of knowing the world. Importantly, Nilsson Stutz’s world is not the object-world of positivism, but rather a plural concept. The importance of this notion will become clear as I turn to discussing the principles of the philosophy behind slow science.

Slow science

In science in general, the rapid methodological development in the course of the twentieth and twenty-first centuries has sparked a distinct counter-movement that goes by the name of slow science. Slow science, as characterized by Cunningham and MacEachern (2016:633) is marked by a consideration of the ethical consequences and human relations in research, recognition of collaborative and communal aspects of learning and research, and critique of the adoption of practices and concepts from business and management into research and its evaluation. Unsurprisingly, the slow science movement has found supporters in archaeology as well. One commonly shared concern within the slow archaeology movement is that methodological development has had an alienating effect. For many, then, slowing down is a way of regaining an appreciation for the humanity and the craft of archaeology.

Caraher (2013), for instance, has voiced a concern for the preservation of the craft of archaeology in the age of increasing digitalization. Caraher contends that the digitalization of archaeology has resulted in the fragmentation of research practices and a weakened state of mutual understanding of the research process among involved parties. More importantly, Caraher contends that digitalization has resulted in the fragmentation of knowledge about the very object of research. Slow technologies, that is

technologies that are inefficient in contrast to fast technologies, for Caraher, would mark a state of a heightened appreciation of those phenomena that routinely go unnoticed in the digital era. Caraher argues that the use of handwritten notes in field documentation, for instance, would provide a stronger link between our bodies and our understanding of the past and the past individuals. This, Caraher contends, would help researchers to recognize their role in the process of interpretation and help make the discipline more humane, more open, and most importantly more inclusive and ethically sustainable. These are all concerns that have to be carefully considered in the study of ancient DNA (Fossheim 2017; Prendergast & Sawchuk 2018; Tringham 2018).

Cunningham and MacEachern (2016) on the other hand provide an ethnographic perspective on slow archaeology. They argue that ethnography can provide a way to resist the streamlining effect of the natural scientific revolution. More specifically, Cunningham and MacEachern contend that ethnoarchaeology could be a form of slow archaeology if it is not understood as limited to the construction of middle-range theories or modern analogies, but instead as a way of developing an understanding of modern cultural variety. This realization, then, could be used in archaeological contexts as a way of intensifying the sense of cultural variety in the past (Cunningham & MacEachern 2016:635; cf. Zubrow 1989). What connects these takes on slow archaeology is the concern that, through methodological streamlining (it is now possible to do in a day tasks that took years in the past), archaeology will end up in a position where only evidence that is relevant for the concerns of fast methods is noticed or considered important.

What is useful to notice in this context is that, through its streamlining tactics, fast science aims towards a separation between scientific practices and scientific results. In addition to the alienating effect of methodological streamlining, one can identify something as simple as the structure of the scientific article as a mechanism of separation. Although the research article has become the major forum for reporting scientific results, its form – which often begins with the presentation of materials and methods and proceeds to the reporting of results and a discussion of their importance – tends to give a distorted image of the research process. Peter Medawar (1991) has argued that the form of the scientific paper gravely misrepresents the thought processes that gave rise to the results reported in it. Medawar's discontent with the scientific paper is based on the idea that science as discovery of new ideas is, contrary to how the scientific paper represents it, not inductive. For Medawar, induction implies that research begins with an untarnished mind and proceeds with the collection of data and inference of a new idea, whereas in reality the new idea often precedes the identification and collection of the evidence. Medawar then points out that rather

than relying on a schema of demonstration, discovery starts with a sense of importance or relevance; that a particular hypothesis should be investigated further (cf. Hanson 1958; McKaughan 2008; Nyrup 2015; Chiffi & Pietarinen 2019).

Medawar's suggestion is that the inductive form of the scientific paper should be reversed: the discussion, which is usually placed at the end of the paper, should come first, and the hypotheses, which the paper misrepresents as conclusions, should be presented at the beginning as hunches or ideas. These could then be followed by 'the scientific facts and scientific acts', that is, the presentation of materials and methods. In other words, Medawar argues that scientist should not be ashamed to admit that 'hypotheses appear in their minds along uncharted byways of thought; that they are imaginative and inspirational in character; that they are indeed adventures of the mind' (Medawar 1991:233).

Medawar's criticism only pertains to one type of scientific paper and he is exaggerating the problem's extent to a certain degree. Nevertheless, his worries highlight the fact that only hypotheses that are deemed successful by the researcher (or ideas that are considered sufficiently cooked by their peers) make it into the publication. By omitting factors like guesswork, failure, or personal audacity, scientific writing often misrepresents the processes that created the conditions of demonstration to begin with (Snyder 2005; Boozer 2015; Lahiri 2017; Lucas 2019). Importantly, then, it is unusual to report in articles the hunches and intuitions that led to discovery, not to mention the anxieties caused by entertaining hypotheses that turned out to be unsuccessful. Against this background, slowing down would denote a hope of developing ways to appreciate and conserve rather than eliminate or blackbox not only the complexities of the past but also the vague and messy social and intellectual processes that led to the discovery of an archaeological idea (Latour 1987; Law 2004; Gero 2007; Caraher 2016, 2019; Sørensen 2016, 2019; Marila 2017; Stengers 2018). The above-discussed example on two uses of evidence in aDNA research serves as a case in point also in this respect.

In addition to these considerations on the distorting effect of scientific publishing, one should keep in mind that as more and more emphasis in archaeology is put on publishing in high profile scientific journals, and because publications in these journals weigh more than publications in those of the humanities in the evaluation of research output, the tendency will ultimately result in lesser degrees of impact for those who publish in the humanities (Lidén 2017). Lidén and Eriksson (2013) note that the above discussed tendencies have only resulted in the widening of the gap between the sciences and the humanities. As a solution to the problem, Lidén and Eriksson (2013) propose that what is needed for better collaboration be-

tween the two fields is a common language, and that the development of that language hinges on the realization that the two fields are connected by common research problems rather than separated in terms of methodology (cf. Lidén 2017; Tringham 2018).

However, I am afraid the idea of a common problem will only reify the original problem: that our research methods are considered connected and comparable through a reality consisting of an ‘external permanence’. Recently, Alfredo González-Ruibal (2018) has noted that not even the Anthropocene, regardless of the global, ubiquitous, and destructive nature of the matter, can be considered a common problem. While the scale and magnitude of the problem transcend its historical causes, and although the problem has a deep and partly common anthropogenic aetiology, the problem nevertheless affects people who cannot be held responsible for creating it. Although the common problem approach is driven by visions of disciplinary unification, it is also essential to notice that the identification of archaeological facts does not hinge on the recognition of common problems, but on the interests of particular researchers or research communities. This also entails that facts that are considered important are only so in respect to particular interests. The real is not simply a matter of common facts, but a matter of disparate concerns (Latour 2004; Witmore 2015).

In her *Cosmopolitics* series, Isabelle Stengers (2010, 2011) provides a convincing analysis of facts versus concerns. Instead of referring to reality as a set of facts, Stengers (2010) uses the term ‘ecology of practices’ to describe how facts are pursued as matters of concern. In this model, ecology is not the science of function within a closed system, or the science of cause and effect, but a ‘science of multiplicities, disparate causalities, and unintentional creations of meaning’ (Stengers 2010:34). Stengers mainly discusses the notion of ecology of practices in the context of modern science. She contends that the *modus operandi* of modern science has been an invalidation of those practices that do not conform to the predominant epistemology’s notion of the real and that, by upholding an image of the scientific method as *the* measure of the real, modern science has gained a monopoly over the imagined external permanence, partly due to convincing rhetoric and the promise of settlement of opinion.

Stengers (2018) suggests that, in order to ‘civilize modern practices’, that is to make them critical of the concepts they admit uncritically (for example the scientific method), modern science should be seen as an ecology of practices motivated by important concerns rather than important facts. In this sense ‘ecology of practices’ is also a philosophy that remains responsible in respect to the fact that science is practised from a variety of perspectives and matters of concern which the postulation of a common problem might end up suppressing, and that the creation of the real is a matter of

assigning importance to the world. Stengers contends that civilized scientists have to cultivate ‘a capacity to participate in the collective assessment of the consequences of an innovation, rather than a decision based on values’ (Stengers 2018:103). In other words, civilized science denotes an ethical sensitivity to the effects of investigation itself because the practices and concerns of one epistemology can produce effects that can hinder or negate those of another.

It should be noted that Stengers’ proposal to civilize modern practices is not an ‘anything goes’ call for abandoning scientific methodology, nor a recommendation to think unsystematically. Nor is civilising the modernist act of universalization and education in order to civilize those who remain unaware of the modern method of scientific evaluation (cf. Cunningham & MacEachern 2016). Civilization rather entails replacing the demand of modern practices for universality and reduction with the systematic consideration of possibility as more than subordinate to plausibility or probability. What civilization means for Stengers (2018:120) is rather a process of healing; a reclaiming of that which is too often abandoned as being too messy or too demanding. This healing is driven by a constant centralization of the ‘unknown’ (Stengers 2010, 2011); a cultivation of the idea that things could be differently.

Slow science for fast archaeology

With Stengers’ view of science as an ecology of practices, each characterized and driven by their particular axiological notions of importance or significance, I want to return to the topic of slow science and the possibility of deceleration in the context of archaeology more specifically. The sense in which I want to call for a deceleration of science is not an attack on technology, scientific methodology, or interdisciplinarity, but rather a call for the type of civilized science outlined by Stengers (2010, 2011). As a gesture of inclusion, a slow archaeology should aim towards methodological deceleration for the sake of preservation of the multiplicity of matters of concern.

What practical possibilities do we have, then, to decelerate archaeology? How can we start to promote a heightened sense of responsibility and inclusivity in interdisciplinary situations? I propose two overlapping avenues. The first one is a direct response to the above-discussed calls for slow archaeology, that is, the deceleration of our scientific practices in order to reveal those intellectual, technological, and social processes that become concealed through the types of methodological streamlining highlighted in this contribution. What this could mean in the case of the scientific paper, for instance, is the systematic reporting of the initial hunches, failed

hypotheses, and intellectual dead ends that were entertained in the course of the research process, but which nevertheless usually remain unreported to our fellow researchers, at least in writing. Whereas the scientific article can be provided with supplementary material and detailed descriptions of the data or the materials studied, why not provide in the supplementary material an inventory of those hypotheses that did not make it into the results chapter of the article?

The reporting of disciplinary anxieties caused by speculation and uncertainty does not have to end at the scientific article. The same tactics could be adopted when arranging scientific meetings, where the scientific presentation is too often structured with the intent of driving a point home, in “‘bullets’, no less’ (Stengers 2018:122). As an alternative, we could start organising what Stengers (2018:124) refers to as slow meetings, meetings ‘not reduced to the ritual of attending a prepared lecture that ends with a few banal questions’. The primary purpose of organising a slow meeting would be to resist the sophistic rhetoric characteristic of today’s consultancy economy and make visible to others (rather than obscure in order to convince) the personal aspects of our research. In this way we could convey also to the wider audience that the research process does not follow a clearly defined protocol or a predetermined logical schema.

Ultimately, the intensification of the ambiguities, insecurities, and uncertainties of research would increase mutual understanding and lead to more efficient communication within and between different disciplines and epistemologies (Gero 2007). This, in turn, should strengthen the idea that the progress of science does not hinge on methodological acceleration and innovation, but on the researcher’s ability to think otherwise in as many ways as possible. Fittingly, Anna Tsing (2015:17–25) refers to this mode of inquiry as an ‘art of noticing’; a way to look around in order to detect the ‘polyphonous assemblages’ of the world rather than a method of looking ahead in the name of progress. In other words, it is better to postpone innovation than it is to solve the problem at hand for the sake of closure or general advancement of a unified agenda.

In addition to publications and talks, we can expand the issue of acceleration to pertain to the whole history of methodological and theoretical innovation in archaeology. Contrary to what I proposed in the beginning of the article, acceleration is not only characteristic of the history of archaeological sciences, but plagues the humanities as well. Archaeological theorising in particular is marked by the reinvention and borrowing of theory from other disciplines and philosophical schools with the intention of providing methods or ideas that are better, more efficient or more robust than the old ones (Bintliff & Pearce eds 2011; Lucas 2015; Ribeiro 2016; Pétursdóttir & Olsen 2018; Sørensen 2019). Often, however, counterpro-

ductive to their objectives, theoretical turns are motivated by innovation for the sake of novelty rather than innovation for the sake of increased understanding of the subject matter. In archaeological theorising, structuralist and poststructuralist semiotic theories, for instance, were deemed flawed or lacking in the wake of new materialisms, and positivism suffered a similar fate with the introduction of poststructuralism. Before that, cultural idealism was banned with the introduction of positivism. Ironically, with the increasing development of scientific practices, genetics in particular, archaeology is facing a possible return to the essentialism of culture-historical archaeology, only disguised in a form of a new positivism or a new scientism (Witmore 2015; Sørensen 2016, 2017; Fosshem 2017; Marila 2017). In this context the purpose of slowing down is to reconsider the effects of theoretical acceleration and its tendency toward simplification for the sake of novelty. What archaeological theorising then needs is a sense of hesitation in adopting the latest theoretical trends. How about staying with positivism, or staying with the structuralist theories of signification? Not for the sake of resisting the latest trends (just like new scientific methods, new theories have to be appreciated for what can become of them), but in order to subject these intellectual traditions, old and new, to a process of healing. Only then we may be able to separate their creative contents and contributions from their destructive effects.

The second sense in which I want to propose a deceleration of archaeology is directly connected to the methodological and theoretical considerations discussed above, but it also connects more intimately with questions of interdisciplinarity and the possibility of developing common understanding. Even more important than arranging slow meetings or reflecting on our personal anxieties in scientific publications is the cultivation of historical awareness. In other words, it is not enough to work towards a common language defined by a common problem, but instead towards an understanding of the disparate histories of the practices and disciplines involved. Whereas the common language approach is aimed at the identification and eradication of the common problem, the historical awareness approach would aim for the exposition of the reasons that led to the identification of that problem in the first place: to whom and why is the common problem a mutual concern? The historical awareness approach, then, would aspire not only to establish a common language, but also to understand that which remains unsaid in collaborative and interdisciplinary situations. The reason that archaeologists routinely criticize geneticists for making overarching generalizations is not because of what geneticists report in their publications, but because geneticists do not address the interests and insecurities felt by archaeologists in the past. This is to say that the practices and values of a discipline are historically meaningful and relevant. The concerns

or goals of research to a large extent depend on the nature of previous research, its subject matter, as well as its methods and the results those have yielded (including unachieved objectives). In order to better understand the concerns of researchers in neighbouring disciplines one needs an intimate understanding of the histories of those particular disciplines (Preucel & Mrozowski 2010; Witmore & Shanks 2013; Piironen 2018).

For archaeologists, this would mean that they first develop an understanding of the anxieties or matters of uncertainty that were felt and entertained in the course of the history of their own discipline. What was the prevailing consensus at a given point in time, or what were the alternatives to the predominant disciplinary position? Why was or wasn't a theory or a philosophy abandoned and replaced with another? For instance, I argued in a recent article (Marila 2018) that the relative slowness of Finnish archaeology in reacting to the New Archaeology in the 1970s was not result of a lack of theoretical readiness, but a deliberate theoretical decision to avoid an epistemology that might end up simplifying the subject matter. In particular, the article showed that those archaeologists whom the historiography of Finnish archaeology has tended to label as empiricists or positivists, C.F. Meinander in particular, were in reality concerned about preserving the multiplicity of interpretation as a research possibility rather than as an epistemological shortcoming or a source of unnecessary speculation. The article aimed to highlight that a more detailed study of the history of the epistemology of archaeology could be one solution to the problem of conflicting research objectives. In other words, we tend to think that past scholars adhered to what now seems like an outdated research position only because that position was replaced with a better one for reasons obvious to us.

As to the geneticists' understanding of the history of their discipline, I am afraid I have less to offer. Although aDNA research is considerably younger than archaeology, historiographies of the discipline have already been attempted (Der Sarkissian et al. 2015; Hagelberg et al. 2015). However, perhaps due to the adolescence of aDNA research, its histories appear as technical reviews of the merits, challenges, and potential applications of the method itself rather than as expositions of the (possibly conflicting) concerns of individual researchers or research groups (although this is an obvious point of interest), not to mention possible solutions to the problem of (possibly) incommensurable research objectives. What this could entail for the relationship between archaeology and aDNA research is a possibility of writing a common history of the disciplines or a history of interdisciplinarity that includes both the concerns of the geneticist and those of the archaeologists. In this sense, the writing of a common history could present itself as a possibility for slowing down for a deeper mutual histori-

cal understanding. Martin Savransky (2012:365, emphasis original) nicely recapitulates the main points in this challenge:

It is a challenge that requires the remaking of existing practices and the invention of new ones in order to transform the milieus that they sustain and induce an *experience of hesitation* that may create the space for the crafting of a problematic togetherness of entities and relations, but also of solitudes, of dreams and hopes. A form of togetherness that can never be stabilized, but which constitutes a risk, and a possibility, for a practical and always partial construction of common worlds.

Epilogue: Interdisciplinarity is a matter of future anticipation

Savransky's (2012) notion of the possibility of a practical and always partial construction of common worlds highlights that gaining a historical understanding is not only a matter of developing understanding or appreciation between disciplines for the sake of efficiency in eradicating the common problem. Ultimately, the development of mutual understanding and appreciation should not be driven by the idea of value-free research connected by commonly shared concerns. If research aims to be inclusive, responsible, and sensitive to the idea that the common world is always partial and partially constructed through scientific practices, it should be driven by an anticipation of those practical effects that adhering to – and acting upon – a particular understanding of the meaning of the terms truth and reality have on those practices that share a different understanding of the practical meaning of those concepts.

This of course raises the question of how it is possible practically to evaluate and compare the *possible* effects of engaging in inquiry and pursuing a hypothesis (cf. Saitta 1983). Importantly, this question necessitates a thinking of archaeology not only as a matter of past demonstration, but also as a matter of future anticipation. In this context, historical understanding too is not only a matter of historical knowledge *per se*, but of anticipation, or as Witmore and Shanks (2013:386) describe the situation in the context of archaeology's historical self-understanding: 'a common image of archaeology was never behind us. On the contrary, a common image of archaeology has yet to be formulated.' This formulation of the identity of archaeology calls for an understanding of unity as a function of both actual and anticipated diversity rather than universality (Witmore & Shanks 2013:387).

If we are to grant anticipation as much importance as we have to demonstration, we also have to grant equal importance to the various forms

of speculative thinking (e.g. Högberg et al. 2017; Ståhl et al. 2017; Reilly 2019; Marila, forthcoming). In this context, speculation has importance beyond its role in hypothesis-testing, and speculation becomes a way to intensify the multiplicity of alternative ways of appreciating the world (Debaise 2017). As I have argued in this contribution, this view of speculation puts due importance on methodological deceleration and, by the same token, on cultivating a sense of hesitation in scientific decision-making. This hesitation is targeted at the consideration of the very effects of pursuing a particular conceptualization of truth and reality. In practical terms, hesitation means that we aim systematically to anticipate the possible practical effects that our pursuing a particular truth would have in the world and therefore forestall the making of a possibly destructive decision.

In other words, anticipation and hesitation are connected modes of appreciation and care, both of which refer to a possible future. The connecting medium or common ground that makes it possible to compare not only two or more hypotheses (however vague the evaluation criteria may be), but also the possible effects of pursuing a truth on those whose interests differ from ours is not of the nature of external permanence. On the contrary, the connecting medium makes itself visible as a form of anticipation of action and as a careful consideration of the various intentions involved; a perpetually self-renewing possibility for felt experience.

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