The Representation of Interpretative Knowledge in Cultural Heritage Research

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A thesis submitted in partial fulfilment of the requirements of the University of Brighton for the degree of Doctor of Philosophy

March 2023

Abstract

This thesis is about how knowledge is made and shared in cultural heritage research. It explores the use of digital tools and media for knowledge making and the challenges and opportunities which these bring to the research ideals of transparency, reflexivity and multivocality. Working from within a constructivist paradigm, the thesis asks whether cultural heritage research projects can share their outputs in modes which properly reflect the interpretative aspects of their findings and the situated nature of their authors' knowledge, and considers whether the affordances of digital tools make them compatible with such perspectives. Archaeological research is taken as the object of study because of its ties to both scientific disciplines and the humanities, and its need to deal with the uncertainties caused by incomplete evidence.

Research practice is analysed via semi-structured interviews with archaeology professionals and the observation of a documentation project over a period of eight months, using theoretical perspectives including Latour's Actor Network Theory and Star's accounts of categorisation and boundary objects. Particular attention is paid to uncertainty, interpretation, the translation of evidence into documentation and how this is mediated by digital tools. These processes are then studied in the context of academic journal articles. Based on these studies, I argue that there is a mismatch between some of the ideals of post-processualist archaeology, for example reflexivity and multivocalism, and the standard forms for documenting and sharing research, which tend to be representative of a positivist-empiricist worldview. I conclude that there are obstacles to pursuing goals such as reflexivity and transparency in archaeological research, variously due to a lack of epistemological clarity in research programmes, the requirement to construct authoritative knowers alongside plausible knowledge claims, and the context of a 'computing turn' which emphasises certain affordances of digital research tools over others.

I therefore set out the proposal for a perspective on research practice which I have called ‘Epistemological Modesty’, inspired by accounts which emphasise the situated nature of knowledge, including Lakoff and Johnson's linguistics and Collins' account of tacit knowledge. I describe the development and testing of a digital research notebook tool which was designed to embody the ‘epistemologically modest’ approach. Based on a series of feedback sessions with students, I conclude that digital archaeology needs to be as much a social, networked practice as a technology-based one, if it is to be successful in taking into account the implications of situated knowledge; and that web-based collaboration and publication has the potential to be a good fit for this.

My findings show that engagement with the particularities of local knowledge—embodied practices, negotiations related to uncertainty, the affordances of technologies, the influence of the researcher’s voice and the full range of other agencies—is vital for transparency and the meaningful reuse of data. Such an approach can arguably help to foster a research landscape which is more self-aware, more multivocal, and better able to bring questions of epistemological validity into focus.

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Acknowledgements and Thanks

This research was funded by the SEAHA David Arnold Memorial Studentship.

I want to record my grateful thanks to the SEAHA (Science and Engineering in Arts, Heritage and Archaeology) CDT and the EPSRC for the very special opportunity to study this topic in depth.

In particular, I would like to thank Dr. Mark Erickson and Dr. Roger Evans, who supported the project from the start and patiently helped me throughout, generously sharing their expertise and providing invaluable guidance, wisdom and encouragement, always with kind good humour.

My sincere thanks go to the project partners, Edmund Lee at Historic England and Michael Selway at System Simulation Ltd. I greatly appreciated their advice and helpful suggestions.

In my research I was very fortunate to receive welcoming and helpful cooperation from many members of the archaeology community, and I am extremely grateful to them all for taking the time to share their experiences and knowledge with me, and for their backing of early-career research. Those curators and scientists at the British Museum who lent me their initial support gave the project the impetus to get started. The team members of the documentation project I observed were particularly generous in allowing me to participate in their meetings and workshops over a period of many months, and their facilitation of observations made the continuation of the project possible in spite of Covid lockdowns. I was inspired by the passion, curiosity and professionalism of all of those I encountered.

My thanks also go to the students who enthusiastically participated in the testing of the Orson software, and to all of those who have shown interest in the goals of the project and provided me with encouragement to develop the software further.

I would like to thank the SEAHA staff and my fellow students at the University of Brighton, University College London and the University of Oxford, who have provided me with support and inspiration in various ways throughout the course of my PhD and the preceding MRes.

I dedicate this work with much love to Rachel, Finlay and Edie. You're the best.

Author's Declaration

I declare that the research contained in this thesis, unless otherwise formally indicated within the text, is the original work of the author. The thesis has not been previously submitted to this or any other university for a degree, and does not incorporate any material already submitted for a degree.

Signed

Michael Kelly

Dated

16th March 2023

Thesis Chapter Structure

1. Introduction

In this chapter I consider the connections between representational techniques in archaeology and the philosophical assumptions of researchers in relation to knowledge construction. I argue that knowledge representation in archaeology mostly fails to adequately reflect the nuanced philosophical positions of cultural heritage research theory, and instead tends to be an expression of a broadly positivist outlook, a 'soft processualism'. This inspires the question of whether digital tools in archaeological research can better reflect theoretical attempts in the discipline to develop self-criticality, reflexivity and transparency. Do the affordances of technology open up new opportunities to do this, and are there factors which have inhibited such changes up until this point?

2. Literature review

This literature review is a study of accounts of knowledge representation in archaeology. The first section presents a selective overview of some key periods in the evolution of the archaeological disciplines up until the digital age, their philosophical approaches to building knowledge claims, and how these relate to the formal *representational* aspects of archaeological documentation and published research outcomes. It introduces some necessary historical context for later discussions.

The second and main section discusses the literature about the affordances of digital technologies for constructing and sharing archaeological knowledge, the epistemological assumptions which inform them and the relationship to representational forms. The review concludes by presenting a perspective on the gaps in the literature that the rest of the thesis will seek to occupy via a discussion of technologically mediated representations of situated knowledge.

3. Methodology and methods

This chapter describes the methodological approach adopted for understanding the epistemological assumptions of archaeologists, their understandings of facticity, and the relationship to the formal aspects of their research practices. It is a social constructivist

approach derived primarily from Actor Network Theory, which looks for the most plausible explanations for phenomena while accepting that a truly 'objective' or certain knowledge of them is unattainable, and that the possibility of alternative valid accounts exists. The approach looks at the interplay of structure and agency and their mutual influences, and after Latour, tries to understand why some configurations of these are more successful than others, allowing particular knowledge models to gain acceptance.

As indicated by the pragmatic research approach, an abductive approach is followed for arriving at explanations. This is based on data acquired from the coding and thematic analysis of interviews with archaeologists and software testers, using qualitative analysis software, and from a narrative description of observations of video-conferencing-based working meetings, workshops and discussions in an international archaeology project. 'Thick description' (Geertz, 1977) is used to give an account of these observations in light of the theoretical assumptions of the research, which do not allow for an objectivist separation of observer and observed.

4. Study 1 — Interviews

The study 1 report gives an account of interviews with a range of professional archaeologists about their research practices. This study seeks to understand what archaeological knowledge work consists of, the assumptions that it rests on, and which digital tools are used in support of this. The role of professional and institutional affiliations is explored, and particular emphasis is given to discussions of the issues of interpretation, the handling of uncertainty, and existing archaeological initiatives for making research work transparent and reusable. The interviews are analysed using codes and themes.

5. Study 2 — Observations

Study 2 describes observations of the video-conferencing meetings and workshops of an international archaeological documentation project over a period of 8 months. The project observation is recounted narratively using thick description, after an initial coding phase. The formalised division between the collection of evidence and its interpretation is highlighted, as is the significance of the internal logic of the representational schemas used,

and the structuring influence of the use of computer databases. A significant theme is the administrative and philosophical effort required to form and maintain a research group with coherent, shared goals, while reconciling data from widely diverse landscapes—physical, political and technological. A complex picture emerges of researchers who are highly aware of the interpretative nature of their work, and diligent about avoiding unjustifiable claims, but do not always have opportunities to fully capture and share the nuanced interpretative aspects of their research.

6. Study 3 — Analysis of journal articles

Following on the from the insights gained from the previous study, I look to gain further insight into the progression from archaeological research and the handling of evidence through to knowledge claims, by viewing the process from a perspective that contrasts with my previous studies: that of the completed, 'polished' research output. I seek to explore the understanding of facticity in archaeological journal articles, the degree of certainty which authors feel empowered to express, the roles of technologies in collecting, processing and representing research, and how these find expression in the academic article form.

7. An epistemological programme

In response to the findings of the first three studies, which demonstrate deficits between some of archaeology's post-processual theoretical aspirations and its working practices, this section explores a potential remedy for the difficulties encountered in sharing nuanced accounts of interpretative archaeological research. The notion of 'Epistemological Modesty' is proposed, a perspective on knowledge building which pulls together strands from a range of theoretical writings, including Gero's feminist archaeological theory, Lakoff and Johnson's linguistics, Collins' account of tacit knowledge and Chapman and Wylie's archaeological pragmatism. Drawing on common themes from these, Epistemological Modesty emphasises the contingent nature of knowledge, its non-universality and the implications of this for making work which is continuously and collectively emergent, rather than fixed and exclusive.

8. Study 4 — Software development project

This study reports on the development and assessment of a software project, ‘Orson’ (Open Research Notebook), designed to incorporate the ‘epistemologically modest’ approach. The software consultation process and feedback sessions with students are described, again using thematic analysis. These cover discussions of the software’s feature set, and its place in the archaeological research process. There is a discussion of the relationship to ‘open access’ research practices which are gaining popularity, particularly in academic fields where ‘reproducibility’ is a concern.

9. Conclusion

What is the role of digital tools in transparent and self-critical cultural heritage research? Here I argue against a return to the ethos of letting ‘data speak for itself’ in the context of what I describe as 'the computing turn'. I reiterate the attempt to bring moderation to the notion of technologically mediated data as an independent witness with its own voice, by using tools such as Orson to *reinstate* the voices of researchers and the subjective nature of their interpretative knowledge claims, into digital research outputs. I discuss the range of affordances which computing technology can bring to research and emphasise the potential of sharing malleable digital material over networks, allowing work to unfold in an iterative, pluralistic way.

* * *

1. Introduction

This thesis is about knowledge making and knowledge sharing in archaeology. It explores how the processes of interpretation in archaeological research are mediated by the use of software and digital tools, and whether the use of innovative tools might engender new ways of working and new modes of engagement with research.

This line of inquiry stems from the basic theoretical assumption that knowledge is contingent, non-universal and continually emergent rather than fixed. The corollary is that for research to be robust and transparent, it must address questions of its own structural assumptions and their limitations alongside the detailed study of its object. The thesis therefore examines the links between digital knowledge-making practices and values which have emerged as research virtues for those sceptical or critical of universal models of knowledge: reflexivity, transparency and multivocality.

1.1 Background and motivation

The importance of reflexivity and the context of research are recurring themes in this thesis, so it is appropriate that I start by setting out the background to this piece of work. My interest in the topic is a consequence of my own professional engagement with knowledge-making work in various forms: first of all as a learning technologist at London art colleges, where I supported staff and students in their use of web technologies for the development and representation of their learning, research and teaching work; and then as a museum employee where I was part of a team developing a digital system for the representation of collections and collection-related research. My academic and professional background has been useful in allowing me to access different professional worlds, and in particular, the transition from working with art college staff to computer scientists in the museum context exposed me to a range of ways of knowing and to the varied commitments which academics bring to research.

My impression was that those working within an art and design tradition were often concerned to share aspects of their own personal experience in their work, and to innovate at a formal level. These two facets of their work were often mutually reinforcing, as

consideration of form and content were not always easily separable, depending on the media being used and the context of fine art or commercial art. Together with the strong tradition of criticality in modern art and beyond, a strong awareness of art 'movements' and the cyclical nature of critical theory in art production, these emphases resulted in a non-consensual research space where subjectivity was valued and authority often resisted. My experience of working with computer scientists, on the other hand, was that there was a greater concern for developing and following consensual models, a sense that while research was often contested, what was at stake was the discovery of definitive answers, and that subjectivity was mainly to be taken into account in order to be eliminated. Technological innovation was an important driver of research. In my area of work, there was little emphasis on the development of a conscious epistemological position, or the need to site work within a theoretical tradition. This was particularly noticeable, and for me dissonant, where computer science was being used in the service of archaeological research, a discipline with a history of sophisticated and strongly contested philosophical ideas. The modus operandi was to bring computer science or its deliverables as a *neutral* resource or resources, largely unburdened by philosophical complications, to the business of archaeological research, whereupon the more nuanced work of interpretation could subsequently be undertaken. This pattern, of the separation of 'facts' from interpretation, is arguably repeated in archaeology's incorporation of outputs from various scientific specialisms (see §3.3.4).

These very broad impressions inspired me to investigate ways of knowing and representations of knowledge in more depth. In my museum post I worked at close quarters with archaeological data and records for artefacts in the museum collection, while hearing and reading stories of the historical and present-day work of archaeologists and their collection of evidence. The perspective I brought prompted me to see a mismatch between standardised records and the rich interpretative work which brought them into being. I was therefore inspired to discover writing by Drucker (2011, 2014) about the theme of knowledge representation in cultural heritage research, which was helpful in articulating some of my own concerns. This prompted further reflections on my part and ultimately resulted in the development of this PhD project.

The potential for innovative practice using digital technology has been one of my main career interests and it was therefore a natural focus for this project. In my professional work I took an optimistic view of the potential of digital media and the web for learning, knowledge sharing, knowledge construction and knowledge management. As I was actively working on the development of web applications, I was conscious of misalignments between the theoretical aspirations of software platforms and the everyday work processes of learners and researchers. This inspired me to consider the bigger picture of research work, alongside the details of software design and use.

While I am a competent computer programmer, my perspective is most fairly summed up as being that of an academic artist or maker who engages creatively with computing for research, rather than as an academic studying research themes specific to computer-science. My professional experience encompasses the development of web applications for a variety of projects, including: the use of computer networking for building communities of learners; the creation of online portfolios to support reflexive learning and development of professional identity; and the structuring and management of museum records to facilitate research and the development of knowledge claims. I brought the lessons learned from these projects to this PhD project, together with real-world experience of how such initiatives fit with the factors of funding, institutional remits, and diverse types of user engagement, into the bigger pictures of educational and research attainment.

* * *

> In an era of fake news, the issue of epistemic vigilance would seem to be ever more relevant (Sperber et al. 2010); at a time when access to massive online information systems such as Wikipedia makes finding things out as easy as a few clicks on a mouse, the problem of what to trust and not has never been more pertinent. And this is not just a problem that goes away by tracing the source of knowledge to an authoritative locus, that is, science. (Lucas, 2019:15)

The wider backdrop to this thesis and its consideration of knowledge making is my personal experience of a particular cultural and political moment, and the pressing issues which it has thrown up, issues which in my view are deeply significant for any researcher who cares about transparency and integrity in their own work and that of others. The latter part of my

PhD project has taken place in the context of the Covid-19 pandemic, and the related limitations to social interactions which were introduced in the UK to stem the contagion of the virus and reduce the load on health services. It has been striking, in this time of turmoil, to see the battle of ideas playing out in real time, in the UK and across the world, about the efficacy of lockdowns, vaccines, masks, border controls and other measures to contain the virus. What has been made clear is the disjuncture between scientific research and its claims, and political policy. Epidemiologists and other researchers have been by turns acclaimed and denounced by laymen, politicians and other policy makers and opinion formers, as the influences of diverse ideological worldviews and conspiracy theories have been brought to bear. The realisation that the implications of peer-reviewed research are not always acted upon in a crisis is perhaps not a huge surprise in itself, especially in a fast-moving field without a strong consensus. What is surprising is the emergence of strong public narratives in direct opposition to research findings for public health, for example on the assumed dangers of vaccine use. From one standpoint, the fact that ideas can emerge so well-formed from the most meagre of roots could be considered a characteristic of the ascent of 'post-truth' politics (e.g. Lewandowsky et al., 2017). This might alternatively be attributed to distrust of authority, or to what Graeber and Wengrow have called *schismogenesis*, a tendency to define a sense of cultural identity in opposition to other groups and their beliefs (Graeber and Wengrow, 2021). Clearly, the existence of such dissonant perspectives can have far-reaching consequences. This is seen most poignantly in the gathering climate crisis, in which the overwhelmingly consensual outcomes of decades-long research programmes by climate scientists have been challenged by misinformation about the science and its implications (deeply rooted in this case, e.g. Oreskes and Conway, 2011), with the result that effective remedial action against warming has been delayed again and again.

In my understanding, these examples highlight the *constructed* nature of knowledge claims and their susceptibility to agencies of all kinds. They undermine the metanarrative (Lyotard, 1984) of knowledge, human progress and enlightenment—the notion that these are linked deterministically to tenets of rationalism, logic and the ever-evolving, ever-improving results of research conducted using the scientific method. A key theme in this thesis is that diverse voices should be heard—but that their claims should be transparent, and open to

examination in a reflective and critical space. Such conditions do not prevail when conflicting narratives flourish for reasons of bad faith, obfuscation or political convenience.

1.2 Theoretical position

In light of these concerns I have adopted a theoretical approach which recognises the constructed, situated nature of knowledge claims. I take inspiration from the 'strong programme' (e.g. Barnes et al., 1996) of the sociology of scientific knowledge (SSK), taking into account as it does the full range of factors which characterise 'normal' scientific activity. The strong programme assumes that this 'normal' work is 'socially' constructed rather than a fact of nature, and avoids a teleological reading of scientific history (e.g. Erickson, 2016:85-86). In this thesis I proceed from the same principle, assuming that this approach is valid not just for the study of science but across the multidisciplinary landscape of archaeological research and cultural heritage (CH) research more generally, and I refer to the work of theorists who write in this tradition. The notion of 'social construction', however, is a multivalent one and can be applied to a range of ideas and objects (Hacking, 1999). I follow the example of Latour in understanding 'social' as referring not to 'the stuff out of which other things were made […] but to the associations of many different sources of relatively solid ingredients' (Latour, 2003:4-5). Understanding that knowledge is constructed means recognising that:

> there is no maker, no master, no creator that could be said to dominate materials, or, at the very least, a new uncertainty is introduced as to what is to be built as well as to who is responsible for the emergence of the virtualities of the materials at hand. (Ibid, p6)

When I refer to constructivism then, it is not as an understanding which allows for an 'unmasking' of underlying autonomous 'social relations', a 'social realist' layer beneath the surface of things; rather, following the example of Actor Network Theory, it is used to mean 'the collective process that ends up as solid constructs through the mobilization of heterogeneous crafts, ingredients and coordination' (ibid, p4).

This key plank in my theoretical approach enables and justifies my analysis of the agency of technological tools in archaeological research, and informs the lines of enquiry in my studies

of archaeologists at work. The associated rejection of the concept of a dominant 'master maker' also feeds into my development of a consciously 'modest' research epistemology (Chapter 7).

Complementary to this focus on local agencies, their qualities and their mutual influences, I will refer often to the notion of 'situated knowledge', the idea that knowledge is actively constructed and shaped by the peculiarities of context. I make the assumption that a significant contributing aspect of situated knowledge is the 'thought style' ('Denkstil', Fleck, 1981) of the research community in which work takes place, and that the notion of research validity is only meaningful where research work is carried out in an academic community with established norms. I pursue the notion, set out by Hacking (1992) that different thought styles, or as he puts it, 'reasoning styles', have characteristic ways of constructing knowledge claims, often with their own distinctive technical tools and objects. I take these tools as a starting point for parts of my analysis, again following the example of Latour (e.g., Latour, 2005, Latour, 2017) and others (e.g., Lemonnier, 1993) in their studies of the agency of technological objects. I also take it as axiomatic that considerations of representational form are inseparable from the epistemological assumptions which underlie those forms—that there is no such thing as a *neutral* representational vehicle for the delivery of diverse types of knowledge claim and that indeed the media used for structuring and communicating research have significant agencies which should be accounted for when trying to understand how knowledge is arrived at. Just as with tools, the form and shape of academic research outputs—the interfaces through which ideas are developed and communicated—play a significant role in ordering, shaping and limiting understanding, and perform the important function of providing a site or object for the negotiated production of knowledge claims. In other words, different representational interfaces have their own ontological and epistemological commitments. For this reason they are worthy of consideration as agential objects in their own right. In the case of software platforms, the distinction between representational form and tool can be blurred, but this potential ambiguity only extends the reach of their significance.

821.3 Research questions

The goal of my research is to make the work of *interpretation* in the discipline of archaeology more open to interrogation, by clarifying how epistemology is enabled and enacted in archaeological research through the application of digital tools and representations. The development of a better understanding of the relationship between knowledge making and digital practices in archaeological scholarship, and the nature of the agencies at work in these processes, is an essential starting point for considerations of directions for future research practice. It can inform debates about the *ideals* of research as well as practical attempts to make research outputs more transparent and reusable.

There have long been debates about what archaeology can know about the past, and at times these have been highly divisive (e.g. Chapman and Wylie, 2016:19). As a consequence there is a history of sophisticated, self-reflective philosophical analysis of facticity in the field, and in general a strong degree of caution about making definitive knowledge claims. In spite of these historical lessons, I argue here, after Drucker (2014), that the standard techniques for representing knowledge in archaeology mostly fail to adequately reflect the nuanced philosophical positions of archaeological research theory, and some of its commonly shared aspirations, such as reflexivity, transparency and multivocality (e.g. Hodder, 2000); instead they tend to reflect the concerns of a positivist outlook. In this research project I attempt to demonstrate both that this *is* the case, to understand *why* it should be so, and to propose alternative approaches.

Studying the affordances of archaeology's digital technologies allows me to consider the implications for research ethics and the aspiration to promote research virtues such as transparency and reflexivity.

The key research questions addressed are therefore as follows:

- How do archaeologists formalise the process of interpretation and what is their understanding of facticity?
- How is epistemology enabled and enacted in archaeological research through the application of digital tools and representations?
- Do digital tools and representations by their nature inhibit or enable opportunities to manifest or support reflexivity, transparency and multivocality?

- How does the formal interface of research outputs affect the ways in which research is engaged with or repurposed?
- Can formal innovations in digital representational modes encourage new relationships with archaeological knowledge?

In order to explore the gaps between archaeological theory and practice, and to develop an understanding of the 'epistemic register' (Lucas, 2019:132) of digital tools and media, I approach archaeological research practice from a variety of perspectives: as an interviewer; as an observer; through the analysis of academic articles; and via the development and testing of a software tool which is designed explicitly to promote reflexivity and multivocality, and to encourage the reuse and reinterpretation of its content.

1.4 Research values

The impacts of the applications of CH research and conservation efforts can be far-reaching, and choices have to be made about where to focus resources and funding. These choices are a reflection of our own interests, whether they be in 'pure' knowledge about the past, the preservation of a building, language or craft, the creation of jobs in a community, or the maintenance of biodiverse ecosystems. One of the central themes in CH research is 'value' (e.g. HM Treasury, 2002, Rizzo and Throsby, 2006). When assessing priorities in heritage research and management, we are required to consider what parts of our heritage we value, but also to take into account what value we are likely to get from heritage initiatives (which are often paid for with public money). Values in CH research are the same as those which shape our fundamental beliefs about how to live and how to make sense of the world and our own history. Unsurprisingly these are often highly contested, as illustrated by recent high-profile cases such the National Trust's initiative to document historical links to the slave trade in some of its publicly held properties (Huxtable et al., 2020). Such examples bring the importance of research ethics in the CH field into sharp focus. An ethical approach is required in order to communicate research results *and values*, clearly and honestly, in order to make them open to fair analysis and critique. Without this basic level of research integrity, the metanarrative of research projects combining to form links in a larger chain of

understanding is invalidated. In this thesis I root the idea of ethical research in the characteristics of transparency, openness and inclusivity. Related to these principles, and dependent on their successful application, is the ideal that research results should be open to reuse and reinterpretation, to facilitate future research and iterative analysis, and to allow for the re-evaluation of research from a range of cultural and theoretical perspectives. These ideals are expressions of a value system which emphasises diversity, inclusivity and epistemological reflexivity, based on the underlying principle that there should be equitable access to knowledge and knowledge-making resources for all. My research presumes that for interpretative research to be transparent, and to be properly reusable and reinterpretable, the limits of the interpretative process should, as far as is possible, be made apparent and interrogable. These themes will be developed at length in upcoming chapters.

1.5 Evolution of the Research Questions

As my project progressed, some of the detail of the research focus evolved. This was due to a developing understanding of archaeology's theoretical history and its diverse professional and academic practices. The practical part of the project was originally envisaged as consisting of the development of digital visualisation techniques which could be useful for incorporating considerations of uncertainty and subjectivity into archaeological research outcomes. This would have resulted in an examination of the same questions outlined in §1.3 but considered specifically through the lens of visualisation techniques and paradigms, rather than in terms of the qualities of digital media and digital representations in general. Having studied attempts to do this in the existing archaeological literature, and having observed archaeologists at work, I concluded that this would not be a good fit with the theoretical themes which were emerging in my research. It became clear that the questions I was interested in, particularly the differences between universal and situated models of knowledge, could not be addressed meaningfully at the narrow level of visualisations of data. Drucker presents a tentative attempt to explore possibilities in this area by reimagining one of the most famous uses of data visualisation for research, John Snow's 1854 mapping of London water pumps in relation to cholera outbreaks (Drucker, 2014). She transforms his representation from a traditional cartographic plan view to a first-person

perspective of London streets. This is a thought-provoking example, but is narrow in its analytical and critical power—for example, it portrays knowledge perspective in terms of individual experience rather than those of the norms of a 'thought-collective' (Fleck, 1981)—and is difficult to generalise into a principle which provides value for research more widely.

It also became increasingly clear, as my research progressed, that innovations related to technological novelty and the affordances of digital communications and data computation have a strong influence on the character of current archaeological research. I therefore turned my attention to the development of a better overall understanding of the relationship between the qualities of digital media, epistemology, and the construction of knowledge claims. This still allowed me to consider the potential of digital representations for the support of research virtues: for example, transparency through the digital documentation of the research process itself, or support for multivocality by making digital research outcomes more accessible and reinterpretable. This approach also retained the potential to provide insight into the handling of uncertainty, by emphasising reflection on the limits of situated knowledge. *Epistemological* uncertainty is to be distinguished from the uncertainty associated with ontological *incompleteness*, as I will discuss in §7.5.5. Too often in data representation software packages, the user is limited to statistical visualisations—bar chart, pie chart, whisker plots, etc.—which are expressions of the same epistemological perspective. The emphasis in this thesis on the 'epistemic register' of representations which embody different ways of knowing enables discussion beyond that of the use of prescriptive visualisation techniques. It broadens the scope of the practical software development aspect of the research, allowing for the consideration of the various contact points between researchers and their software tools, beyond the visual alone.

1.6 Defining terms and scope

In the context of this thesis 'representational forms' refers to the media with which research is documented and communicated, their formal aspects, their structuring principles and the opportunities they afford for engagement and interaction. My main focus is on the use of digital software tools and platforms for the creation and sharing of knowledge claims, but I

will also consider journal articles and their uses of narrative and visualisation, due to their historical and ongoing importance and strong links to digital forms.

'Digital research' and 'digital archaeology' are broad terms which refer to the uses of computing in archaeology, from the earliest uses of mainframe, punch-card computing through to the current age of desktop computers, pervasive web access and mobile device use. The most relevant aspects of digital archaeological research for this thesis are those which are widely affordable and accessible, and have a strong bearing on the practicalities of collecting, organising and sharing evidence and findings. The landscapes of technology and software use change rapidly, but certain features mark out the ongoing appeal of computing for research: cheap and powerful processing power, capacious storage, networked connectivity, mobile access, and innovative ways of capturing and representing data.

I hope and expect that this research will be relevant to a broad range of academic disciplines. However, discussion is limited to CH research and in particular, archaeology. It is important to emphasise the cultural significance of archaeological work and the ethical dimension of research in that field, and this is perhaps best expressed through a discussion of heritage values. However, I fully recognise that in terms of academic subjects and professions, archaeology has a distinctive tradition and often a different set of methods from cultural heritage studies or cultural heritage management. Archaeology was selected as a subject of study because of my previous experience of working with archaeologists, but also for the following important reasons:

- the discipline's inherent need to confront uncertainty
- its complex multidisciplinary identities, which encompass the academic traditions of humanities and the sciences
- the historical internal debates in the discipline which help to crystallise broader issues about epistemological validity in research, making it a useful point of reference for other fields

My assumption is that in archaeology, the conspicuous gaps in the evidential record mean that the theoretical glue required to piece together these fragments into coherent claims is more clearly discernible—that the processes of knowledge making are more externalised,

more debated and more susceptible to analysis in that field than they are in many other disciplines, even though their workings may not otherwise be particularly unusual. Ideally, the lessons learned from studying archaeology may have relevance for other disciplines in which the 'normal' work is less amenable to deconstruction and analysis.

The political and cultural significance of archaeology's research practices and narratives continues to drive debates about its identity and purpose. In this thesis I make the case for a multivocal archaeological practice, and challenge the ideal of definitive, universal knowledge models; but my investigation, conducted as it is primarily in the context of British institutions, is inevitably a reflection of work made in that country's disciplinary traditions, and is thus relatively narrow in its scope. This is particularly true in the case of my interviews and observations of archaeologists. All interviewees were from a European or North American tradition, as were almost all of those in positions of authority in the project I observed. For some of the archaeologists I encountered there was a strong consciousness of a need to challenge the historical cultural hegemony in archaeology and they had worked collaboratively to this end with archaeologists or other stakeholders from countries with different traditions. I report on these perspectives in the thesis. I also include the voices and perspectives of those challenging or working outside of the Western tradition, via references to the literature; but I was conscious that the institutional literature resources which I made use of to construct an understanding of the history of archaeology were strongly weighted to be representative of the historical dominance of the British/American tradition. In studying a mostly British tradition of archaeology, from within an English academic institution, then, my purpose is to understand how that particular manifestation of archaeology makes its knowledge claims, and the extent to which the theoretical championing of research virtues such as multivocality and the recognition of a range of ontological understandings have found expression in its mainstream practices. The concept of 'archaeology' in this thesis is therefore an imperfect shorthand expression for the Western tradition, assumptions and experience of the discipline, but is explicitly *not* used, in spite of its singularity, as an expression of hierarchical validity.

1.7 Research field

This research applies a theoretical approach inspired by Science and Technology Studies to a cultural heritage topic, with a particular focus on the uses of digital tools for research. Furthermore, it uses the practical development of a software application as a means of gaining insight into and developing hypotheses about the agency of research tools. This combination of themes takes the project into the territory of interdisciplinary research. It can be best understood as a social science project, directed as it is towards understanding the research practices of archaeologists; however, in order to meaningfully explore the topics of archaeologists' understandings of facticity, and the role of digital tools in constructing archaeological knowledge, it examines questions of epistemology, the genealogy of archaeological thought, and the functioning of software tools. Themes from philosophy, archaeological history, Digital Archaeology (see §2.5) and human-computer interaction are therefore all present. This eclecticism is in part a consequence of the wide-ranging focus of archaeology, the transmutable nature of its disciplinary boundaries, and its appetite for absorbing technical and scientific techniques. In addition, the postulation of a link between the epistemological affordances of digital technology and situated knowledge-making results in a requirement to analyse software tools in the context of disciplinary practice. The exploration of the relationships between knowledge frameworks and representational forms is at the heart of this thesis, and it is taken as axiomatic that software tools and representational forms are subtended by epistemological assumptions which have a bearing on their structural and interactional aspects. In considering representations of knowledge, it is necessary to engage with the nature of the claims being made, but also, wherever possible, the philosophical traditions which they represent, and the agencies which contribute to their construction. The resultant approach, which might be considered interdisciplinary in character, is appropriate for an ANT-influenced understanding which attends to the qualities of objects and media as well as institutions and people—and finds precedence in the oeuvre of Latour, for example (e.g. 1993, 2017, 2021).

* * *

In the following survey of the relevant literature I discuss the writing which examines the relationships between archaeological schools of thought and their characteristic research outputs. In academic research the epistemological context for knowledge claims is sometimes stated explicitly, and the agencies which shape the work are foregrounded; more often, they are not. The next chapter draws out these connections, starting with some vital historical context, then taking a detailed look at a range of digital practices in archaeological research. It concludes by presenting a perspective on the gaps in the literature that the rest of the thesis will seek to occupy, via a discussion of technologically mediated representations of situated knowledge.

2. Literature review

2.1 Introduction

This literature review is a study of accounts of knowledge representation in archaeology. It is divided into two sections. The first section reviews accounts of pre-digital archaeological representations. This provides some necessary historical context for the subsequent discussion and for topics in later chapters (e.g. §7.5).

The second and main section is concerned specifically with the literature related to the affordances of digital technologies for constructing and sharing archaeological knowledge, the epistemological assumptions which inform them and the relationship to representational forms. It is structured based on the distinctive characteristics of digital research tools and representations.

There is a wealth of available material on the relationship between epistemology and knowledge representation, digital or otherwise, which does not refer specifically to archaeology. Summarising this literature is out of scope here, though naturally I will draw on such sources throughout the rest of the thesis. I limit my discussion in this review to archaeological research, with the result that any more general philosophical works are only referred to indirectly where they have been drawn upon by archaeologists. The use of imagery in archaeology is another theme which I have not addressed in any depth in this review: I have strived to keep my discussion to more generalised topics on representation as defined in §1.6.

2.2 Section 1. The pre-digital

This introductory section presents an overview of some key periods in the evolution of the archaeological disciplines up until the digital age, their philosophical approaches to building knowledge claims, and how these relate to the formal representational aspects of archaeological documentation and published research outcomes. This does not attempt to be a comprehensive chronology. I focus instead on those observations which provide

insights about key innovations and moments of transition in the history of archaeological thought.

2.2.1 The evolution of fieldwork

From one perspective, the historical development of archaeology can be understood in terms of a shift in the preoccupation with objects, or finds, to the creation, management and interpretation of archaeological records. The documentation of a site has become a proxy for the site itself, and the primary source of interpretation. This underscores the significance of the representational practices used to 'preserve' sites since, as Lucas puts it, 'archaeologists work on texts and documents as much as objects and deposits' (2012:18). The notion of the 'total record' in archaeology is often associated with Pitt Rivers, who in the late 19th century 'espoused a very firm position of the importance of total collection during excavation and its relevance for generalizing classification' (ibid, p45). But it was not until after World War II that urban redevelopment on a massive scale 'led to the emergence in the early 1970s of the contemporary practices of cultural or archaeological resource management, which form the foundation of current legislation and organization' (ibid, p67). The emergence of salvage or 'rescue' archaeology, excavations carried out as a precursor to a commercial development process which will often destroy or otherwise significantly impact local traces of the past (Renfrew and Bahn, 2016:74), brought the issues of preservation and destruction of archaeological remains to the forefront of archaeology. The results-driven nature of rescue archaeology has been subject to critiques suggesting that pro forma recording is 'formulaic', and that the emphasis on creating a 'total' record has encouraged archaeologists 'not to think about the uncomfortable facts about the site, but to try to put them (if "facts" they be) into the archive for the future to elucidate' (Lucas 2012:71, quoting Reynolds and Barber).

Prior to the 1870s, archaeological work had commonalities with antiquarianism, having roots in the discipline of history, and a concern for the typological and aesthetic value of individual finds. As with antiquarianism, this early archaeology was focused on 'collections rather than the observation of material in situ' (Lucas 2001:14). The 'professionalisation' of archaeology in the late nineteenth century (Levine, 1986), with the establishment of university chairs and journals, and its perceived development as a 'new science', coincided with a shift in attention from classification to culture, and from finds to sites. This shift of

emphasis had a significant bearing on recording and representing practices. The first texts on methodologies were published at this time (Lucas, 2019:29), and 'there was frequent reference to the standard nineteenth-century epistemology of knowledge through induction: the collection of facts followed by reasoned interpretation from those facts' (ibid). There was a burgeoning use of statistics and the first hints of the modern relationship with 'data', with Lubbock, Pitt Rivers and others introducing the use of statistical tables for the presentation of 'facts' (ibid, p31). The concern for 'a more contextual understanding of cultural lifeways' (Lucas, 2001:15) resulted in a prioritisation of cultural particularity and new importance being stressed on considerations of the archaeological site's own particularity. This was translated into fieldwork practices which reflected 'a greater concern for the specific spatial and temporal contexts of objects' (ibid). The result was a shift in focus from museums and studies to the field, and pro forma recording sheets were introduced to standardise the work and build in checks on quality and quantity. Changes in excavation processes and associated recording and representation practices reflected the evolving nature of site perception:

> For Pitt Rivers and others of his time, the archaeological site was a repository of objects which, if carefully excavated and linked to types of monuments, were instrumental in the construction of evolutionary typological sequences. For Wheeler, Kidder and their contemporaries, the site became a repository of an artefactual assemblage indicative of a culture group, and, if stratified, the locus of critical information on the chronological changes within this assemblage. For us, the site is a repository of behavioural patterns, structured activities revealed through close analysis of contextual association within or between assemblages. (Ibid, p62)

Archaeology's fieldwork recording practices arguably have their own agencies, which impact the excavation process and the materiality of the site:

> Consider a section drawing; conventionally, it is said to represent a cross-section of a deposit or series of deposits. But to draw a section, we must first prepare a vertical face in the ground. In fact, the reason we cut the ground in this particular way is precisely because we are already predisposed to read the soil as if it were a drawing. In short, the way we sculpt a site, the way we intervene with it, is set up precisely for the manner in which we read it in translation. In other words, we want the site to

> take on the material form of the archive as closely as possible and not the other way around, as orthodoxy would have it. (Lucas, 2012:239)

This can be seen in the symbiosis between Wheeler's influential approach to drawing cross-sections of dig sites, wherein layers are represented symbolically rather than naturalistically and have clear separating border lines (Figure 1), and the widespread adoption of excavation procedures based on discrete stratigraphic units (Lucas, 2001:208).

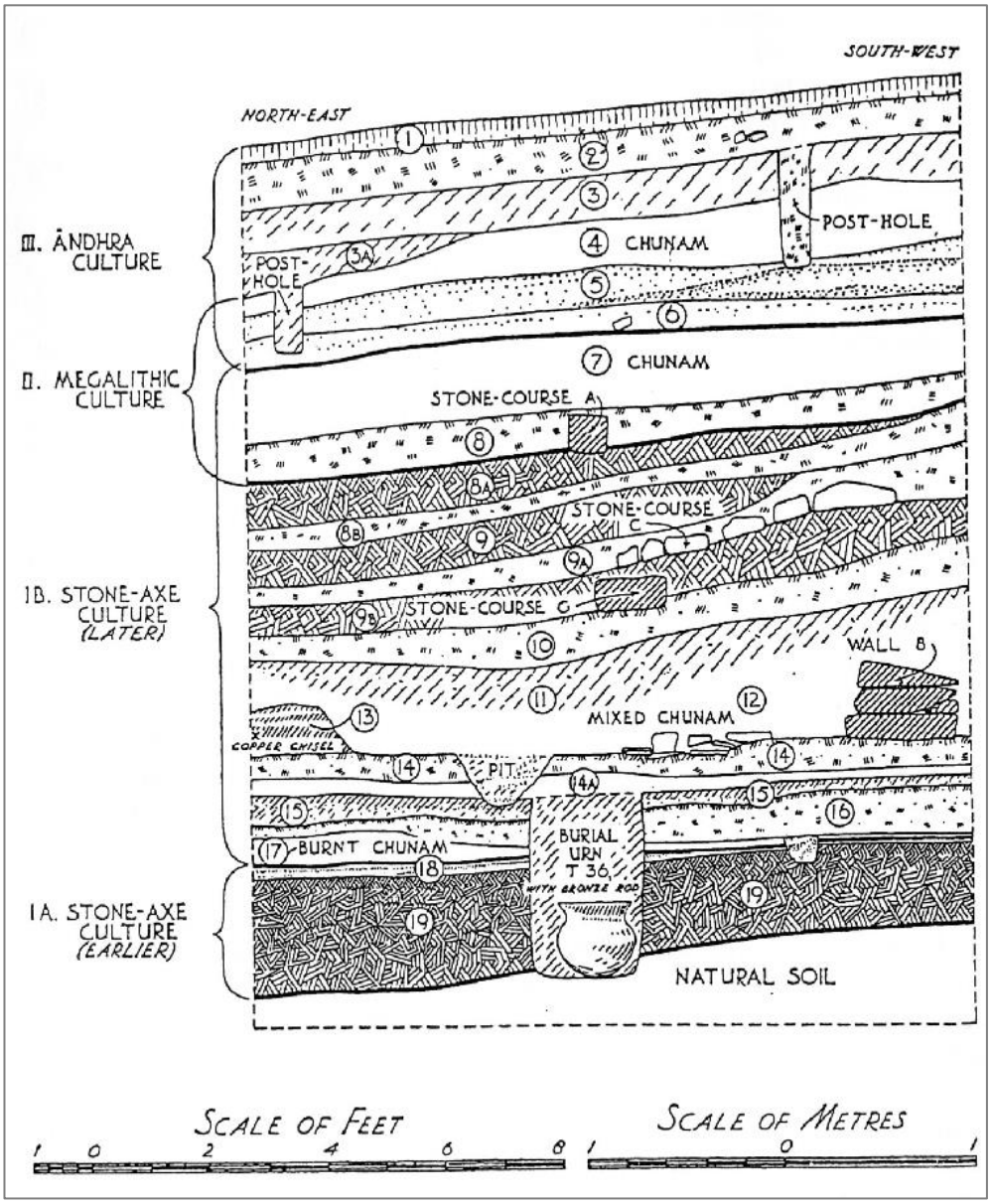


Figure 1. Example of a stratigraphic section drawing by Wheeler, from Brahmagiri, India (Reproduced from Wheeler, 1954:50).

The evolution of the stratigraphic method of excavation, linked to the changing relationship with perceptions of objects, cultures and behaviours, has culminated in the standardised use of the Single Context Recording method (SCR) (Lucas, 2001:56) , together with context sheets (see Figure 2, a page from a modern example of a context sheet) for the recording of stratigraphic 'units' and the use of the Harris Matrix (Harris, 1989) to visualise the relationships between the component stratigraphic elements of the recorded site (see Figure 11). This combination of techniques removes the previously deterministic link between the dug sections and the stratigraphic analysis—physical and stratigraphic relationships are not conflated but distinct (Lucas, 2001:57). This allows the site to be viewed as a 'palimpsest of discrete events' (ibid, p58), illustrating the conception of the site as a repository of structured activities. The introduction of this representative mode had significant agency on archaeological field practice: its use meant that 'all baulk sections could be dispensed with and everything excavated in plan' (ibid, p57).
The SCR method is characterised by a methodical and systematic approach and depends on the capture of information about stratigraphic units, known as 'contexts', in context sheets, i.e. standardised forms:

> It is critical that all records are signed and dated, and many records have boxes headed 'checked by ...', to be filled in by the supervisor. There is a clear inscription of surveillance in modern record-keeping which is directly linked to these hierarchies. It is argued, of course, that such a system is to provide a standardised and 'objective' record of the excavation, but in fact it controls not only the record, but also the bodies who produce it. (Lucas 2001:18)

This 'disciplining' of bodies has been subject to criticism by archaeologists describing the erasure of excavators in site photography (e.g. McFadyen et al., 1997). For Lucas what is most pertinent about such practices is the separation they enforce between fieldwork and interpretation, 'which sees the production of archaeological knowledge as akin to factory production rather than […] as a craft' (Lucas 2001:21). He sets out this conceptual pipeline of production as consisting of a series of phases: excavation; finds processing; analysis; and synthesis; and points out that this is 'the premise of English Heritage's Management of Archaeological Projects (MAP 2) which is the model for most contract fieldwork today in

Britain' (ibid, p23). The 1998 'PUNS' survey of user needs with regards to archaeological publications in UK and Ireland concludes that there is widespread dissatisfaction with the structure of archaeological reports (Jones et al., 2003). The report finds a discipline in transition, thanks to the influence of electronic communication. The authors specifically highlight 'the fallacy of 'preservation by record"' (ibid) and recommend that researchers take advantage of the availability of diverse media types to produce reports which are tailored to specific project requirements. A follow-up 'PUNS 2' initiative in 2023, led by the Council for British Archaeology, demonstrates an increased concern for the public benefits of archaeology (Recording Archaeology, 2023).

SURREY ARCHAEOLOGICAL SOCIETY **CONTEXT RECORD SHEET**

SITE CODE	GRID REFERENCE		SITE SUB-DIV	CONTEXT NUMBER
CATEGORY	TYPE	QUERY	REDUCED LEVELS	
			TOP	BOTTOM
LENGTH	BREADTH		DIAMETER	DEPTH/HEIGHT

COLOUR

COMPOSITION

TEXTURE

INCLUSIONS

CONSISTS OF/FILLED BY | PART OF/FILL OF

STRATIGHRAPHIC RELATIONSHIPS

BELOW

ABOVE

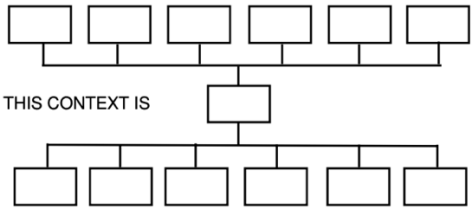
WITHIN

CONTAINS

SAME AS

EQUIVALENT TO

UNCERTAIN



PROMPTS FOR DESCRIPTION

CUTS:
1. Shape in plan
2. Corners
3. Break of slope - top
4. Sides
5. Break of slope - base
6. Base
7. Orientation
8. Inclination of axis
9. Truncation
10. Physical relations
11. Definition
12. Other comments

DEPOSITS:
1. Extent
2. Physical relations
3. Definition
4. Finds Density
5. Other Comments

DESCRIPTION:

(continued over)

PLANS	SECTIONS/ELEVATIONS/PROFILES	PHOTOGRAPHS
SAMPLE NUMBERS	RELIABILITY GOOD ☐ FAIR ☐ POOR ☐	METHOD OF EXCAVATION

INTERPRETATION

(continued over)

COMPLETED BY DATE	☐ % EXC	CHECKED BY DATE	DATE/PERIOD/PHASE

Figure 2. Context Sheet, one page from a series of recording forms.

Surrey Archaeology Society (2022).

2.2.2 Textual styles in archaeological research

In the 18th and 19th century, archaeology was recorded 'almost exclusively' in diaries and letters which exhibited 'many of the identifiable attributes of the genre of travel literature' (Mickel, 2015:301). Hodder states that:

> the style of writing (trope) of site reports in the 1770s in Archaeologia was very different from our own. […]
>
> First, the report is fixed in time and place. The letters are always dated to a particular day (such as May 31,1771), and the place of writing is also given. The events described also fix the report in a particular context. […]
>
> Second, the report frequently uses the first person pronoun that is appropriate to a letter. The author, the subject, the 'I' is undeniably present. (Hodder 1989:269)

Borrowing from Foucault's work, he suggests that the dominance in these early reports of the accounts of individuals depended in part on inherited authority and privilege: 'The individual and personal are emphasized in the texts, partly because power outside the text is also individual, personal and arbitrary' (ibid). In the 19th century, articles become more common, and by the end of the century, we start to see reports in which artefacts are placed into archaeological typologies. Modern-day concerns and writing styles start to become apparent in the reports of the late 19th and early 20th century:

> A sandstone block 'was found', and 'it must be noted that'. The imperative seems to suggest there can only be one possible interpretation. […]
>
> Indeed, admitted interpretation has largely disappeared behind objective description. Thus, 'a comparison. . . will show that' (Ashby 1906: 118)—as if the observations as well as the artefacts had been found, and as if the description is self-evident, distanced from any onlooker or author. (Ibid, p271)

Hodder suggests that the interpretative and personal aspects of earlier accounts were marginalised due to a legislative programme in England which empowered national commissioners to control excavations, thus dispersing power 'within the fabric of science and its institutions', and displacing its former traditional base of authority and power (ibid, p272). While approving of the removal of the arbitrary privilege which early archaeologists

enjoyed when they 'devastated monuments', Hodder is frustrated by impersonal modern-day reports which are 'dull, excessively long, detailed and expensive and read by no one except the delirious specialist' (ibid, p273). The code, he complains, has 'become everything, pursued for its own sake' (ibid). While not arguing for a return to 18th century writing, he argues that there is a duty to give heritage some meaning when preserving it, and that we 'have to work the older ideas out in new ways' (ibid, p274)—a useful dictum to consider in light of the goals of this thesis.

Pluciennik points out the 'hybrid nature and aims' of archaeological texts and their contexts of production: the narratives 'typically comprise a complex mixture of descriptions, arguments, tropes and explanations which often defy overall categorization' (1999:653). What historical texts have in common is an intellectual, moral and political viewpoint, and the 'use of narrative as the form of presentation and explanation as the goal' (ibid). In this regard they are to be distinguished from the narratives of 'hard' science which have a focus on 'confirmation (or contradiction) by replication' (ibid). In the examples he analyses:

> The story is typically told in the third-person passive, giving an often spurious sense of objective description, and less rarely (though increasingly) in the (authorial) first person, which at least emphasizes the intervention, constructed interpretation, and manipulation of the material by the writer. There is usually little sense of actions, events, or history considered from the actor's point of view… […] If there is a rhetoric of empathy, it is with the intellectual (and less often emotional) journey and experience of the author rather than of any past Others, who are represented in a distanced manner. (Ibid, p667)

Based on his observation of the hybrid nature of archaeological texts, and the fact that 'the discipline overlaps both sides of the 19th century divide between the natural and human sciences' (ibid, p659), Pluciennik suggests that neither conventional literary analysis nor evaluation in terms of explanatory value are sufficient for making sense of archaeological narratives. For the same reason he welcomes experimentation at the formal level of archaeological text-making:

> If Ankersmit's idea of situated perspectives (and similarly much feminist epistemology [e.g., Haraway 1988]) is accepted, then different viewpoints, goals, and meanings can be welcomed as more in sympathy with the diversity and richness of

> the past. This should equally be reflected in a variety of styles, approaches, and aims expressed through experimentation with the forms and functions of archaeological texts and images. (Ibid, p666)

Lucas explores the complexity of archaeological texts further by deconstructing them into their constituent parts in an attempt to understand their knowledge-making work. In a process analogous to White's (1973) categorisation of the narrative models of historical interpretation, he identifies four key text types used in archaeological articles and monographs: description, narration, exposition and argument. Where White links narrative models to ideological implications, Lucas discusses his types in terms of their 'epistemic and ontological registers' (Lucas, 2019:29). While acknowledging that there are continuities between the different types (and in his examples, demonstrating that some texts are hybrids of these), he makes the case that the *register* of each one should guide the criteria to draw on when evaluating the 'strength and quality of its knowledge production' (ibid, p132). I discuss and make use of this approach in §7.4.

2.2.3 New Archaeology and the scientific approach

The disciplinary divisions referred to by Pluciennik were most famously described by C.P. Snow in his 1959 lecture on 'The Two Cultures' (Snow, 1959). In it he bemoaned the gap between the intellectual traditions of the humanities and the sciences and advocated for bridge-building between them. According to Trigger, culture-historical and functional-processual approaches had existed in a complementary fashion in the archaeological scholarship of the 1950s, but this changed in the early 1960s when:

> a group of American processual archaeologists launched an all-out attack on culture-historical archaeology, which they proposed to replace with an approach that was evolutionist, behaviorist, ecological, and positivist in orientation. (Trigger, 2006:386)

The so-called 'New Archaeology' instigated by Lewis Binford and colleagues maintained that 'the potential of the archaeological evidence was much greater than had been realized for the investigation of social and economic aspects of past societies' (Renfrew and Bahn, 2016:40). Their optimistic view of archaeology argued for the use of explicit reasoning, inspired by the philosophy of science, and they sought to make conclusions testable, and to

analyse a culture as 'a system which could be broken down into subsystems' (ibid). Before, this up until the 1960s, most archaeologists were 'naïve empiricists' (Trigger, 2006:29). Trigger attributes the strong impact of New Archaeology to 'the deeply engrained tendency of Americans to value what was useful':

> Their contempt for what was not practical manifested itself in the low respect they accorded to historical studies generally (Bronowski 1971: 195), an opinion epitomized by the industrialist Henry Ford's remark that 'History is . . . bunk'. (Ibid, p407)

For Binford,

> cultural changes came about as a result of human groups' responding rationally to the stresses produced by natural ecological changes. Because all aspects of culture could be understood in terms of their adaptive significance, it was not necessary to ascertain what specific groups of people had actually known or believed in order to understand change. Thus, culturally specific beliefs, cultural traditions, and idiosyncratic behavior need be of no interest to archaeologists. (Ibid, p395)

This approach entailed 'a turn away from the approaches of history towards those of the sciences' (Renfrew and Bahn, 2016:41). In a reading which borrows from Kuhn's paradigm-oriented understanding of science (2012), Kristiansen connects this move to the undermining of the Neolithic chronology in the 50s and 60s by ^{14}C dating technology, which created opportunities for a new theoretical and interpretative framework to replace the old (Kristiansen, 2017:120).

The New Archaeology sought to separate out a universal reality and its observable phenomena from the observer. The emphasis on systems and process (the 'process' of 'processualism') affected site interpretation, as 'the archaeological record came to be viewed less as a set of cultural vestiges or relics and more as components of a social system' (Lucas, 2012:51). In Trigger's account, the proponents of the approach disagreed almost from the start the 'about the causes of cultural change and the degree to which human behaviour was shaped by these factors' (2006:418). In spite of these differences, there was an effect on archaeological *practice* in shifts towards 'problem-oriented and research-directed excavation', 'greater standardization of recording to avoid idiosyncrasies of the

individual field worker and enhance data comparability' and 'the adoption of sampling strategies in terms of systematic spatial coverage and object recovery' (Lucas, 2012:62). The overall emphasis on generalization and sampling methods contributed momentum to the application of the 'scientific method' and scientific representation of data in the discipline, for example in the use of statistics, and grids:

> Both on the larger scale of the landscape and the micro-scale of the site, sampling strategies were undoubtedly a major innovation in fieldwork around the 1960s. Use of grids for field survey, geophysical and geochemical methods of site prospecting, and the routine adoption of on-site screening (i.e. sieving) and flotation of deposits for environmental and other remains are all major developments that took off after this time.
>
> Binford, in an important paper on research design, criticised most earlier fieldwork for treating sites as essentially 'mines' for the recovery of artefacts with little regard for features or the relationship between artefacts and features. (Lucas, 2001:60)

Renfrew, a British archaeologist influenced by Binford (Trigger, 2006:393), frames the emergence of New Archaeology as a 'turning point in archaeology', arguing that its goal of *explaining* as well as *describing* has been highly influential (Renfrew and Bahn, 2016:41). In spite of a shift from an early scientistic 'functionalist-processual' phase to a less positivistic 'cognitive-processual' phase (ibid), the New Archaeologists required new techniques, consisting of 'intensive field survey and selective excavation, coupled with statistically based sampling procedures' and these, Renfrew suggests, in what is the standard UK introductory textbook to archaeology, 'are the key elements of modern field research' (ibid). The continuities with current practice pointed out by Renfrew and Bahn are significant for considerations of the formal aspects of modern-day fieldwork, their underlying philosophical assumptions, and the symbiosis with representational types. In particular, they point out:

> the great willingness of the New Archaeologists to employ more sophisticated quantitative techniques, computer-aided where possible (computers were first used for the storage, organization, and analysis of data in the 1960s). (Renfrew and Bahn, 2016:41)

The compatibility of the New Archaeologists' research methods with computing tools can be seen as a bridge to modern digital archaeology and the ongoing opportunities presented by a 'computing turn' (see Chapter 9).

2.2.4 Processualism to post-processualism

The path to contemporary archaeological practice(s) does not follow a direct line from the work of Binford and his colleagues in processual archaeology. In the 1980s the post-processualists rejected the 'idealist quest for generalizing laws of human history and behaviour' (Chilton, 2014:36) and shifted focus to the idiographic. Their theoretical approaches argued for constructivist understandings of knowledge creation, emphasising, to greater or lesser extents, the role of subjectivity in experiencing the world and making sense of it. Rather than being a single philosophical movement, post-processualism can be understood as a blanket term for critiques of the deterministic arguments and logical positivist methods which had shown flaws over the 20-year course of their deployment:

> Postprocessual archaeology was and is not a cohesive theoretical approach or paradigm, and proponents embrace a wide variety of theoretical perspectives: neo-Marxism, postmodernism, feminist theory, post-structuralism, critical theory, etc. As Preucel (1995:147) puts it, 'the term refers not to a unified program but, rather, to a collection of widely divergent and often contradictory research interests.' (Chilton 2014:35)

This broad countermovement, in addition to introducing a culture historical theoretical framework, also, according to Kristiansen, largely abolished science and quantification, for their perceived effects of 'dehumanizing' history:

> …for the next two decades quantitative methods and science-based knowledge more or less vanished from archaeological interpretation. Instead agency-based, contextual interpretations took the front seat. (Kristiansen, 2014:12)

In a more moderate account, Preucel describes the rise of post-processualism as a 'gradual incorporation of questions of identity, meaning, agency and practice alongside those of system, process and structure' (Preucel, 2012:17). In spite of the diverse perspectives represented, there were shared theoretical imperatives, not least a reckoning with

hegemonical power structures and faith in the emancipatory potential of new practices. A lasting legacy of the movement, therefore, is greater inclusivity in archaeology and a broader range of concerns (Chilton, 2014:36).

The emergence of post-processualism in archaeology was a reflection of wider contemporary philosophical trends in social science and critical theory, which were inspired by the claims of structuralism, post-structuralism, and critiques of institutionalised power structures. During the same period came significant changes in another field: that of accessible computing.

2.3 Section 2. The Digital

Already in 1973, Clarke was able to single out 'computer methodology', along with 'isotope chronology', as the most important of the many technically sophisticated methodological aspects of the 'New Archaeology' (1973:9). Computers were 'sense-extending machine tools' and at the same time provided 'powerful hammer-and-anvil procedures to beat out archaeological theory from intransigent data' (ibid). By the end of the decade, highly affordable home computing had arrived in the world's wealthy countries. In 1980, the Sinclair ZX-80 was launched in the UK for less than £100[1], and in the US Microsoft started the development of MS-DOS for the PC[2]. Thereafter, computing became increasingly ubiquitous for communication, information management and publishing in most spheres of knowledge-related work, and its importance only increased in the 90s with the advent of the World Wide Web ('the web') and the promises it brought for sharing and accessing information and ideas. The computing 'revolution' therefore came into full bloom alongside the processual/post-processual debates in archaeology, but as Clarke's comments illustrate, the opportunities to exploit computing in archaeological research were already well established for those applying scientific and statistical methods to archaeological ends. As computing became more affordable and ubiquitous, new possibilities for publication, visualisation and networked communication became ever more apparent. For archaeology,

[1] http://www.computinghistory.org.uk/det/54284/Sinclair-ZX80-8K-Basic/
[2] https://www.britannica.com/technology/MS-DOS

with its multi-disciplinary processes of knowledge production, comprising sequences of data collection, documentation, analysis, interpretation and publication, the promises and potentials of working with digital media have been diverse, and engagement with their integration into practice continuous. This has prompted some soul searching—for example:

> critiques by Schollar and Llobera, separated by almost fifteen years, share remarkably similar conclusions: computer methods rarely lead to new archaeological knowledge beyond making it possible to record and retrieve information faster and in larger quantities than before. (Huggett, 2015a:80)

Debates on this topic have tended to 'reinforce a view of digital archaeology as an under-theorised, subordinate and consequently under-valued field' (ibid). In spite of such relative pessimism, there have been many initiatives, in a fast-changing field, which have attempted to develop transformative uses of digital technologies.

2.3.1 The affordances of networks

A consequence of the nature of digital media is that they can be deconstructed into discrete parts ('packets') and transported across electronic networks, to be reconstructed at the receiver's end in an agreed format, without degradation and in near real-time. Thus, users with access to networked computers implementing the appropriate protocols can overcome previously significant geographical and temporal barriers to communication. By now, this affordance is so widely taken for granted that the fact of unequal access to technology and the means to make use of it can tend to be overlooked (e.g. Fredheim, 2020, Marwick, 2020). However, in terms of emergent ideals for archaeological knowledge construction, this facility offers promise in a range of areas.

1. Access, openness and discoverability

Discussing 'Open Access' and 'Open Data' in archaeology, Kansa suggests that 'the future of the past is increasingly open' (2012:515). According to Huggett, access to a host of free archaeological data resources 'has transformed the practice of archaeology' (2014:1). Green

summarises the values underlying the establishment and maintenance of the Archaeology Data Service (ADS)[3], a prominent digital archive for archaeological data in the UK:

> Free, open access to this data, reduces repetition in research, increases the value of data, and provides many opportunities through data re-use to combine and re-interrogate datasets, allowing new archaeological interpretations to be developed. (Green, 2016:17)

While this service is primarily intended for the preservation of excavation and survey records, we also find embodied in it the value of broadening access to the means to formulate knowledge—in this case by transcending geographical and financial barriers, and by favouring free-to-use file formats. Kansa contrasts this with the traditional models of access to research, in which 'academic publishers not only "own the past" (as presented in copyright-protected scholarship), they own much of the scholarly conversation about who owns the past' (op. cit., p502). Other immanent values in open access models are efficiency and the cumulative increase of value by expansion of available datasets: 'As more researchers deposit data, the amount of data available for cross-site, cross-cultural and cross-period research continues to grow.' (ibid). (The notion that scope for knowledge increases proportionately with the scale of data being analysed and interpreted brings its own paradoxes, which I will discuss later.) There are, however, still many issues to be solved with regard to access, including the prevalence of 'grey literature', i.e. paper-based records which are stored by local councils and are difficult to access, heterogeneous classification models, different storage formats and software standards for record creation, institutional limits placed on data access, and difficulties in searching through available data (Green, 2016, Huggett, 2012d).

Huggett takes note of the 'unarguable' benefits of the ADS in light of statistics regarding its use, related to measurable qualities such as efficiencies of time-saving (2015b:24), but also interrogates the notion of 'openness', claiming that 'with some exceptions, much of the data in repositories is only partially "open"' (ibid, p7). He details this partiality by describing a hierarchy of 'openness' ranging from viewable datasets through to data with no exclusions

[3] https://www.archaeologydataservice.ac.uk/

for reuse (ibid), illustrating the necessity for nuanced accounts when implementing or assessing technical solutions for accessing data.

Kansa, one of the founders of the American archaeology repository service Open Context[4], notes that in spite of some progress, 'Open Access and Open Data […] still largely remain at the margins of archaeological practice' (2012:499). More recently, open access has become more firmly established in scholarship, particularly in science disciplines: one study suggests that in the sphere of scholarly publications, 'while we can read around 30 per cent of the [scientific] journal articles published in 2010 without any paywall restrictions, this fraction has grown to around 50 per cent for articles published in 2019' (Heidbach et al., 2022). Whether there has been a similar increase in the *deposits* of projects to archaeological repositories is not clear: the number of listings of archive collections on the ADS website[5] shows an overall upward trend, as visualised in Figure 3, but this is not to say that the practice is common: the interviews in §4 suggest that it is still not widespread.

[4] https://opencontext.org/
[5] https://archaeologydataservice.ac.uk/about/collectionsHistory.xhtml

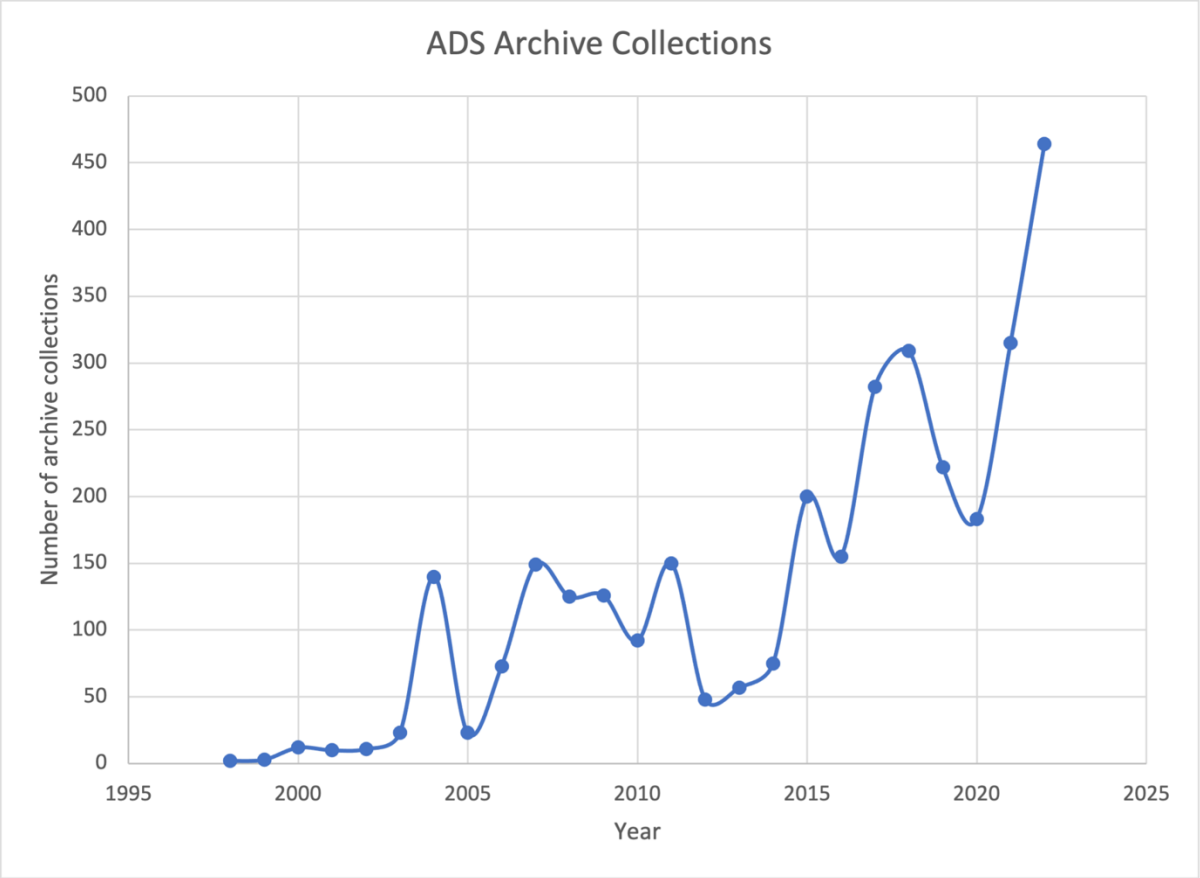


Figure 3. The number of archive collections deposited on the ADS website per year, 1998-2022.

The FAIR Guidelines (Findability, Accessibility, Interoperability, Reusability) is a set of characteristics designed to make data reusable. It lays out basic standards for the creation of content as reproduced in Table 1 (Wilkinson et al. 2016:4).

Table 1. The FAIR Guiding Principles.

The FAIR Guiding Principles
To be Findable: F1. (meta)data are assigned a globally unique and persistent identifier F2. data are described with rich metadata (defined by R1 below) F3. metadata clearly and explicitly include the identifier of the data it describes F4. (meta)data are registered or indexed in a searchable resource To be Accessible: A1. (meta)data are retrievable by their identifier using a standardized communications protocol A1.1 the protocol is open, free, and universally implementable A1.2 the protocol allows for an authentication and authorization procedure, where necessary A2. metadata are accessible, even when the data are no longer available To be Interoperable: I1. (meta)data use a formal, accessible, shared, and broadly applicable language for knowledge representation. I2. (meta)data use vocabularies that follow FAIR principles I3. (meta)data include qualified references to other (meta)data To be Reusable: R1. meta(data) are richly described with a plurality of accurate and relevant attributes R1.1. (meta)data are released with a clear and accessible data usage license R1.2. (meta)data are associated with detailed provenance R1.3. (meta)data meet domain-relevant community standards

Meeting these 'basic' standards is not straightforward however, and generally there is 'absence of motivation or incentive to reuse' (Huggett, 2018:94) (see also my review of literature related to reusability, Appendix 3), resulting in the primary problem of researchers

failing to deposit their work in open repositories. The depositing of data is not cost-free[6] and not always financially accounted for in site excavation budgets. For Kansa and Whitcher Kansa:

> Scholars only invest time and resources in organizing and sharing primary content if there is a clear and significant reward. […]
> That reward may be different for different scholars: some seek increased visibility of their research offered by digital dissemination; others seek access to data from other projects; and still others see making primary data available as a responsibility of the discipline. (2011:87)

Openness may not be a universally appropriate ideal. Kansa points out that 'Concepts about the public domain are culturally situated, and, while often useful in certain communities, these concepts are not universally applicable' (2012:509): for example, indigenous stakeholders may regard archaeological Open Data as 'a form of cultural appropriation' (ibid). Fredheim takes issue with the supposition that a more 'open' archaeology is necessarily a pathway to a more *ethical* archaeological practice, querying some of the values underlying public participation initiatives and noting that some can reinforce existing inequalities (2020). This topic is discussed in more detail in the next section.
The recommendations set out by the European Data Portal to make repositories sustainable make clear that they are social enterprises as well as data collections:

> Design decisions must balance the competing functions a portal has, between being a platform for publishing and discovering data, for example. Portal owners should be clear about their priorities and how these inform their decisions, as this will help them to manage relationships with publishers, users and funders. (Berends, 2020)

The authors emphasise the work required to 'monitor and enforce standards of data publishing quality, discoverability and timeliness', without which the portal could end up as 'a source of broken links, out of date and unused data, and poor metadata' (ibid): policies and standards for best practice are required as the 'backbone for any potential hard levers

[6] Aside from time investments, archives often charge a fee for deposits, e.g. the Archaeology Data Service: https://archaeologydataservice.ac.uk/easy/costingCalculator.xhtml

enforcing data quality' (ibid). They explore the notion of data quality in terms of Berner-Lee's '5 Star Deployment Scheme' for open data[7]. The value for access of sharing data in non-proprietary open formats (as in '3 star' data and above) is clear but I suggest that there may be a danger in resorting to 'lowest-common-denominator' formats such as CSV, in that the nuances of user intentionality in the shape of visual, organisational or interactional patterns evidenced in the originating software tools may be lost (along with the potential to understand how that software may have shaped the output). The emphasis on data as a stand-alone source of meaning is indicative of a narrow positivist perspective. This and the somewhat intimidating sounding use of 'hard levers' to 'enforce' quality, give an indication of the potential for the ideals of openness and interoperability to have a strong shaping influence on the research processes which produce open data outputs, as well as the outputs themselves, in ways which may not always have the positive effect intended—for example, with regard to values such as transparency. On this theme, Huggett points out potential problems with a lack of commensurability between records in archaeological repositories and failures to document the processes by which data have been arrived at:

> The operationalisation of data within a computer environment strips out the context of recording—or at the very least, increases the distance from it. (2014:4)

As a consequence, 'the theory-laden, purpose-laden, and process-laden nature of the data remains largely hidden' (ibid). In other words, while welcoming the benefits of open access to datasets, we should recognise the issues 'associated with recovery and recording biases' which it might foster (Huggett, 2015b:26). Kansa proposes a model of 'loosely joined, complementary archaeological information systems', rather than a 'monolithic "one repository to rule them all"' approach, suggesting that data-oriented sites could be complemented by narratives created in 'authoring venues' such as the Journal of Open Archaeological Data or Visible Past (2012:514). This approach does not solve the difficulties of enforced schema for data, and a lack of contextualising information in repositories; but it does open the way to a sensibility which understands the production of knowledge and

[7] https://5stardata.info/en/

meaning as developing in diverse interactions and productions, rather than simply as a property of empirical data alone.

2. Access and multivocality

An ethical imperative to share archaeological knowledge with the general public has been dated back to the 1950s (Wheeler, 1954:192). There have been attempts to institutionalise this (in the UK) in the form of criteria for recognition of research impact in the Research Excellence Framework (REF)[8]. Access is a prerequisite for multivocality in knowledge representation, an ideal stemming from a desire to situate knowledge production in socio-cultural contexts and reject hegemonical privilege in claims to knowing 'truth' or in the systemisation of the creation of facts. Hodder's work on reflexive archaeology was a response to the perceived need to support 'alternative voices' and reinstate context into interpretation:

> Many archaeologists have been frightened by this proliferation of voices and have sought comfort in an authoritarian archaeological science; science as objective and untrammelled by politics. […] One response in archaeology has been to erect barriers and to police the boundaries of the discipline. […]
>
> We cannot just hand over objective data to interested groups. At least some of those groups recognize that interpretation is involved in the very collection of evidence, in the laboratory itself, and at the trowel's edge. If the project responds to multivocality simply by building a visitor centre and making a CD-Rom, then the authority of archaeological science is retained. (Hodder 2000:33-34)

Hodder champions multivocality as 'an oppositional practice, capable of critically transforming archaeology' (2007:210). He argues that the voices of diverse stakeholders 'should be represented in the research agenda and the interpretation of the [archaeological] site' or that they should at least 'be provided with a platform to express their ideas or concerns' (Farid, 2015:59). Richardson articulates this as 'Public Archaeology':

[8] https://www.ref.ac.uk/about-the-ref/

> the conceptual and ethical paradigm of Public Archaeology is the renegotiation and exploration of the issues of power relations, participation, individual agency and social inequalities, through communication and dialogue between archaeological professionals and non-professional members of the general public (2013:2)

and emphasises that archaeologists should 'respect and value the interpretations of the past by non-professionals, without the imposition of their 'correct' interpretational methods' (ibid). Computing networks and the practices they enable are seen as potentially conducive to these ends, as Boast and Biehl describe:

> We write from a perspective that is informed by conceptions of knowledge as performance, of objects as citations, and of the potential of the Web as a contact zone; we identify the critical need to construct environments that support the generation and representation of knowledge in, by, and for different communities; and we evaluate the potential for the narratives, values, and interests of multiple knowledge communities to be appropriately represented with archaeological information that is created using the technologies and practices of social computing. (2011:119)

Boast and Biehl study users' remote access to a museum's collection in order to highlight the role of perspective for the production of meaning, in so doing demonstrating the lack of cultural context in official records which focus on physical characteristics and find details. Of course, simply providing internet access to records does not guarantee the democratisation of archaeological practice. Richardson warns of a 'top-down' approach to Community Archaeology that 'maintains the expert status of the professional archaeologist, which gives voice to participants supported by the validation of these experts' (op. cit., p3). In some cases careful control may be exerted over non-professional participation, preventing the practice from being 'truly participatory and inclusive' (ibid). This can be seen not just as undermining the contributions of participants expressing their own perspectives, but as stifling the transformative potential of a multivocal approach which depends on engagement with a range of perspectives:

> it is not simply the presence of social computing technologies that mattered, but the nature of the voices that use those technologies, ultimately allowing users to engage with multiple perspectives around the object. What was most apparent was the

> necessity, from the first encounter, for users to begin to create their own engagements with the objects unencumbered by excessive protocols or rules. (Boast and Biehl, 2011:138)

It has been observed that internet technologies have helped to grow 'Expectations of and opportunities for social, collaborative and individual participation and interaction with cultural heritage' (Richardson, 2013:1). Importantly:

> Professional archaeological organisations are increasingly encouraged, if not required, to disseminate their grey literature reports, publications, educational resources, data-sets, images and other archaeological informatics through digital means, frequently as mandatory outputs for impact assessment and public accountability (ibid).

The potentials of social computing have been highlighted for augmenting or supplanting a top-down distributive event-based model of publication with one which opens and maintains spaces for conversation:

> Social computing offers a space for exploring the power of appropriation and reuse of digital objects, but this must be extended to consider the ability to contextualize and engage local and vernacular accounts of digital objects from multiple communities. Future research will continue to probe these critical issues and enable digital performance to serve as environments that support the generation and representation of knowledge in, by, and for diverse communities (Boast and Biehl, 2011:150).

Cook identifies the existence of:

> a growing community of archaeologists at the intersections of digital and public practice, experimenting with the creative, flexible, and immersive capabilities of advanced digital imaging, mobile, augmented, and virtual technologies to find how to create and share archaeological knowledge in captivating and meaningful new ways that also increase multivocality and context. (Cook, 2022:143)

Invoking the concept of 'knowledge braiding', which is 'based in the value that is created through actively interweaving many truths in interpreting and presenting the past, bringing

together diverse people and perspectives in transformative processes' (ibid, p145), she maintains that:

> Taken together, paradigmatic frameworks of knowledge braiding/cabling, combined with maker-based models for sharing tools, skills, knowledge, and craft, in addition to collectivized approaches to digital and public archaeologies, offer up exciting new roadmaps for reinterpreting and understanding the past in the present. (Ibid)

Cook cites museum interventions involving communities in Victoria and British Columbia in Canada as examples of 'crafting hybrid heritage'. The interweaving of 'a complex series of digital and analog, public and private threads' (ibid. p152), including in-person events which brought together diverse stakeholders, presented complex challenges, but resulted in positive responses from participants:

> a resounding theme in student, museum, and community perspectives was the way in which the process of doing collaborative digital archaeology was as meaningful as the digital products themselves. (Ibid, p154)

Cook also notes that the use of publicly accessible digital archaeologies to confront dominant narratives and values in the discipline can open up those already dealing with systemic discrimination to further personal risk and abuse. She calls for commitment to the fostering of those digital platforms which offer 'support for more diversity in the discipline and narratives of the past', those which encourage 'inclusive and equitable digital archaeologies' (Cook, 2019:407).

Srinivasan notes, based on his long-term experience of developing models of knowledge co-production with indigenous communities, that:

> The belief systems, values, and perspectives of source communities are threatened in the digital world, where terms such as openness or participation are evangelized without scrutiny. We cannot simply develop systems, technological or otherwise, that just "average" everyone's opinion. These support mass participation rather than diverse knowledge. The perspectives and ways of knowing held by diverse communities must be considered sovereign, autonomous, and worthy of respect. (Srinivasan, 2017:119)

In considering the land burning practices of the Yolngu aboriginal people of Australia's Northern Territory, a process known as 'worrk', which makes the land fertile, he contrasts Yolngu ontology with that of visiting environmental scientists:

> Yolngu knowledge builds on specific relationships between families, lands, and practices. They are activated through practices, performances, and rituals associated with the ontology of worrk. In contrast, the prescribed burn for the sciences abstracts these practices into sets of steps and guidelines that can be enacted theoretically anywhere, independent of the specific scientist or landscape. The connected vector of people to land is absent in this scientific ontology. (Ibid, p125)

If we are to avoid the suppression of the 'knowledge traditions and practices of diverse communities', then, technologies are required which 'support the sovereignty of different ways of knowing':

> We must do away with the types of user-centered design or appropriate technology projects that give all the power to an engineer or creator far removed from the project's communities of users. (Ibid, p123)

This illustrates the ethical imperative behind treating knowledge production as a situated process, and for carrying forward the implications of this to the uses of digital tools and digital data. Drucker proposes the use of the term 'capta' in place of 'data', to recognise the active role of the researcher in 'capturing' information about the world and to indicate that they cannot plausibly act as a neutral intermediary in that creative process (Drucker, 2011). Chippindale adopts the same term, emphasising the connotations of hunting, uncertainty and risk (2000:605). For Bowker, the concept of 'raw data' is 'both an oxymoron and a bad idea', and data should in fact be 'cooked with care' (2005:184). These perspectives, like Srinivasan's, highlight the difficulty of looking to the transformative potential of improved access in isolation from data collection and presentation. In fact, web technologies have increasingly been used to gather contributions of 'crowd-sourced' archaeological content (e.g., Bonacchi and Moshenka, 2015), though this is far from being standard practice. Fredheim points out some of the negative potential of these and other public participation initiatives, in the absence of an appropriate ethical framework: 'participation deficits' based on paternalistic or patronising assumptions can cast participants as somehow deficient or,

over-simplistically, as beneficiaries (2020). He concludes that 'When considering "open" archaeology, then, we should be asking what is being opened and how meaningful this new access is' (ibid, p15). Again, this serves to emphasise that representational forms in archaeological research should invite meaningful engagement with the situated nature of their claims, and that this requirement is not necessarily fulfilled by simply improving the availability of data alone. The point is reiterated in Garstki's recommendations for the implementation of a 'critical' digital archaeology:

> A critical approach in 'the digital age' requires the reciprocal practice of creating knowledge, while at the same time acknowledging the circumstances in which that knowledge is created and enacted. A key here is thinking of technology as a process and not just a product. Processes occur within sets of intentions that are influenced directly by the paradigm(s) within which the practitioner is situated. It is therefore important to remember that paradigmatic change is not a product of technological change; they are intertwined and complement one another. A critical lens for archaeology in this case does not begin with how digital products are used but must focus on the reciprocal relationship between process and paradigm(s) (Garstki, 2022:4)

2.3.2 Qualities of digital media

I described previously the capability of digital media to be losslessly reproduced. I take this to be a defining quality of their nature. They are composed of discrete constituent parts which are representable in immutable and exact numerical form and can be passed to a reassembling interface. This numerical translatability, and the quality of being divisible and reconstitutable, is what allows them to be so rapidly transportable over electronic networks. It also presents clear limitations, as while we may have the means to encode the senses of touch and smell, mainstream computers are not yet able to reproduce these encodings in ways which approximate the original (human) experience. There are significant implications for the representation of embodied knowledge, those physical practices of surveying or excavating which are not easily communicable by textual or audio-visual means and thus tend to be learned on the job and enacted performatively. This already gives us a strong indication of how digital ontologies might facilitate the transmission of some representational forms over others.

1. Reproducibility

The multi-disciplinary nature of archaeology means that researchers create a wide range of artefacts, including databases, maps, photographs, audio recordings, tables of measurements, moving images, and so on, almost all of which are now digitizable at low cost, if not 'born digital'. Kansa observes that:

> While some researchers have long sought to disseminate such data, paper reproduction often proved unwieldy for comprehensive sharing (Schiff 2012). To control printing costs, publishers discouraged researchers from putting too many tables, plans, drawings and images into print (2012:505).

Digital reproduction drastically diminishes the significance of the relationship between materiality and the economics of reproduction, allowing in theory for the sharing of a broader range of data types, including sensory data, and significantly, more of it. The low costs and often trivial commitments involved in editing, augmenting or otherwise reworking digital artefacts make them useful tools for experimenting with alternative hypotheses and creating comparative versions in a process of negotiating between knowledge claims. This surely makes them ideal for the realisation of Pluciennik's (1999) vision of experimental texts which in their formal qualities reflect a range of situated perspectives (see §2.2.2). Richards-Rissetto and Landau point out that:

> Procedural modeling of geospatial data into 3D introduces new possibilities because we can create multiple simulations based on different data sources. […] This allows researchers to evaluate multiple different scenarios, and could potentially reveal to the public the complexities of digital 3D archaeological reconstruction. (2019:128)

Theory does not always translate into practice, however, because the conventions of publications often still reflect the limitations of printed media, even when they are in digital formats[9], and there are ongoing issues to be solved in making large volumes of data, or

[9] A perusal of openly available articles from a high-ranked archaeology journal, the Journal of Archaeological Science (https://www-sciencedirect-com.libproxy.ucl.ac.uk/journal/journal-of-archaeological-science) showed a frequent use of hyperlinking throughout articles and some offers of supplemental textual material or spreadsheet files for download; articles otherwise used a form which was directly translatable to printed publication. (Accessed 1st July 2021).

complex data representations, human-readable (e.g. Pang et al., 1997). Nevertheless, multimedia is clearly a valuable archaeological tool for 'addressing the representation problem' by 'expanding the range and diversity of performances of the inscriptions from an excavation' (Boast and Biehl, 2011:129). The success of online academic journals like Internet Archaeology[10] has made the exploitation of the opportunities presented by digital media more feasible, as in for example Williams (2008), where audio and video files and slideshow narratives are incorporated, along with a proposal for an accompanying blog.

On the other hand, a negative consequence of certain digital media qualities—reproducibility, malleability and the consequent perceived disposability of digital media—is that there may be doubts about artefacts' authenticity (Di Giuseppantonio Di Franco et al., 2018). There is also the issue of their relative lack of longevity. Green observes that 'In the analogue world, the rate of degradation of an asset is relatively slow, but digital resources can become obsolete extremely quickly' (2016:17). This is due to the proliferation of formats and standards, as well as ongoing technical innovation and revisionism in the computing and web spaces—for example in the shape of the continuous need for security updates, or for the capitalist requirement to create new markets. A perusal of the web sites of well-documented digital archaeological initiatives (e.g. Ashley et al., 2011; Boast et al., 2011) demonstrates the significance of this, as many either no longer exist, are in states of disrepair through lack of maintenance, or appear dated, technologically or aesthetically.

2. Malleability, non-linearity, intertextuality and interactivity

The database could be considered an exemplary manifestation of the 'deracination' of facts, the atomisation of knowledge into constituent, stand-alone parts, and seen, in its modern prevalence, as a prominent agent for the enculturation of a positivistic ontological worldview. This is the essence of Manovich's notion of the 'computerization of culture' which is described as a projection of computer software's constituent parts, namely data structures and algorithms—and of the computer's 'unique ontology'—onto the cultural sphere (Manovich, 2001:198). For Manovich, the database and the separation of data from narrative, are defining characteristics of 'new media', and there is a reversal of priorities

[10] https://intarch.ac.uk/

from traditional narratives, where 'the database of choices from which narrative is constructed (the paradigm) is implicit; while the actual narrative (the syntagm) is explicit' (ibid, p203). New media, on the other hand:

> reverses this relationship. Database (the paradigm) is given material existence, while narrative (the syntagm) is de-materialised. Paradigm is privileged, syntagm is downplayed. (Ibid)

This is somewhat simplistic: for many engagements with new media, the database is in fact effaced, inaccessible and used 'behind the curtain' as an enabling technology for the emulation of pre-digital narrativistic forms. However, the structure of new media has enabled new forms and altered our perceptions of previously familiar ones, and these aspects are what interest Manovich:

> Although database form may be inherent to new media, countless attempts to create 'interactive narratives' testify to our dissatisfaction with the computer in the sole role of an encyclopedia or a catalog of effects. We want new media narratives, and we want these narratives to be different from the narratives we saw or read before. (2001:208)

In the domain of archaeology, similar opportunities have been perceived for forms which embody ideals of non-hierarchical, multi-perspectival knowledge representation. Notable among these is the innovative work done by Tringham and colleagues from the 1980s onwards (Tringham, 2022) in using computer-based media to represent archaeological research and associated data. The project 'Last House on the Hill', which was manifested in various iterations (ibid), sought to surmount the limitations of conventional publication forms:

> Access to our legacies, analog or digital, are not assured, not by a long shot, so long as the de facto preservation standard through publication comprises of only the synthetic accounts of these events in the form of monographs. The sharing and the preservation of human traces digitally, coherent access to these traces for future generations […], depends on a coherent reckoning of all of the evidence streams from our practice, not just the hand-picked pieces chosen for their aesthetics or relevance to the particular assertions we make in the authoritative texts of the final field reports. As challenging as it continues to be, our discipline requires us to not be

> satisfied with a definitive accounting as told by a select few, rather to present to our current and future audiences a full, unmitigated documentary of the choices, both large and small, that led to our conclusions about the archaeological past. (Ashley et al., 2011:2)

In this project, flexible 'database narratives', hyperlinking in web documents and the resultant foregrounding of intertextuality and expanded agency in reading/writing narratives are seen as a means of escaping 'definitive accounting', to allow for the construction of 'recombinant histories' (Anderson, 2000). (Somewhat contradictorily, the quoted account of the project is singular in its perspective and is predicated on a techno-optimism which dismisses difficulties encountered in the innovative use of digital media as temporary hiccups which will find technical solutions in the future.) In an earlier project, 'Chimera Web', Joyce and Tringham make a connection between the formal aspects of a fragmentary, hyperlinked network of archaeological resources and the contingent nature of archaeological evidence and knowledge:

> We think that the experiences of navigating hypertexts, composed of fragments connected by networks of association, provides a better analogue to the process of constructing archaeological knowledge than other formats that obscure the contingency and incomplete nature of archaeological arguments. (Joyce and Tringham 2007: 229)

In the same way, Tringham perceives resonances between the formats of her archaeological storytelling projects and the ambiguous character of the data being represented:

> The idea of moving away from textual representation to a more performative, practice-based, entangled storytelling resonates with what I have been trying to achieve since the 1990s, especially in the afterlives I have composed since 2010, recognizing that such compositions provide an ideal medium for the representation of the ambiguous nature of archaeological data. (Tringham, 2022:192)

Somewhat surprisingly, in a retrospective account of the various iterations of the 'Last House on the Hill' project and the difficulties in preventing experimental web projects from joining what she calls the 'Dead Web' (becoming obsolete or defunct), Tringham comes to

the conclusion that:

> the source data, securely archived in accordance with conventional standards, seem to me to be the ultimate publication medium of a project and need to be carefully and constantly curated and sustained. (2022:197)

The user interfaces 'that disseminate the data in different genres and allow the broader public to interact with the primary sources' are described as 'afterlives': these are 'worth curating in the short term, but are less of a priority' (ibid, p197). This may be a pragmatic assessment, born from hard-won experience of the evanescent nature of experimental digital applications, which are challenging to maintain because of 'the fickleness of public opinion and the constant pressure on software developers to better themselves' (ibid). However, the suggestion that source data, through the application of 'conventional standards', can be an 'ultimate' or definitive publication medium, understates the constructed nature of the primary sources and the classifications and database structures used to order them. There is a danger, in contrasting their (highly relative) preservability with the extreme transience of web and other digital interaction technologies, of casting them as neutral, factual source material. From my perspective, the facilitation of the interactive co-construction of meaning is the primary value of projects such as 'Last House on the Hill', and there is no epistemological hierarchy which makes this performative process and its outcomes less significant than the source data used to help construct it. Arguably, the requirement of ongoing inputs or updates for the maintenance of a resource is an indication of its relevance to present concerns; while those resources which remain intact with little input might be considered inert or of diminishing relevance, rather than timelessly valuable. Tringham perhaps recognises this in her call for databases to be 'constantly curated and enhanced by a digital preservation loop' (ibid, p198); but there is no reason why the same logic should not apply to the user interfaces which facilitate interactions with that data.

The interactive aspects of computing can blur the distinction between author and reader, helping to accentuate the concept of the reader's active role in producing meaning from texts, a theoretical position which post-processual archaeological methodologies share with post-structuralists (e.g., Foucault, 1979, 2002, 2018). Manovich's claim that there is dissatisfaction with uses of the computer as a catalogue, and a desire for new and different

media narratives, implies a need for the negotiation of a space between carefully structured but definitive narratives, and the potentially disorienting freedom of a 'choose-your-own-adventure' approach. One possibility is demonstrated by Bonde et al.'s innovative online representation of a historical monastery complex (2009). The role of the author in producing meaning is foregrounded by offering a range of interactive routes through the collected evidence, based on alternative plausible narratives. Thus contingent rather than definitive chronologies are provided. Graham documents uses of digital archaeology which enable:

> the ability to iterate, to recombine, to remix, remesh, replay, replicate, reuse. If the use of computation does accelerate some aspects of practice, in this acceleration it creates spaces of possibility for other aspects. This means that digital archaeology is not an 'industrial' mode of knowledge production, but a 'craft' mode. It requires that you engage with the particularities of each situation to make 'good' archaeology. (Graham, 2020:18)

He makes the case for the uses of 'archaeogaming', and for a computer modelling approach using Agent-Based-Models (ABMs), both of which have potential to inspire 'enchantment' in theoretical engagements with the past. ABMs 'provide the laboratory we need for running different micro might-have-beens' (ibid, p25). This is a generative approach for developing hypotheses, rather than a reductionist one—for example by analysing available data about travel routes in the Roman Empire in order to posit theories about the diffusion of Roman culture throughout Britain, Gaul and Iberia. Graham frequently adopts the problematic notion of using such processes to 'raise the dead'; but in spite of this ongoing rhetorical strategy, he does issue a reminder of the need to dissociate model agents from real humans: 'Thinking of these agents as kinds of golems […] reminds us that we are dealing with fictive entities whose aggregate actions are lenses to help us study our world' (ibid). He indirectly counters the dangers of representationalist thinking (see next section) by introducing of a degree of criticality towards the value systems and hierarchies which make up his models: 'if we consider the results of these agent-based models as enchanting, we have to go deeper and ask "Enchanting for whom?"' (ibid, p42).

For Richards-Rissetto and Landau, the digitally-mediated practices of archaeological data open up opportunities for 'data intimacy', in the processes of 'datafication' (i.e. 'The creation of new data through digitization, most often but not always through post-

processing' (2019:121)). In translating archaeological data using 'slow science' and 'close readings', rather than wholly automatic means, they identify information that would otherwise have been lost (ibid, p130). They suggest that digital media supports an iterative approach that 'can lead to new archaeological questions and methodologies and enhance the nature of our scholarship' (ibid), one which they advocate over a linear 'chain' of research practice. Their approach seems to be in large part born out of the need to *compensate* for the innate qualities of digital data and the automated processes they facilitate; though the emphases on non-linearity, avoidance of 'end-products', and on the possibilities for evaluating multiple modelling scenarios (see 'Reproducibility', §2.3.2) are all positive exploitations of the digital nature of media and its reconfigurability.

3. Perspective and Embodiment

Other multi-perspective approaches have been enacted in the creation of virtual computer-based environments where avatars and/or first-person perspectives are controlled by the user, in simulated embodiments of pre-historical experiences, with the goal of cultivating 'cultural presence' (Pujol-Tost, 2017; Morgan, 2009). Similarly, virtual environments have been used to embody the immersive experience of archaeological interpretation, posited as taking place in an 'interstitial space', with the goal of expressing post-humanist ideals, in which 'material expression from the past and present can co-mingle' (Morgan, 2019:326). While these projects *foreground* epistemological concerns and seek to embody them in digital forms, the uses of interactivity are so ubiquitous in computing[11] that the epistemological commitments underlying standard interactive exchanges can often go unremarked. For Hacıgüzeller, this was the case with Geographical Information Systems from the 1960s, but 'the scholarly atmosphere began to change in the 1980s due to the increase in GIS use in spatial-analytical geography that came at the same time as a poststructuralist/modernist "spatial turn"' (2012:248). Hacıgüzeller underlines the 'scholarly constructed nature of the strong and historically stable relationship between GIS and positivism' (ibid, p246), and traces a history of critical thought which traverses human geographers' rejection of GIS in the 1990s as a 'Trojan horse that could overpower social-theoretical considerations in geography' (ibid, p249), through to later, more nuanced

[11] In fact Manovich declares the notion of interactive computing to be tautological (2001:71).

accounts which show 'awareness of the complex relationship between technology and epistemology in general, and GIS and positivism in particular' (ibid). Amongst the earlier critiques, Taylor describes the conceptual move from Geographical *Knowledge* Systems of the 1960s to Geographical *Information* Systems, as a return of 'the very worst sort of positivism':

> What does it mean to retreat from knowledge to information? Knowledge is about ideas, about putting ideas together into integrated systems of thought we call disciplines. Information is about facts, about separating out a particular feature of a situation and recording it as an autonomous observation. Hence disciplines are defined by the knowledge they produce and not by facts: a 'geographical fact' that is not linked to geographical knowledge (e.g. 'Test matches are no longer played in Dacca') is merely vernacular ('trivial pursuit') geography. The positivists' revenge has been to retreat to information and leave their knowledge problems—and their opponents—stranded on a foreign shore. (1990:212)

Of particular interest among the later critiques is that of GIS as a manifestation of 'representationalist' thinking, which can be summarised as:

> the belief in the ontological distinction between representations and that which they purport to represent; in particular, that which is represented is held to be independent of all practices of representing. (Barad, 2007:46)

This notion of a bifurcated world, where there are 'representations, on the one hand, and ontologically separate entities awaiting representation, on the other' (ibid, p49) has received significant challenges from a range of theorists, yet is 'so deeply entrenched within Western culture that it has taken on a common-sense appeal' (ibid, p48). There is an epistemological critique of GIS which views its representations as emblematic of this divided conception of reality: they 'just' represent, and have no agency. This is significant because:

> when representations are treated as purely representing, they are taken as a substitution for an independent reality, but when they are taken as performing, they are acknowledged as making a difference here and now, in various ways contributing to the production of realities. (Hacıgüzeller, 2012:253)

The fear is that GIS representations can give a misleading sense of an 'objective' reality, with their narrow focus on topography, vegetation and so on, and their apparent separation from the 'subjective' world of 'culture, the mind, meaning and the present' (ibid). (Similar concerns arose, also in the 1990s, about the aura of 'authenticity' fostered by computer-based archaeological reconstructions—this is discussed in §3.4.1.)

4. Reusability

One of the promises of networked access, digital malleability and the separation of data from narrative, is that data becomes more reusable. As a consequence of the archaeological emphasis on 'preservation by record' (e.g., Harris et al., 1993:277) due to the destructive nature of excavation, the majority of the literature in this area is concerned with the reuse of data from site excavations or surveys, rather than from 'final' interpretative outputs. The ADS suggest that 're-use of data is the single surest way of maintaining the integrity of data and tracking errors and problems with it' (ADS, 2014), which I interpret as a plea to join a quest for the refinement of a singular truth. While reuse is held up as an ideal, there are barriers to its uptake: 'Making data shareable and accessible is not the same as actual reuse', and 'Unless steps are taken to encourage researchers to take up and reuse such data, the data cycle easily stalls in the absence of motivation or incentive to reuse' (Huggett, 2018:94). For numerous reasons likely to include prestige, impact, funding and the lure of the novel, 'there remains a strong tradition within archaeology of conducting new research by collecting new data rather than reusing old data collected by others' (ibid, p96). Importantly, it has been reported that the reuse of data to test archaeological claims is relatively rare (ibid, p97), in spite of the existence of some well-known examples (e.g. Clarke's 'pivotal reinterpretation' of late 19th century excavations at Glastonbury Lake Village (Chapman and Wylie, 2016:112)). More often data is accessed to establish a baseline for research employing comparative methods.

The ideal of reuse and the apparent associated difficulties in realising it brings into focus a recurring theme in this thesis: the separation of 'facts' from the context in which they were constructed. How can knowledge which is derived from very particular circumstances, such as an archaeological dig, be made mobile so that it can be usefully applicable in other contexts? Can 'facts' be deracinated and still retain value and significance, or in so doing do they revert back to being data points without generalisable meaning—mere 'information' as

Taylor would have it? The *selection* of data deemed worthy of collection is itself a significant delimiting factor in the creation of records, one based on interpretation and built on assumptions which are likely to remain undocumented. There is an argument that the existence of data is dependent on a framing knowledge which precedes them (Huggett, 2015b:18). Collection also depends on the data being capturable in the first place, and being susceptible to unitisation (ibid). The difficulty in conveying the nature of 'capta' is in communicating the circumstances of their capture—that is, providing sufficient context. Only by doing so, it is claimed, can they become usefully reusable. This requirement has been discussed at length in the literature (e.g., Faniel et al., 2013:297), most often in terms of 'metadata'—data about the data—and 'paradata'—data about the process of recovering the data. Typical metadata conventions, exemplified by the Dublin Core specifications[12], deal primarily with authorship, licensing and discoverability. Paradata is more likely to be useful for filling out context for reuse but while certain technical processes or software algorithms may lend themselves well to close documentation, there are obvious problems in communicating embodied knowledge, tacit knowledge, or subjective, self-reflective information about process in universal or standardised terms: a paradox which may be partly responsible, along with a general lack of incentives, for the absence of paradata in the majority of documentation. Hodder's reflexive method 'replaced decisions about sampling with negotiations about priorities' (Hodder 2000:35), and attempted to reduce the gaps between retrieval, interpretation and specialist analysis, in theory avoiding universalist interpretations of decontextualized evidence. His notion of 'interpretation at the trowel's edge' was an attempt to 'discourage the idea of excavation as a mechanical process of recording objective data' (ibid, p36). To facilitate the documentation of this interpretative process of excavation, he introduced the use of research diaries and video recordings, and a networked database system:

> …so that the field and laboratory specialists can query each other's data and make comments on the provisional interpretations of their colleagues. All the different types of data, from field records to plans and drawings to measurements of lithic and ceramic artefacts to the film and diary data to be described below are available on

[12] https://www.dublincore.org/specifications/dublin-core/

> the same data base. The separate computers are linked by a hub to one central computer to which all have access… (Ibid)

Mickel notes that diaries had been an integral element of increasingly systematized archaeological methods starting in the early 20th century, but that:

> the autobiographical nature of the diaries and the concomitant difficulty of determining the truth of their content emerged as problematic for a discipline seeking a more objective scientific authority. Archaeology needed to retain this medium's ability to document diverse, acute, and detailed observations but with increasing rigor and standardization. (Mickel, 2015:302)

In the context then, of subsequent archaeological recording conventions which had become characterised by the use of standardized forms, Hodder's reintroduction of the research diary accords with:

> a shifting paradigm not only in archaeology but in anthropology more generally, wherein the success of truth-claims is predicated not on the researcher's ability to demonstrate universal applicability but rather on a clearly stated position, a situated authority, and the clear demonstration of the specific interpretive steps taken to reach a particular conclusion. (Ibid, p303)

In addition to having the function of preserving interpretive steps, the diary was seen as affording a valuable freedom to researchers at the Çatalhöyük excavation site, allowing them 'to hypothesize without certainty or citation, and to imagine the Neolithic' (ibid, p304) in ways not supported by the recording forms. The use of diaries was seen as a corrective for the fact that 'any data base (sic) is a construct' in which 'some degree of fixity and codification is necessary', and were used as a means of encouraging reflection, evaluation and the exploration of 'biases and preunderstandings' (Hodder 2000:36). Sandoval's analysis of Çatalhöyük diaries (2020) uncovers limitations in certain examples of their use: namely the failure, in accounts of how interpretations of vestiges develop, to include interpretations of remains themselves and the past; the omission of considerations of the project's research agenda; and a bias towards textual formats. He concludes that 'projects should aim to develop and adjust their own reflexive procedures, considering their particular

circumstances' (ibid, p151), rather than simply relying on the mechanisms developed at Çatalhöyük.

The inclusion of reflexivity and subjectivity into research practice can be considered as a challenge to the ideals of standardisation and codification, as it allows for content which is not necessarily translatable into the formats of formal excavation projects' research outputs (see quotation by interviewee 3, pg. 117).

Reuse is further challenged by the existence of different recording conventions, data errors and anomalies, and undocumented processes of 'data cleaning' (Huggett, 2018:98). In general:

> The meta/paradata associated with data are most commonly of value to the computational tools used to locate and manage the data rather than to the human agents seeking to make use of them. (Ibid, p99)

and adding specificities clashes with the goals of generalisation:

> different kinds of reuse may require different levels of supporting contextual detail […] and by implication, the levels of contextual information available will place limits on the kinds of reuse that are possible for a specific dataset. (Ibid)

Information is broken down into data in the form of discrete parts, to make database storage possible. Research narratives derived from the database may reconstruct the data into information, but in the process of translation, tacit knowledge and framing epistemological assumptions are lost (Huggett, 2018). As distance increases from the original context, the effect, when retrieving data, is something akin to accessing a memory of a memory of a memory. As we will see below ('Data at scale', §2.3.3), this problem may be magnified at scale. (I discuss the wider literature on reusability in §8.4.2.)

2.3.3 Computing power

1. Reconstructions and the crisis of hyperrealism and authenticity

Cheap computing power has put the creation of increasingly sophisticated visualisations within reach of archaeologists. Epistemological problems in making archaeological reconstructions have been acknowledged by the scholarly community since at least the mid

1990s (Denard, 2016), when attention was drawn to hyperrealistic reproductions and their potential to mislead (e.g. Eiteljorg, 1998). For Miller and Richards, computer models carry greater authority than paper images, and 'clinical' fixed representations can give a misleading impression of truth. They express wariness of visualisation tools which 'rarely support the display of uncertainty or "fuzzy data"' (1995:20). This preoccupation with the potentially misleading finality of realistic depictions and the potential for technological authority to create a false sense of objectivity is repeated throughout the literature (e.g. Strothotte et al., 1999, Haegler et al., 2009, Garstki, 2018). Eiteljorg (1998) makes the distinction between artists' impressions created using media which a convey a sense of authorship, and therefore impart subjectivity—such as paintings—and photo-realistic models created with computers. He makes a call for further research on how such models affect viewers.[13] There is also the counter perspective:

> ...we should never forget the magic in a good image or animation of a vanished civilization. (Kensek, 2007)

Eiteljorg, like Kensek, does not want to sacrifice 'compelling' imagery (ibid). He suggests that the inspirational potential of realistic reconstructions should lead us to make the diverse purposes of reconstructions (e.g. better understanding, spectacle) and the audience (scholar, museum visitor, etc.) core considerations in our analyses of authenticity and the uses of uncertainty. In other words, what is considered good for museum visitors may not be good for scholars. The implication is that emotional responses have no place in scholarly work, and that true understanding is a purely cerebral process. Yet as Perry points out, practitioners who ground their visualisations in research and data-based evidence 'also have a concern for artistry and flourish that pulls audiences into the visual narrative and engages the imagination' (2015:193). Conventional archaeological documentation tends to 'mask the craftwork' of the discipline (ibid), the tacit and embodied aspects. These are inherent in the production of digital media also, but failures to recognise this 'arguably makes us ill

[13] More than twenty years later, we might consider how long-term exposure to the use of computer-generated imagery (CGI—a term which highlights the sense of authorship and authority we tend to award to machines) in the entertainment and advertising industries has affected modern audiences' sensitivity to the nuances of computer mediated reconstructions and their quality of 'reality'. It seems likely that they should be able to identify changes in the state of the art over relatively short periods of time. Further research might indicate a need to make greater allowances for audience scepticism in these fields.

equipped to appreciate the genuine epistemic productivity of digital visual production' (ibid, p198).

Related to the notion of authenticity in visual digital representations is our understanding of the provenance of data. The concept of 'paradata' (Beacham et al., 2006) encapsulates the 'intellectual capital generated during research' (ibid, p2), the interventions made by the researcher in gathering the data and making transformations of it for presentation purposes. These processes, often lost or obscured in the sharing of the research outcome, and therefore considered sources of uncertainty in themselves (Brodlie et al., 2012), are deemed to be important contributors to the production of knowledge. The concern to make paradata a more visible part of the research process became part of a wider objective to increase credibility in the use of 3D reconstructions, as their potential drawbacks become increasingly difficult to ignore.

In an attempt to establish a framework to encourage intellectual rigour, transparency and reliability in the field, a series of guidelines for good practice, which became known as the London Charter, was drawn up in 2006 by a group of cultural heritage researchers (Beachem et al., 2006). This was later specialised for archaeology in the so-called Seville Principles (Bendicho, 2013). The Charter calls for the use of systematic documentation to compensate for the fact that 3D visualisation methods lack a substantial history of methodological debate, as part of a goal to communicate the 'nature and degree of factual uncertainty of a hypothetical reconstruction' (Beachem et al., 2006). Cohesion between aims and methods is prioritised—there is a need to fit the reconstruction to the audience, as noted above. In the Seville Principles we find commitments to the proper incorporation of metadata and paradata, and to making alternative virtual interpretations available, 'provided they afford the same scientific validity' (Bendicho, 2013).

Such concerns and initiatives encouraged moves towards the formal inclusion of indicators of uncertainty in archaeological reconstructions. The section in the London Charter on 'transparency requirements' recognises that 'the type and quantity of transparency information will vary depending on the aims and type of visualisation method [...] as well as the [...] level of knowledge, understanding and expectations of its anticipated users.' (Beachem et al., 2006:§4.2). This could be interpreted as being derived from the influence of

a situated knowledge approach which takes context into account, and the emergence of the principles seems likely to have been enabled by the work of contemporary post-processual theorists such as Hodder (e.g. 2000). It is telling, however, that in subsequent work related to transparency in reconstructions, any relationship to this section of the Charter seems often to have been filtered through the narrower perspective of a universal knowledge model, and 'accounting for different user expectations' is typically treated as a requirement to cater to audiences with different levels of expertise, for example academic specialists vs non-expert museum visitors. A characteristic approach to this is given in Apollonio (2016), where a table of archaeological evidence types is correlated with uncertainty scores, providing a blueprint for the mapping of data sources to levels of uncertainty. Entries in the table range from evidence derived from the architectural or archaeological artefacts or 'real' items (most certain), with accuracy therefore limited by characteristics of the measuring instruments and/or the surveying process, through to conjectural hypothetical reconstruction (least certain), made without reference to tangible documentary sources. Another typical theme is exemplified by Kinkeldey et al.'s paper on the assessment of the visual communication of uncertainty (2014), which is concerned with optimal visual metaphors for uncertainty, exploring which are the most intuitive and therefore the most effective.

In both of these examples, uncertainty is considered as a variable in a tacitly taken-for-granted universal knowledge system. The goal of the research is to make the system more internally coherent and make representations effective and intuitive, by accommodating ontological uncertainty: but there is little consideration of the epistemological assumptions of the system, the interactional processes involved in building knowledge, or of the potential for cultural variations among audiences (for example in the assessment of what is 'intuitive').

There is perhaps less scope for ambiguity in the application of the emphasis on paradata in the literature on archaeological reconstructions, again formalised in the London Charter and Seville Principles. As with the broader call for transparency, this can be seen as evidence of a humanist influence which emphasises a constructivist model of knowledge production, and as a reflection of the concerns of the post-processual movement in archaeology (Thomas, 2000). From this perspective, every description is a reflection of the archaeologist's

viewpoint and subjectivity (Jensen, 2018). For Beale (2018), the influence of the image-maker in archaeology has been greatly underestimated, and the inclusion of metadata and paradata can help us to unpick this, if only to a limited extent. In Clark's (2010) view the whole notion of 'reconstructing' the past has been detrimental to the discipline: we should talk only of 'constructions' of the past and understand them as tools for understanding, rather than statements of reality. Windhager et al., (2018) take up this theme, arguing that interfaces themselves are cultural artefacts, and that critical self-reflection is therefore required on the part of designers. They make use of the concept of 'synoptic tasks' to identify processes of active meaning-making through the finding of patterns and relationships in data. Concrete steps are recommended to optimise resources in pursuit of the ideal of critical engagement, including disclosure of data and design choices, plurality in perspectives, contingency and the encouragement of users' 'self-activation' and engagement, leading to empowerment. Cultural heritage collections are imagined as potentially dynamic entities which can be re-formed through support for innovative forms of participation, such as the sharing of user-driven narratives alongside 'authoritative' ones.

This aspiration to use computer-based visualisations for knowledge production and 'sense-making' (Windhager et al., 2018), rather than simply reporting conclusions already expressed in textual or numeric expressions, is found throughout the literature: for Gupta and Devillers this approach creates spaces for collaboration with scholars in other disciplines (2017:875), while for Demetrescu (2015) it highlights the importance of collaborative environments which make the reconstruction process transparent. This ideal is often presented in opposition to 'closed' visualisations which efface their own assumptions (and uncertainties) and do not offer purchase for critical engagement. In their web-based presentation of a 'virtual monastery', for example, Bonde et al., (2009) specifically invent strategies to disrupt viewer's 'trust' in the provided image content, with a view to fostering critical analysis rather than suspension of disbelief.

It is useful to note the separation between research approaches which try to promote transparency by incorporating measures of uncertainty into definitive knowledge models, and those which seek transparency through criticality, by, for example, arraying a range of narratives from different perspectives alongside each other. This duality between singular

and pluralist knowledge models, and their embodiment through the affordances of digital tools, is a recurring theme in this project. Interestingly both approaches seem able to find legitimisation in the formalised principles of the London Charter and the Seville Principles (see §8.5.5).

2. Data at scale

As discussed above, access to site excavation data has improved as a result of digital storage and web-based access, resulting in increasingly larger volumes of archaeological data being available to researchers for analysis. Furthermore, data capture techniques such as 'structure from motion' (Renfrew and Bahn, 2016:85) now generate high resolution models, often resulting in voluminous datasets. These factors have made the discussions about Big Data highly relevant in archaeology, and have led archaeological researchers to seek to exploit the perceived potentials of data analysis at scale.

Torrejon et al. make the argument for increased objectivity through the retrieval and analysis of *more* data about the landscape, by recording context beyond individual sites—the 'spaces inbetween' (2016). They argue for the use of standardised recording methods to 'achieve the greatest possible degree of objectivity', and for exhaustive analyses in exploring plausible scenarios:

> A possible interpretation could be presented among other plausible versions, and their probabilities should be discussed.
> In order to investigate a specific archaeological site, the knowledge about the archaeological landscape in which it is embedded is crucial. (Ibid, p239)

Alongside the reliance on a reproducible 'scientific' method and the affordances of technology to reveal latent truths in data at scale, there is an attempt here to try to moderate the abstracting tendency of a machine-managed process by extending the effort to capture context. In this case, that context is simply provided by obtaining higher volumes of data, sampled from a wider area than is standard, using the same technical procedures. This raises interesting philosophical questions about the useful *limits* of context (and the spectre of Borges's fantastical 1:1 scale map (1999)), and what might be reasonably expected of the attention span of readers. There is also the issue of the suitability of a universal recording standard for diverse sites. In the suggested approach a technological

pipeline is created for the production of interpretation, which in turn depends on the cultivation of interdisciplinary expertise. The close documentation of this process amounts to another means of providing research context, in the form of paradata.

The advocacy for greater digital literacy in archaeological practice is also taken up by Cobb et al., who similarly see increased potential for knowledge building in the use of large datasets and the application of machine learning:

> By increasing the quantity of data that researchers can access, we enable the pursuit of increasingly rigorous, fine-grained analyses and conclusions. We facilitate the ability to ask new questions that would have been impossible in the absence of sufficiently large datasets, such as broader fine-grained morphological analyses of manufacturing standardization […] or tracking conflict-zone looting. (2019)

These are further examples of the theme of the separation of 'facts' from the context in which they were constructed. Colt Hoare's motto, 'we speak from facts, not theory', is often quoted as an illustration of early archaeology's 'naïve inductivism' (Lucas, 2019:18). There has been a tendency for archaeologists to draw a clear distinction between facts and interpretations:

> archaeological data were facts and constituted the core of the discipline, while interpretations were transient and changing. Accordingly the archaeological record was seen to become 'better' as a result of the collection of more data and the development of better techniques for interpreting these data. (Huggett, 2015b:15)

I have referenced challenges to the notion of data as being 'raw' and somehow independent of research practice and bias. Is the ongoing wave of interest in the potential of Big Data a reversion to a positivist outlook, or is there something more nuanced about it, and Manovich's 'computerization of culture'? It seems difficult to see how data which is highly abstracted from its original source and combined with data from other sources, can be meaningfully subject to analysis which accommodates the post-processualist emphasis on situated knowledge. Some are sceptical of the potentials attributed to Big Data, asserting that meaning does not emerge unprompted from datasets, no matter their scale:

> However, just because a dataset is large does not mean it is representative or unbiased, and methodological issues are even more important with large and

> disparate datasets […] Indeed, Boyd and Crawford highlight the mythological aspects of 'big data': specifically that large datasets somehow offer a higher form of intelligence and knowledge that can generate insights that were previously impossible, with the aura of truth, objectivity, and accuracy. (Ibid, p23)

There are clear difficulties in making data from different geographical sites and temporal periods commensurable. Where abstractions are used, as they must be to make concordances across datasets, there is the potential that the expectations for outcomes are to some extent built into the research methodology, based as it is on classifications, established scientific techniques and shared narratives about the past. This is what Huggett calls the 'purpose-laden' aspect of research (2014:4), after the concept of 'theory-ladenness', familiar from the work of Kuhn (2012). He highlights a key paradox behind the use of open data and data at scale:

> increasing access to increasing amounts of data has to be set against greater distance from that data and a growing disconnect between the data and knowledge about that data (Huggett 2015b:13).

In Chilton's view however, the use of large datasets and the advent of Big Data does *not* inevitably mean a return to a processualist approach: in fact 'it simply continues to improve and expand the powerful toolkits that archaeologists have at their disposal':

> Amassing larger datasets does not remove the interpretive nature of the creation of these datasets in the first place: 'what makes the archaeological data speak to us, when we interpret it, when it makes sense, is the act of placing it in a specific context or set of contexts' (Shanks and Tilley 1987:104). Acknowledging the value-laden and context-specific nature of datasets does not stymie us from moving forward, but it does present a challenge—especially as larger and larger datasets are combined from multiple sources and contexts. (2014:38)

Kristiansen anticipates transformation as a result of what he describes as 'the ongoing scientific revolution of archaeological knowledge', which has created a demand for 'changes in archaeological methods and theory, some already underway, some still to be developed' (Kristiansen, 2014:12). He foresees the emergence of new interpretative models, seeing potential in areas such as network modelling for the bridging of the gap between what he calls macro and micro theory:

> What we will see is rather a heavier reliance on large datasets, whether from micro or macro studies, as exemplified by Ötzi the Iceman or the victims of a third-millennium drama in Eulau, and more complex modelling. This invites theorizing that is more integrated in actual modelling, such as agent-based modelling or complexity theory. Some will see this as a return to a more processual, positivistic approach, which may in part be true, but it is one that is also informed by critical theory about the use of the past. It will therefore be more engaged in political and ethical issues. (ibid, p25)

This highlights a possible direction for digital archaeology—the application of new computing methods within post-processual critical frameworks: though whether these are reconcilable is unclear. Larsson cautions in response to Kristiansen that 'every possibility can be misused' and that:

> we run the risk of being naïve, and political idiots initially by uncritical and unreflecting adaptation of these advantages simply because they are there and are 'new'. (2014:55)

Sørensen is sceptical of the idea of a third scientific revolution in archaeology posited by Kristiansen, lamenting the 'fetishisation of data' and contending that:

> the Scientific Turn in archaeology comes with a price that remains largely overlooked, namely that archaeology's approximation to science has produced a growing suspicion towards interpretations that cannot be scientifically proven or quantified objectively. Second, I believe that the increasing suspicion of unquantifiable occurrences in archaeology generates an unhelpful return to the ethos of letting 'data speak for itself' (Gramsch 2011, p. 52, Johnson 2011), because—as the popular legend goes—'facts do not lie' and thus become associated with 'truth'. (2017:102)

He is wary of a perceived need to 'force scientific methods onto otherwise ambiguous archaeological research topics', suggesting that this often leads to the careless use of scientific data and to a 'distorted notion of interdisciplinarity' (ibid).

3. Classification

Classifications in archaeology (and beyond) are crucial means for allowing data to be regularised and made communicable across contexts (Huggett 2015b:24); for this reason it is 'all the more important to reveal the forms, decisions and assumptions which underpin

them' (ibid). There is an argument that the unquestioning use of taxonomies can itself be a way of smoothing over troublesome knowledge. It is natural that in building knowledge we should proceed from some established axioms, or in Kuhn's term, a paradigm (Kuhn, 2012)—otherwise we would be condemned to start from zero every time we wish to make an assertion. However, there is a tendency to confuse taxonomical competence with understanding or knowledge of purpose[14]. Gero observes that archaeological writings often conceal ambiguities inherent in material evidence through the use of typologies to homogenise data, a process she names 'cleaning the data' (2007:321). (The phrase and the process is commonplace among data scientists, and accepted as a normal part of data handling.) By this account, in addition to serving a practical purpose for organising and conceptualising data, typologies offer opportunities to conceal differences and iron out ambiguity. Gero also problematises the machine-based processing of data against universal standards instead of cultural contexts, a process she calls 'machining the data' (ibid). For Boozer, it is when typologies are used to provide a 'material context for new finds', ('xeroxing' in Bell's term (2015)) and as a 'crucial medium for communication between researchers' (Boozer 2015:94) that their 'tyrannical' aspects become evident. She illustrates this with examples which show that typologies can become entrenched simply as a result of chronological precedent—that the earliest discovered sites can become the de facto 'standard' that others are judged against; and that classifications, and the images associated with them, of 'representative' or 'typical' artefacts from such sites, can become, over time, confused with reality (ibid). Such practices are by no means limited to archaeology: these tendencies are recognisable from the discourse of psychology as patterns of cognitive bias. We discard specificities to form generalities, and we notice and give prominence to things already primed in memory or often repeated (e.g. Kahnemann, 2012).

The processes of detailed, empirical work are likely to have little effect on 'xeroxing' without an associated reflexivity about the limits of existing approaches. For Clarke, 'the more fundamental the metaphysical controlling model, the less we are normally inclined to rethink it' (1973:14). The classificatory systems used in 'semantic' computer applications, that is, applications which depend on an understanding of the relationships between things,

[14] Feynman notes, for example, that an ability to *name* the birds of the forest does not amount to an understanding of their different anatomies or environmental adaptations. (Feynman, 1999:4)

tend to be based on ideas of transcendent, universal logic, which cannot easily represent metaphor-based understanding, the situated nature of archaeological research or the cultural contexts in which artefacts are produced. Semantic systems structure things in the world, creating sets of relations called 'ontologies' which can be managed by computers and processed to produce basic inferences. This is useful for search and finding homologies or other patterns in large datasets. Specialised ontologies such as CIDOC-CRM[15] are used in archaeology and other areas of cultural heritage, for example in the organisation of museum collections. There has been a call for ethnographical studies of these systems so that archaeologists can better understand the implications of their use, because while these methods are standards based, 'they remain largely uninvestigated and unchallenged', and:

> an archaeology of the cyberinfrastructures we are constructing, including the range of interrelated and interdependent technical, organizational and social aspects, is of vital importance. (Huggett, 2012c:548).

It is only logical that classificatory systems *exclude* possibilities at the same time as they make reasoning possible (e.g. Bowker and Star, 1999). Therefore, while we cannot do without them, researchers should be highly transparent about the derivation and application of the classifications they make use of.

2.4 Discussion

The literature cited demonstrates the significance of the representational modes of archaeology over the course of the discipline's evolution, and how these reflect the philosophical underpinnings of research approaches. What emerges is a symbiosis between the evidence retrieved and representational forms, resulting in research outputs which often take on the shape of a desired outcome, rather than faithfully reflecting the sometimes messy processes of evidence collection and analysis, hypothesis testing, debate and interpretation. From idealised cross-sectional drawings of excavation sites, to 3D

[15] The CIDOC Conceptual Reference Model (CRM) provides a 'formal structure for describing the implicit and explicit concepts and relationships used in cultural heritage documentation'. https://cidoc-crm.org/

reconstructions of archaeological monuments, to universal classifications and semantic ontological schema, the 'theory-ladenness' (Kuhn, 2012) of research, or as Huggett has it, the 'purpose-laden' aspect of research tools and software packages (2014), steps forwards from its expected supporting role to share centre stage with archaeological data. The stories we tell of our research are strongly chronological in nature—this, surely, is a vital aspect of our sense of research integrity, that we faithfully follow the evidence wherever it takes us, without prejudice, and at the end we dispassionately record the steps of the journey. What the examples from the literature shows us is that the research process is in fact, in many cases, a synchronous process: we embark on a project with the outcome already in mind. We may not know the detail of its ultimate contents but we can see its shape or its form, and that strongly influences the way we collect and arrange evidence and data. Even as we are digging the layers of the earth we are drawing the cross-section; even as we fly the survey drone we are drawing the map or constructing the structure-from-motion model and composing the narrative. The research may be spread out over time but it often develops from the end backwards, and therefore in one sense happens all at once.

As some of the examples above demonstrate, at times the influence of the final representation becomes too apparent, resulting in intellectual dissonance, or the undermining of the chronological narrative of integrity: for example in the case of authoritative-looking 3D reconstructions built from a limited evidence base; or where highly disparate data is overly simplified and conformed to fit to a common database scheme. For the most part, however, the form of the final output is taken as 'natural' and as a consequence of the preceding parts of the research process.

The synchronous reading of the research process, in which form is largely predetermined and has a strong bearing on data collection and structuring, makes sense in the context of understandings of knowledge-making work which sees it as a process of managing complexity. Lakoff and Johnson's work in linguistics sees the use of metaphor in this light: 'Love is war': 'Love is a journey': 'Love is madness' (1980:44-49); when concepts are too complex for straightforward human cognition, we use metaphorical concepts to represent different aspects of the concept, and these are often mutually contradictory. Similarly, projects can use classifications to master complexity and ambiguity. These are systems of exclusion as well as reasoning, and in archaeology they are heavily dependent on interpretation, as the evolution of C.J Thomsen's 'three-age' chronological system

demonstrates (Trigger, 2006:Chapter 4). Even in the case of apparently ideal 'Aristotelian' classifications, pragmatism can at times result in systems giving way to 'fuzzier' criteria based on prototypes—'heterogeneous objects linked by metaphor or analogy' (Bowker and Star, 1999:65)—which, like metaphor-based understandings, can be mutually inconsistent. The use of a representational form with controllable, built-in limits, be it a database or a journal article narrative, is a way of framing complex research evidence and findings in a manageable way. Precisely *because* of their limits, such forms entail epistemological and ontological commitments. If researchers understand the world in terms of a knowledge paradigm (Kuhn, 2012), their work consists in explaining phenomena in terms of that knowledge. I suggest that the heavily accented *ethical* aspects of research are a tacit acknowledgement of this—an attempt to create a balance between strongly predesigned outcomes on the one hand, and unpredictable and unwieldy research programmes on the other. This is one way of interpreting the gaps observed in the literature between research aspirations, and research practice. The former are often expressions of ethical or moral imperatives in research, for example, 'openness', 'reusability', 'inclusivity'. The literature suggests that true 'openness' is only marginally extant in archaeological research (Kansa, 2012), that reuse of research resources is spoken of often but takes place infrequently (Huggett, 2018), and that inclusivity in the documentation of museum collections mostly happens only at the most superficial of levels (Boast et al., 2007). I suggest that where such gaps exist they are at least in part a result of the tension between the perceived *ethical* requirement to hold research work to account, and the affordances of final forms which lend shape to the overall undertaking—shapes which tends to emphasise other values: narrative inevitability and closure, epistemological 'obviousness' or 'naturalness', authority, dispassionate objectivity.

Latour notes the possibility of delegating morality to technology (2021). He cites the example of the special keys used to lock Berlin Treppenhäuser (shared staircases for tenement flats). The goal to keep the doors to the street locked, in order to prevent graffiti or antisocial behaviour in the shared stairwell space, is achieved by an ingenious design (still in place in many buildings) whereby any visitor, after inserting the key and unlocking the door, is obliged to lock it again from the other side, in order to retrieve the custom double-ended key from the lock. The requirement to 'do the right thing' is therefore delegated to the technology, relieving humans of choice or responsibility. The lesson is that researchers

need to carefully attend to the moral or ethical agency inherent to the software packages and other technological tools which they employ in their work.

2.5 Areas for further research

There is a body of literature in Digital Archaeology[16] which calls into the question those uses of computing for archaeology which are simply about doing the same things, but faster and with more data (Huggett 2015a:80). Frustration is expressed with the lack of any truly original, transformative practice born out of the affordances of technology (Llobera, 2011) and with the 'fetishization of data' (Sørenson, 2017). At one level these complaints are rehearsals of the processual/post-processual debates—which seem to have never really gone away—but reviewed through the lens of technology enhanced practice. This is due to the compatibility between a positivist ontology and the primacy of the database in computer-based media. Manovich calls for the 'computerised culture' to produce new media narratives which break from tradition (see §2.3.2), and some of the examples of archaeological work cited, (e.g. Ashley et al. (2011), Boast et al. (2007), Bonde et al. (2009), Morgan (2019)) show a similar impulse from advocates of the new possibilities of Digital Archaeology. But such initiatives are the exception rather than the rule. For Llobera:

> Despite their long presence in archaeology, the impact of computer applications has been surprisingly limited […] i.e., they have not been part of any radical departure in how we conduct archaeology. […]
> We are able to record information much more quickly in the field but to what degree is this "new information"? How much has it changed the way we conduct our analysis? We have the capacity to process and visualize information in novel ways but are we actually doing this? More importantly, are we even thinking about new possibilities? How do these new developments relate, if at all, with theoretical orientations currently found in archaeology? Has the introduction of information systems precipitated new ways of doing archaeology? (2011:217)

[16] The distinction between 'upper case' Digital Archaeology, which consists of reflection on the uses of technology in archaeology, and 'lower case' digital archaeology, which is the everyday use of digital tools in the discipline, was made by interviewee 8 in Study 1, §4.

The literature shows that since Llobera's comments in 2011, researchers *have* been thinking about these new possibilities, at least at the edges of the discipline. The projects listed above, for example, recognise that digital tools have particular affordances, and seek to exploit these in new ways. What is often lacking, however, is a detailed analysis of the epistemological commitments of digital tools, in the way for example, that Lucas attempts to develop an understanding of the epistemic virtues of narrative types in archaeological texts (see §6.4). Do digital tools and forms have intrinsic epistemological qualities, or are these a product of culture and context? Can they be reconfigured to different ends than those we have learned to expect of them? To explore these issues seems like a good fit for a discipline which is highly preoccupied with material culture and with making interpretations of the functional and cultural significance of human tools and other artefacts. In the rest of this thesis I will seek to develop an analytical understanding of the epistemological and ontological commitments of the tools used by digital archaeologists, with the aim of better understanding the gaps between theory and practice which have been recounted in the literature. In particular I wish to explore what a digital archaeological practice based on the assumption that *knowledge is situated* might look like, and whether it can be successful in fostering the research virtues of transparency and reflexivity. While it may not be always stated explicitly, it is often dissonances between universal and situated knowledge models which underpin much of the disquiet in the literature about the perceived failings of current digital practices in archaeology. What tends to be missing is any kind of detailed account of how to remedy these failings.

3. Methodology and Methods

3.1 Methodology

My theoretical approach depends on the assumption that the knowledge systems we construct to make sense of the world are both specific and manifold, rather than universal—that they derive from ongoing negotiated processes between inherited belief systems, local social-political influences and our interactions with the material world. An important implication of this approach is that there is no place accessible to us outside of our experience from which we can 'move the world' by the leverage of our logic or understanding: and equally we cannot look to conjure representational forms in an epistemologically neutral place.

The basic assumption that knowledge is contingent and non-universal, is what both justifies this project and guides its lines of enquiry. Its corollary is that for research to be robust and transparent, it must address questions of its own structural assumptions and their limitations alongside the detailed study of its object. The methodological approach adopted grows out of this theoretical ground.

With the goal of exploring the nature of local and contingent knowledge systems, I take a perspective which assumes that while there is a human tendency to understand history and the production of meaning in terms of anthropic determinism, change and actions are a product of networks of agents, both human and non-human. In this I draw on Actor Network Theory (ANT) (Latour, 2005), looking at the interplay of agencies and their mutual influences, and after Latour, trying to understand why some configurations of these are more successful than others, allowing certain knowledge models to gain acceptance, and to translate to different contexts—to be more 'mobile' in his terminology (Latour, 1987, 1996). This 'symmetrical' view is also important for those branches of the discipline of archaeology focused on material culture, and therefore makes up a significant strand in the study of archaeology itself (e.g. Witmore, 2007). This perspective is significant to me personally because of what I see as human failures to account for the significance and value of non-human agents, and a flawed conception of humanity as existing in separate, privileged echelon from nature; factors which together contribute to extremely negative

consequences in terms of shared environments, justice and equitable access to resources, including knowledge resources. McLean and Aroles suggest that:

> authors from an ANT perspective would argue that dualisms such as structure/agency, social/material, social/technical, nature/culture, should be viewed as outcomes in a constant process of becoming, rather than as starting points that need additional separation and clarification. (2016:62)

In my own research I follow the ANT emphasis on the 'social' as being a 'constantly becoming' outcome of a web of relations, rather than an independent body of influence. At the same time I do not see the landscape of social relations as being a flat one: just as Latour finds that some models of knowledge construction are more 'mobile' than others, I will explore the notion that knowledge mobility is easier in some directions than others—that there are paths of varying resistance, that certain agents are more reactive, or have greater 'valency' than others, and that inertia is a feature of existing relations and new ones. While the collective profile of this varied landscape is commonly understood and referred to as the 'social', I will find it more useful to conceive of this in terms of evolving sets of relations, translations and mediations.

My methodological approach is influenced by other strands of critical practice, though I apply their lessons only where they are compatible with my main critical framework. Critical Realism (CR), for example, sees knowledge as a 'social and historical product' (Robson, 2016:31), giving it some common ground with ANT and moderate schools of constructivism (Maxwell, 2012:ix), and making some of its methods relevant for my research (though see §1.2 for clarification of terminology). The CR perspective brings the concept of the 'intensive' study, which is one designed to answer questions, generally about a small number of cases, in a subject group linked by a causal theme (e.g. shared occupation) rather than abstractly via a taxonomical similarity (e.g. age). This approach complements my focus on local agential networks when working with material garnered from interviews and observations. Danermark et al. point out that social science does not have access to experiment, the ability to 'close the system' (2002:167) by exercising control over the variables under study. The CR response to this limitation is to make use of abduction, a form of logical reasoning identified by Pierce (ibid, p89) which consists of iteratively developing

the context or case in which the studied outcome becomes possible, in a process of balancing empirical data and well understood areas with hypothetical mechanisms and their predictive capacities (Bazeley, 2013:336). This is how the generation of explanatory mechanisms using empirical data recovered from 'intensive' procedures takes place. Abduction is pragmatic rather than formally logical, in that it can offer plausible interpretations rather than be used to derive definitive causation. For this reason it is used in qualitative studies where 'the goal is to generate theory rather than to generalise from a sample to a population' (ibid). As a consequence we can say that for the critical realist, knowledge is always fallible, and is never definitive: CR is 'ontologically bold but methodologically cautious' (Danermark et al., 2002:203). This makes some of its methods highly appropriate for research which recognises and probes at the limits of knowledge in archaeological (and any other type of) interpretation, *and understands knowledge construction to be a contingent process*. This is, furthermore, a vital feature of the programme which I will develop in this thesis, of 'Epistemological Modesty' (§7), which advocates for the most plausible explanations for phenomena, while accepting that a truly 'objective' or certain knowledge of them is unattainable, and that the possibility of alternative valid accounts exists; it is a more explicit commitment to the idea of 'constant becoming' attributed to ANT by McLean and Aroles (op. cit.), turning it consciously towards the research process itself as well as its objects of study.

Critical Realism diverges from ANT in some fundamental respects. One of these is in the understanding of social structures. In CR, these are viewed as multi-layered, stratified, relational, and existing in pre-structured contexts (McLean and Aroles, 2016:61). Structure and agency have ontological significance, and they are understood primarily in terms of human action. As previously discussed (§1.2), in ANT the 'social' is actually a symptom or side-effect of complex processes of translation and mediation, agency is not limited to humans, and it is distributed across largely non-hierarchical relational encounters. In the thesis I am committed to the overall methodological approach of Actor Network Theory and its focus on the relations between agents over CR's conception of a transcendental ontology. However, I see the uses of 'intensive' studies and abduction as compatible with the work of building knowledge 'from the ground up' (Latour, 2005) through the application of an ANT perspective and a concern with relational processes; and suggest that such

practices are to be found in key ANT texts (e.g. Latour 1996), even if they are not explicitly named as such.

Like ANT, Critical Realism emphasises 'the essential role of context'; it also legitimises seeing 'individuals' beliefs, values, motives and meanings as causes' (Maxwell, 2012:38). In my research I emphasise the aspects of these which are consequences of participation in professional, institutional and technological networks, and the commitments these induce, some of which are less susceptible to being empirically recorded than others. When considering the roles of individuals' values I draw on the work of Fleck, and the notion of 'thought-styles' which he brought to the analysis of science workers as members of 'thought communities' in their 'generation of scientific facts' (1981) in order to examine the extent to which the professional disciplines and sub-disciplines shape the assumptions, working practices and goals of researchers. This is analogous to Knorr-Cetina's notion of 'epistemic cultures' (1999). An abductive approach is followed for arriving at explanations, and this is based on data acquired from the analysis of interviews with archaeologists and other academics, using a process of coding, and from descriptions of observations of archaeologists in working meetings and workshops.

I describe my experience of making observations using 'thick description' (Geertz, 1977), in recognition of the fact that 'an account unfolds in the course of an engagement whereby things reveal something of themselves' (Witmore, 2020:2), and in keeping with the theoretical assumptions of the research, which do not allow for an objectivist separation of observer and observed. 'Thick' description, a concept developed by Ryle and extended by Geertz (Given, 2008), refers to the incorporation of details of context, and of people's public behaviour in those contexts. This mode incorporates:

> the cultural framework and meanings of the actors, their codes of signification, providing an emic account grounded in the actors' cultural context; thick description is thus the essential activity of ethnographic research. (Ibid, p880)

For Denzin, 'thick' description has the following features:

1) It gives the context of an act;
2) It states the intentions and meanings that organise the action;
3) It traces the development and evolution of the act;

4) It presents the action as a text that can then be interpreted.

(Denzin quoted in Ponterotto, 2006:542)

There are clear parallels to be drawn between the knowledge-building activities of the interviewees in their archaeological work, and those of the researcher-interviewer. In both cases evidence is used in the construction of hypotheses, under the influence of cultural and social structures and personal and other agencies. Both attempt to produce something substantial from incomplete evidence, within the constraining bounds of an episteme which cannot be fully knowable in its assumptions. I attempt to develop these parallels throughout the project, reviewing my own practices against the same measures used to review those who were interviewed and observed. The use of 'thick' description makes possible 'thick' interpretation, without which reports 'lack credibility and resonance in the research community…': this interpretation 'brings readers to an understanding of the social actions being reported on' (ibid). In adopting 'thick' description I aim to site my own research process in specifics and highlight (and question) my own interpretative perspective. Interpretation, as Danermark highlights:

> is dependent on the researcher's earlier experiences, her theories, frames of reference, and the concepts she uses in the interpretation of the studied object. Together this constitutes what Hans Georg Gadamer calls 'prejudice or pre-judgement'. This concept has affinities with 'hopeful conjectures' (Popper), 'scientific paradigm' (Kuhn), and 'general background theories' (Feyerabend). (2002:159)

The need for a reflexive hermeneutics is also addressed in part through the presentation of parts of my writing process and findings in a custom web application derived from the practical software development part of my project (see Appendix 5).

3.2 Methods

For Law, when making sense of the 'messy set of practical contingencies' involved in producing knowledge:

> we need to find ways of living in uncertainty. The guarantees, the gold standards, proposed for and by methods, will no longer suffice. We need to find ways of elaborating quiet methods, slow methods, or modest methods. In particular, we

> need to discover ways of making methods without accompanying imperialisms. (2004:13)

This helps to point out a potential contradiction in this thesis. In writing about research and developing a proposal for virtuous research practices, there is the danger that I might site *my own* research work in a different space from the practices I am studying: a space which is not only somehow separate, and immune to the same critiques, but potentially fails to embody the proposed values. I have had to accept that this project is one of discovery, of testing ideas and developing hypotheses, and that over the course of the project I would not have access to a fully formed set of practices which epitomise the *conclusions* of the thesis. In attempting to meet the requirements for a PhD thesis, I have also been to some extent constrained in the types of methods I have been able to adopt. Keeping these factors in mind, I have tried to be self-reflexive and to put my research *methods* into the context of my research *themes* wherever this was practical and not burdensome to the reader.

The 'intensive' process employed in this project consisted of a combination of semi-structured interviews and observations of archaeologists as they planned, organised and discussed their research. The use of interviews and observations fits with a methodological position which does not seek to establish definitive causation but looks for evidence to support the generation of plausible theories, with judgement made on the basis of theories with the best explanatory power. It builds upon the notion that knowledge building emerges from the individual's relationship to material contexts, local agencies and the historical practices which happen within thought communities. The use of semi-structured interviews allows for the investigation of 'social' and cultural influences at broad and narrow levels through discussion of beliefs and professional practices. (While the term 'social' is not uncomplicated for the ANT scholar, it is recognised that for interviewees 'social' forces are often understood to be tangible factors in their research.) It is assumed that while there may be broad correspondences between the research standards of different academic communities, the particulars of any given professional approach are culturally and historically specific. The use of interviews allows for the relationship of interviewees with such consensual identities to be explored, and observations help to give insight into the tacit assumptions of those working in a particular professional field. Archaeology in particular is a highly multidisciplinary subject, and I was interested in exploring these questions from a

range of perspectives, through the experience of different specialists, looking at all steps in the pipeline of knowledge production and how they interrelate. This was less possible in the observation phase than I had hoped due to Covid-19 restrictions at the time of research: but on the positive side this resulted in a more in-depth study of an archaeological specialism which has the use of digital technologies deeply ingrained into its practices.

3.2.1 Research participants

Archaeologist interview participants (Study 1)

In approaching archaeologists to take part in the research interviews, my main concern was to secure a cross-section of experience and specialisms, in order to get an overview of the discipline. I sought to include participants with specialised experience of the main themes of the research, namely digital research practices and their epistemological commitments, and representational modes in archaeological research documentation. The London Charter and Seville Principles publications (see §2.3.3) and their recommendations provided a useful point of entry for the introduction and discussion of these topics. I was able to find participants from the fields of academia, commercial archaeology, the museums sector and the archaeological repository sector. The process involved inviting the participation of archaeologists who had published on relevant topics, as well as more generally circulating the opportunity to take part throughout those UK networks of archaeological professionals which were accessible to me through my involvement in heritage studies and my former position of museum employment. Some of the approaches made resulted in recommendations of other archaeologists, rather than commitments to participate. I sought to attain a balance of perspectives across the main disciplinary specialisms, but to limit the numbers of participants to a manageable amount, bearing in mind the commitment in my project to a range of studies, including a software development project.

There were limitations to the recruitment process: ideally I would have been able to interview a selection of professionals based on an even distribution of role, experience, institutional membership and other factors, but as participation was based on availability and goodwill, that was not feasible. In spite of this I was able to attain access to a wide range of opinion and experience, and I persisted with my recruitment process until I felt this requirement was satisfied.

The primary scope of this research was the mainstream of archaeological practice in the UK. The lines of such a category are blurred, particularly in the context of international journals and online resources which are available to all those with web access and language familiarity. I interviewed one archivist who was based in the US rather than the UK, but whose work was sited in an international context of English-language academic ideas and publications; I also interviewed an academic based in a European country who had previously worked in the UK and had published to international journals. However, my goal was to understand the nature of research claims in the well-established institutions of UK archaeology and to look at the processes used to bring them about. My recruitment process, proceeding as it did through the channels of high-status universities, museums and companies, was successful in finding representatives of my area of study. I did not directly access the perspectives of indigenous groups, amateur archaeology groups, or citizen scientists. The themes of inclusion and multivocality which form an important part of the thesis are therefore explored indirectly via the writings of feminists, post-colonialists and others who actively challenge the status quo in archaeology and problematise its historical narratives. While one of my key areas of interest in the thesis is the handling of the issues of inclusion and multivocality by hegemonical institutions, and my perspective is one at the very edges of the archaeological community, it should be acknowledged that I write about established archaeological traditions and institutions from a position firmly within the walls of their fortified academic edifices.

My commitment to anonymising research interactions (see §3.3.1) limits the amount of detail I can provide about participants. However, one consequence of the focus described above was that all of the interviewees in Study 1 were well established in their careers, often in positions of authority and the authors of substantial bodies of published work. They were in a position to influence others in their fields to varying degrees, either directly or via the example of their research interests, methods and publications. They were, without exception, enthusiasts with a passion for their subjects and had a detailed grasp of practice in their specialisms. While in a few cases academic participants' research interests were strongly oriented towards critical practice, and challenging the disciplinary status quo, all had nevertheless achieved a level of authority and status in recognised institutions.

Observation participants (Study 2)

In the observation phase of my project, Covid lockdowns put a severe limit on social interactions and on opportunities to shadow archaeologists at work. I was therefore extremely fortunate to have access to an archaeological project producing digital research outputs and conducting its meetings and workshops in the online domain. This opportunity came about through word of mouth after enquiries in UK academic and commercial archaeological networks. The project was run by a high-status institution, directed by an archaeologist of long experience who is a recognised authority in his field. The project made extensive use of contemporary research technologies, and the project director was highly aware of both opportunities and possible pitfalls in using digital research practices for collecting evidence and representing knowledge claims, making it an ideal object of study.

Software project participants (Study 4)

To test and discuss the features of the Orson software (§8) and how they related to research practice, I conducted a series of interviews with university students. The opportunity to take part was advertised in university buildings and via circular emails by lecturers in a range of university departments across the UK, and in a guest presentation I gave to a cohort of university undergraduate students studying cultural heritage. Participants were each paid a £15 fee to take part, and this was advertised in the posters and emails which were circulated. The decision to invite students to participate directly was a pragmatic one, following an unsuccessful phase of inviting numerous archaeology professionals and educators to deploy a test instance of Orson for use in their own research, or as part of their teaching programmes with students. While there were some positive responses to this proposal, no commitments to participate were forthcoming. Ideally, the discussion sessions would have been conducted with professionals who had experience of 'real-world' research projects using digital tools and workflows; in lieu of this, I still found it extremely useful to discuss Orson with aspiring researchers who in all cases, it turned out, had well-developed research practices, and clear ideas about the conventions of research in their respective fields. A significant proportion of them already had some professional experience. The fact that participants were students naturally had a bearing on their perspectives on research, and on the status of their relationship with me as an interviewer, both of which factors I discuss in my report in §8.6.

Out of the 14 people finally interviewed, ten were studying in the field of cultural heritage, one was a researcher in an ethnographical study, another was a cultural studies student, and two were biochemistry students. Ten of the interviewees were Masters students, three were recent graduates and one was working as a research assistant. Four had previous professional experience in their field.

Those who volunteered to take part did so of their own volition; there was no requirement or expectation of participation as part of any course work, and no academic credit for doing so. The main possible motivations for volunteering then, were an interest in the subject area of research documentation, and the fee for participating. During the interviews none of the participants gave the impression that they were taking part only for the sake of the fee (for example by being unopinionated or uninformed on the themes discussed).

The participants' interests made them receptive in general to the notion of using digital research tools. One student commented at the end of the session that he had put himself forwards partly because he had aspirations to become a PhD student himself, and wanted to see an example of PhD research in action, though as a cultural heritage student he was already motivated to take part. Two others mentioned their own efforts in making small web applications, and expressed interest in and appreciation of the Orson software at a technical level. For others the cultural heritage theme of the project was relevant to their area of study or they had an interest in using digital media for their own research, or as an object of study itself. Taken together, these interests meant that participants were generally predisposed towards exploring the ideas presented in the session. This was acceptable, as the primary audience for Orson is an academic, digitally literate one; and a predisposition towards the research theme did not translate to a lack of discrimination when assessing research tools. If anything, the opposite was likely to be true, as participants were asked to consider the use of such tools in scenarios related to the details of their own subject areas and research processes, themes which they had already contemplated in some depth.

3.2.2 Interviews

Study 1 – Archaeologist interviews

The first set of interviews, with archaeology professionals, was designed to gain an understanding of archaeologists' working practices, as well as their values and motives. Almost half of these were conducted face to face, the rest using video-conferencing means.

Particular attention was paid to the roles of representational techniques and digital technologies, and to archaeologists' understandings of facticity in their work. The questions related to interpretation and subjectivity were framed in part via discussions of the London Charter (Denard, 2016) and Seville Principles (Bendicho, 2013), as these were likely to be familiar initiatives to most participants.

12 separate interviews with 10 individuals were conducted and transcribed. (Two individuals were interviewed twice.) The interviews were a mixture of face-to-face and video-conference meetings. Each interview lasted between 50 and 60 minutes. The transcribed interviews were thematically coded using qualitative analysis software (see Methods, §4.2).

The semi-structured approach was useful for shaping the overall narrative of interviews and making them comparable and susceptible to analysis. At the same time its flexibility allowed for the depth of individual experiences and priorities of interviewees in diverse roles to come through (Robson, 2016:269), and, vitally for the analysis of agency, captured the chronological and contextual connections in participants' accounts. This context would have been largely lost in a fully structured interview or in a questionnaire format, and the topics under discussion did not in any case lend themselves well to simple or neat text-box answers.

'Open' questions (Robson, 2016:275) were used wherever possible, and neutrality in questioning was strived for. The questions were designed to capture information about participants' practices, the professional contexts of their work, and their beliefs about research methods. Some prompts were scripted after each interview question to encourage discussion when required. The template script for the semi-structured interviews, together with rationales for the questions, is shown in Appendix 1.

Study 4 – Software project interviews

The second set of interviews was with students and researchers, to solicit feedback and discussion about the software project 'Orson' (§8). I conducted a series of 14 remote, one-to-one video-conference interviews with university students, over the course of a month. Each meeting lasted between 50 and 60 minutes. These included screen time spent exploring tasks using the Orson application. Interviewees were given the URL of a web-based instance of Orson, and login details. They then shared their computer screens with

me while performing some simple tasks in Orson, and discussing scenarios for its use. Each session was recorded, and the audio in the video recording was later analysed thematically using codes in the Atlas.ti software. The goal of the interviews was to gain insight into the agency and affordances of the Orson software package, and its relevance to participants' research practices, along with that of any other related software tools and processes for documenting research which might come up in the discussion.

A semi-structured approach was taken to the interview process. The interviews combined hands-on use of the Orson software with discussion. The exploration of the software's features was designed to inform discussions later in the session of scenarios involving the digital documentation of research and the sharing of that research in a web context. Less than half of each interview session was devoted to hands-on activities with the software; the rest of the time was devoted to the discussion of theoretical questions and hypothetical scenarios (see Appendix 4). The diverse nature of the participants' experiences and academic backgrounds meant that flexibility was essential in the interview design if context and its implications were to be adequately captured.

3.2.3 Observations

Observations were made of an international archaeological documentation project over a period of 8 months. Due to the limitations imposed by Covid-19 safety procedures, all observations were conducted using video-conferencing means. This had a limiting effect on the retrieval of certain types of contextual information, but in other ways was perhaps more revealing than physical attendance at a research project would have been. In particular it was possible to observe the project over a long period of time and to gain an in-depth understanding of team structures. The international nature of the project team meant that online meetings were a natural fit for its ongoing management, though it was clear that site visits with partners which would normally have taken place were curtailed by pandemic restrictions. By good fortune the archaeology workshops I observed online were on the topic of the interpretation of visual data, primarily satellite and aerial photography. These were well suited to communication via video conferencing and allowed for close-up insight into the use of software tools, through the use of screen-sharing. The workshops were highly instructive because their primary function was to induct inexpert users into good interpretative practice. This made them useful for gaining a picture of professional

expectations in this specialised field. Observations were audio-recorded, with the permission of the participants, and transcribed. Videos of the workshop were recorded and shared by the team, and I had access to review these retrospectively.

In a longer-term project and one with more intimate access to its object of study, there may have been potential to develop the observations of archaeologists into a broader ethnographical study of their 'digital lives', and the contributory effects of, for example, social media interactions, or computer-related skills and interests developed outside the formal work of archaeological research (e.g. gaming, digital imaging), to the 'reasoning style' of research practice. Due to my relatively limited access, and my commitment to a series of studies representing distinctively different perspectives, I chose not to follow this path.

Without doubt, my observations were, in Huggett's phrase, 'purpose-laden' (2012:4). As Law puts it in the context of the sociology of science,

> it is not possible to make observations of nature in a neutral way. Instead, what scientists observe, and how they observe it, is always tied up with their paradigm. (2004:44)

I have tried to be honest about my selective attention in observations towards those themes which are relevant to my interests: I have also tried to be honest and balanced in identifying themes which confounded my expectations or resisted easy categorisation or explanation.

3.2.3 Coding

For the analysis of the transcribed interviews, a coding process for identifying salient themes was used, which was applied systematically across all of the available material.

The coding process was an iterative one, involving:

1. Generation of initial codes. While some codes were inspired by hypotheses which the interviews were designed to test, (the 'etic' perspective) this was otherwise done inductively through interaction with the data. This stage was revisited throughout the process to augment and refine the set of codes, in light of subsequent interpretations.
2. Classification of codes into themes. Where these had mutual relevance or were synonymous, they are added to a 'code group' or theme. See below.

3. Exploration of relationships between themes using network diagrams and tables was created using visualisation tools in the qualitative analysis software package Atlas.ti[17].
4. Interpretation. Based on the previous stages, interpretation of the material was made via the creation of plausible narratives. To assess the quality of the analysis, this process took into account checks for representativeness of the data, the influence of researcher effects (as much as awareness of this is possible), and the replicability of findings.

Maxwell's (2012:109) distinction between categorizing and connecting strategies in data analysis was useful in this process. While the coding and tabular analyses were based on similarity relationships in the data (categories), the interpretation of the network diagrams and the creation of other interpretative narratives were based on contiguity, a 'real connection […] that is *not* a matter of similarity' (ibid). These two strategies were mutually supportive in the analytical process.

I used Atlas.ti to check for recurring themes based on similarity and recurring correspondences between codes, and to create connected visual 'networks' for the exploration of connections between parts of individual accounts, in order to explore causality in context.

Code frequencies and correspondences were used to describe generalised empirical patterns and identify systematic differences in the content, but not as evidence for induction or to establish a hierarchy of importance.

Robson states that:

> …for those […] who wish to persuade scientific or policy-making audiences, there are ways in which qualitative data can be dealt with systematically. (2011:466)

I found that taking the time to develop a systematic approach and go through the lengthy coding process greatly increased my familiarity with the content of the interviews and observations, and revealed details which might otherwise have been missed. It was also invaluable for revealing connections between individual interviews and observation

[17] https://atlasti.com/

sessions.

3.2.4 Codes and theme allocation

In ‘grounded theory’, predefined codes and themes are eschewed in favour of the identification of emergent patterns in the material—the idea being that the research themes should be ‘grounded’ in the evidence alone (Robson, 2016:161), and that evidence should not be shoe-horned into preconceived conceptual frameworks. My selection of codes reflected prior hypotheses and thus was not ‘grounded’. This reflected a desire to take into account the influence of historically developed research conventions, and of ‘black-boxed’ research procedures and tools: in short, factors which are not always easily available via the everyday empirical experience of the world. However, I also took note of unanticipated topics which frequently cropped up in the research, and coded these. This was particularly useful for revealing detail about the peculiarities of the interviewees’ processes and the social contexts which were relevant to their work, for example access to physical or knowledge resources, technology use and funding. This was consistent with an approach which registers the importance of both mediatory relational mechanisms and individual experiences of their consequences for making sense of reality. Following on from my methodological concerns, I looked for transformative interactions in the relations between things. I emphasised the close documentation and analysis of those agential forces which were apparent, and could be interpreted as influencing the shape of archaeological knowledge work, going beyond deterministic accounts of individual agency to consider how knowledge claims emerge from institutional structures, academic traditions, tools, and representational techniques, as well as the humans who practice archaeology.

3.2.5 Article analyses

My aim in studying archaeological journal articles was to approach the construction of archaeological knowledge from an angle which was distinctively different from the other studies I had undertaken, and to home in on the tangible outputs of the pipeline process of research, the ‘inscriptions’ (Latour and Woolgar, 2013) which resulted from the ‘translations’ of field work and other processes of collection and analysis (see §5.2.6). My approach was to treat these texts as they were presented: stand-alone repositories of

knowledge, presenting claims which transcended the circumstances of their production. For that reason, and following Foucault (1979), I was not concerned with the authors' *intentionality*, or the context of their production beyond that set out in the content of the works. This recognises the role of the reader as co-creator of texts and rejects the notion of a definitive meaning, owned by the original authors. This was to be a purely textual analysis, and the claims of texts were to be judged on the rhetorical and representational qualities of the medium, and the strategies used for trying to make locally gathered evidence into universally useful understandings.

The archaeological journal articles I analysed tended to be centred on quantitative data and descriptions of the manipulation and interpretation of this data. As my concern was not to validate this data collecting and processing work, but to explicate the significance of its formal features, my own analytical approach was a qualitative one. Robson sets out a typology of qualitative analysis thus:

a) Quasi-statistical methods;
b) Template approaches;
c) Editing approaches; and
d) Immersion approaches

(2011:457-458)

A statistical content-analysis approach analysis was not appropriate in this case, as my interests were quite narrowly focused on the uses of evidence for making knowledge claims, and the steps involved in such a transformative process. An unstructured analysis without the use of codes was also unsuitable, as my areas of interest were fairly clearly defined in advance. My process had most in common with Robson's 'Template' approach (b): I sought to identify patterns in the text, and used these as templates for data analysis. This consisted of creating a short list of codes for describing the *functions* of the sections of content in the articles. I then checked on the internal consistency of the arguments advanced across these different template sections, and tried to apply interpretative frameworks for understanding their selection, configuration and presentation, namely considerations of research community conventions, an ANT-oriented consideration of research *translations*, and an analysis of narrative style in relation to epistemic function.

3.3 Ethical Approach

Considerations of ethics are central to this thesis, being, as it is, a project of research concerned with the *practices* of research. I refer frequently to the research virtues of transparency, reflexivity and multivocality. My focus on these qualities stems directly from my theoretical assumptions about the situated nature of knowledge construction and the significance of localised practices and agencies. When knowledge-making work is understood to be a contingent process, rather than an uncovering of universal truths, it is imperative, when reporting upon it, to share the circumstances of its production and the details of its processes as openly as possible; to maintain an awareness of the limits of an interpretative standpoint; and to acknowledge that knowledge claims made from different standpoints can have significance and validity.

I noted previously that post-processual schools of thought resulted in the incorporation of questions of agency and practice into archaeological research, alongside those of system and structure, and that this led to an increase in inclusivity in the discipline (see §2.2.4). There is, therefore, an overlap between my own theoretical assumptions with their focus on the idiographic and those schools of critical thought identified with the post-processual movement. One of my concerns in this thesis is to ascertain to what degree the values of post-processual archaeology are expressed in the research practices and concerns of those archaeologists I interview and observe.

3.3.1 Transparency

One of the ideals which I promote as a research virtue is the notion of transparency. By this I mean openness and honesty about research methods, motivation and assumptions, and a willingness to share information both about the wider context of research and the collected evidence and conclusions which result from it. Honesty and a degree of openness are basic requirements for research to be trusted and gain traction, and they allow claims to be subject to fair analysis and critique. Without this basic level of research integrity, the metanarrative of research projects combining to form links in a larger chain of understanding is invalidated. As I discuss in §7.4, the goal of making an authoritative *knower* has the potential to overtake integrity in the construction and reporting of knowledge claims. Transparency is increasingly a key value in the highly competitive fields of scientific

research, partly as a guarantor of ethical behaviour, but also as part of attempts to validate universal models of knowledge by making experimental research reproduceable, and after Popper, falsifiable (Erickson, 2016:75). This has resulted in the 'open access' movement in research publication (see Knöchelmann (2019) for a discussion of this in relation to the humanities). In my research I strive to be transparent about my own assumptions, the interests I bring to my topic, the nature of the evidence I encounter, and what I can reasonably *know* about it. However, I recognise that there are limits, in the context of social science research, when taking an ethical stance which emphasises openness. In many cases, the human objects of research may not be able to share the freedom of expression enjoyed by the researcher, and may be exposed to harms by a researcher who insists on sharing their experiences or opinions openly. This is most obvious where the sharing of an interviewee's identity alongside their comments might harm their relation with their employer/employee, compromise others or result in public disapprobation or persecution. In this project, the topics discussed are generally not so acutely sensitive as to invite such dangers; however, I resolved to anonymise the interviews and observations I conducted because I hypothesised that in the subject of study there was a *reported* reality with particular narrative conventions, which was familiar from published research outcomes; but that alongside this, the range of *everyday* research activities and experiences were less well reported and only selectively translated into publications. My assumption was that anonymity would free interviewees to express themselves openly about the everyday practices of research, including those aspects which were *not* commonly reported on in final publications; and to be honest about negativities or ambivalences related to their work which they would not otherwise be happy to share publicly. This hypothesis seems to have been proven correct: in interviews participants were able to identify and discuss 'unofficial', undocumented pressures on their projects, and at times were openly critical of certain research projects in their fields, or of overall progress in their disciplines. These comments would almost certainly not have been included 'on the record' without the commitment to anonymity. In the observations phase there was, especially at first, some reticence about my presence in meetings, which I was able to mitigate by emphasising the anonymised nature of my reporting. On the negative side, anonymity makes it difficult for the reader to fully contextualise the claims and opinions voiced by those in interviews and observations. This is of particular relevance when findings are reported from a small sample of contributors, and

an individual's comment may be misrecognised as being representative of the opinions of the broader archaeological community. I am conscious that the decision to anonymise contributions therefore creates a tension with the imperative often stated in this thesis to share the *context* of research as much as is possible.

In my analysis of research evidence I sought to be transparent by providing clarity about methodological assumptions and working hypotheses, and by following proposed procedures systematically, for example when making use of analytical categories, in the shape of codes, to inform the creation of narratives and the implication of connectedness.

3.3.2 Reflexivity

My emphasis on reflexivity stems from an awareness of the cultural, material, geographical and historical influences on my own research practices, and of the fact that those practices cannot take place in a 'neutral' space, insulated from the objects of their study. As described previously in reference to post-processualism in archaeology, this comes about as a result of incorporating questions of agency and practice into research. The perception of the need for reflexivity in archaeology is common to those schools of thought which emphasise the role of subjectivity in experiencing and making sense of the world, and I have therefore posited it as one of the core 'research virtues' against which to consider contemporary archaeological research practice.

Robson suggests that there are two types of 'researcher effects':

> The effects you have on the case; and the effects your involvement in the case have on you. (2016:479)

In my accounts of interviews and observations I use 'thick description' to consider what effects were produced by my presence, lines of questioning, perceived status, and stated prior interests, and in conducting these studies I sought to minimise the effects of my own agency where possible.

My theoretical approach accentuates the subjective aspects of the research process and the situated nature of knowledge production. I am therefore obliged to apply this same understanding to my own research, and acknowledge that my own process consists of the active construction of narratives from a disparate collection of evidence. Robson provides a

set of strategies for assessing the quality of data analysis, which I have adopted for this project where possible, including the use of 'testing patterns' (2016:479) (e.g. looking for negative evidence), to check on the validity of interpretations; and 'testing explanations' (ibid, p480) which fits well with an abductive search for valid hypotheses.

3.3.3 Multivocality

Related to the principles of openness and reflexivity, and dependent on their successful application, is the ideal that research results should be open to reuse and reinterpretation, to facilitate future research and iterative analysis, and to allow for the re-evaluation of research from a range of cultural and theoretical perspectives. An emphasis on diversity, inclusivity and epistemological reflexivity follows naturally from the principle that there should be equitable access to knowledge and knowledge-making resources for all. Just as important is respect for different cultural scholarly traditions, and an understanding that no school of research has privileged access to a definitive or redemptive truth which trumps others. The call for multivocality is most forcefully made from the position of those who are oppressed or silenced by the self-appointed authoritative accounts of 'objective' technoscientific practice (Haraway, 1997:32), e.g., Mika et al. (2020), Srinivasan (2017). My emphasis on inclusivity and multivocality as a research value is explored in my design considerations of the software project Orson (Chapter 8), in which I seek to develop features which support multiple interpretations, reinterpretation and rewriting of research in a decentralised, free-to-access tool.

3.3.4 Contested research virtues

It is important to point out that the 'research virtues' I have chosen to focus on in this thesis—transparency, reflexivity and multivocality—are not universally held to be priorities in archaeological research. This will become apparent in the following discussion of interviews and observations. I highlight them because of their relevance to interpretative knowledge and the complexities of producing knowledge claims from incomplete evidence—particularly in the context of archaeology's attempts to grapple simultaneously with the ideals of humanities and scientific research, and with contested accounts of history. They are representative of core ideas from the discipline's own somewhat

combative history of thought; but unsurprisingly, are of lesser relevance to those in pursuit of the ideal of 'objective' knowledge by means of technological interventions or the scientific method. They may indeed be perceived as challenges to the authority of deductive or inductive methods, and to the assumptions and authority of some established academics. I explore these themes throughout the thesis, and in particular in my discussion of 'troublesome knowledge' (§7.5).

3.3.5 Ethical approval

The appropriate process was followed for obtaining approval from the University of Brighton's research ethics committee for the various stages of this research project. The submission of the ethics documentation was made to the Social Sciences Cross-School Research Ethics Committee, and included provision of a risk assessment, a participant information sheet for interviewees, and a consent form for interviewees and those being observed. In my application I gave details of my planned methods, and a list of scripted interview questions. I committed to making clear to participants that their participation was voluntary and that withdrawal was possible at any time with no questions asked. I gave information about my proposed strategies to protect the privacy of participants and to ensure confidentiality; and I provided guarantees that any textual or multimedia recordings of interviews and observations would be stored securely offline, in an encrypted format. The committee gave their approval to the project after requesting minor clarifications on some points.

* * *

In this thesis I make the argument that the need for ethics in research goes beyond the need for honesty and fairness. It acts as a balance for the 'mechanisms of closure' which are used for stabilising knowledge and provides a guard against over–deterministic thinking. There is a danger of research ethics taking on a symbolic role in this respect, as I discuss in relation to technological objectivity and the predetermined shape of research outcomes (see §6.5), making the conscious adoption of values such as transparency, reflexivity, multivocality and the ideal of research reuse all the more important. Throughout this project I aim to take a self-reflexive approach to applying methods which are informed by the theoretical

approaches of Actor Network Theory and complementary constructivist understandings of knowledge work. I 'write myself into' my research accounts using thick description and highlight my own priorities and assumptions where this is relevant and practicable. In the following chapter, I describe the first of my 'intensive studies', a series of interviews with professional archaeologists.

4. Study 1—Interviews

4.1 Transcription conventions

I have adopted the following conventions in quotations:

[…] : Text was omitted for the sake of brevity or clarity.

__ : Text was redacted in order to preserve the anonymity of participants.

4.2 Participant backgrounds and perspectives

Interviewee quotations in the rest of this chapter are identified using the numbering in the following list, which sets out the occupations of the participants:

1. Academic archaeologist and research project manager
2. Academic archaeologist
3. Commercial archaeologist and software developer
4. Archaeological digital archivist
5. Archaeological digital archivist
6. Museum curator and archaeologist
7. Academic and commercial archaeologist
8. Academic and digital archaeologist
9. Academic and digital archaeologist
10. Academic and museum curator

Of those interviewed, 8 were UK based, one was in the US and one was working in Europe. 8 of the 10 were originally educated in the UK. The range of interviewees usefully encapsulates the diverse nature of the role of 'archaeologist'. The defining image of archaeology is of the painstaking excavation of remains from a pit in the ground. Of the 10 people interviewed, 2 had little or no experience of this kind of fieldwork. 4 were still

actively engaged in fieldwork activities, often at a senior administrative level. The others had previous experience of fieldwork and excavations but were now working in another specialist area.

Specialisms were marked out on a number of different lines, most often by interest in a particular historical period and/or geographical area, but in other cases by a focus on recording or analytical techniques, the use of digital tools in archaeological research, or the dissemination of archaeological knowledge in museums.

Based on the interviews conducted, the following broad areas of archaeological practice were identified:

1. 'Working' or commercial archaeology—the everyday archaeology which happens in advance of or alongside every significant building project and landscape development. This is done on a for-profit basis, but can involve strong ties to academia (one interviewee had senior roles in both commercial archaeology companies and in a university department). Also known as 'rescue archaeology', 'salvage archaeology' or 'development-led archaeology'.
2. Archaeological research carried out by heritage organisations. This work provides information which is useful for conservation, for local councils and planners, and for academics.
3. Non-commercial archaeology. This encompasses all archaeological research which is undertaken solely for its own sake, rather than as a side effect of a building initiative, for example. This is most commonly carried out by universities, often in collaboration with experts working in museums and galleries, and funded by the universities or charities.
4. Research, management and exhibition of museum collections.
5. Archaeological sciences. These are specialised fields devoted to the collection and analysis of archaeological evidence, usually in support of 1), 2), 3) and 4).
6. Archaeological philosophy. This field analyses the methods and assumptions of those in the other categories, asking for example, how much knowledge we can legitimately derive from the evidence and techniques available to us. This includes

> Digital Archaeology, which is concerned with the influences and potentials of technology in archaeological research.

This list is derived from the descriptions of the small group of people interviewed and does not purport to be comprehensive.
The interviews made clear that these categories are often interdependent and overlapping, but that professional identities are nevertheless strongly aligned along the perceived divisions between the different areas of practice. For example, one interviewee stated:

> I'm not sure University disciplinary archaeology takes museums particularly seriously. And I think that is something to think about when you're talking to people in the different areas, because we're all hyper specialised, but there is that common disjunction… …it's not to do with levels of education, it just means broadly, *I*, as someone trained in __, have a very weak sense of what *you* might be specialised in... So I think that's quite an interesting tension to explore in our work. (Interviewee 10, 2020)

While the parameters of professional identity seemed to be quite clearly drawn, the boundaries between the different 'types' of archaeology were extremely permeable. In at least 4 cases, interviewees were either currently active in both the commercial and academic spheres of archaeology, or had moved from one to the other over the course of their careers. One example was cited where profit-based archaeology work was used by a company to fund work which was purely of academic interest:

> And we are very flexible about where we put our resources. When you're running 200 projects a year, the goal is that on 180 that will make a margin, the other 20 are pits into which we throw money. And we throw money into those pits, not because we've messed up, but because there's something exciting worth doing. (Interviewee 7, 2020)

When recruiting staff, one commercial archaeologist used academic professionals as panel members and took applicants' academic publication records into account. Similar blurred boundaries were reported between academia and the heritage and museum sectors.
The research outputs from all of these sectors were of interest to the broader archaeological community, and were used as points of reference where they were

accessible. For example, the heritage sector's work was useful for local councils assessing plans for development, or when formulating conservation policy, but its reports were also of interest to academics. Museum and heritage workers contributed research papers to academic journals. These overlaps meant that there was a shared frame of expectations regarding the types of information recorded about sites and objects, and the forms they were presented in, even if research practices or goals were not fully aligned.

All interviewees had educational backgrounds in archaeology, but this often sat alongside other experiences and interests which informed their career paths and practice; for example those with a facility for computing became digital archaeologists or archivists.

Almost all were members of the Chartered Institute of Field Archaeologists. This was often a professional requirement, but was almost always mentioned as an unimportant aside. For one interviewee this was not a professional requirement in her post and she had not done a great deal of fieldwork.

4.3 Themes

The coding of the interviews resulted in 790 codes, which were then allocated non-exclusively to 10 main themes, listed here in order of the number of codes contained in each theme:

1. Institutions and organisations.
2. Data structure and access. Includes classification and relationships.
3. Technology.
4. Publication, sharing and engagement. Includes reuse and reinterpretation.
5. Research philosophy.
6. Data capture and production. Includes collaborative and collective work.
7. Interpretation.
8. Specialist knowledge and skills.
9. Practical research considerations. Includes time, money, funding, availability of information, data quality, sustainability, usability.
10. Research management.

The codes were allocated to themes according to the context of the topic of discussion. For example, the code ‘database’ was allocated to the theme ‘Institutions and organisations’ in one case because it had been attached to a discussion about university IT infrastructure, but in another case was allocated to the theme ‘Data capture and production’ because it had been attached to a discussion about formatting fieldwork data for digital storage.

In addition to topic-oriented codes, other codes were allocated to adjectives and nouns which were associated with strong opinions or emotions, to help pick out issues which participants felt strongly about; for example ‘challenge’, ‘comfortable’, ‘frustration’, ‘laborious’, ‘uncomfortable’, ‘valuable’.

In the following section I summarise the key points which were conveyed in each thematic area.

4.3.1 Institutions and organisations

‘Institutions and organisations’ was the theme most frequently referred to, with around twice as many codes as any other. This highlighted the high level of institutional dependence in the interviewees’ archaeological work, and the fact that organisations with budgets and accumulated expertise enable research work to be carried out, but also help to define the shape of expert communities, and to filter and constrain their outputs. The individual codes helped to show how the influence of institutions is woven through all aspects of archaeological research: from the most frequent one in that theme, ‘funding’, to others such as ‘data reuse’, ‘database’ and ‘data formats’, which illustrate the connection between institutions or organisations and digital infrastructure (discussed further in §4.3.3); ‘academic publishing’ and ‘REF (Research Excellence Framework)’ which emphasise the credit system whereby certain types of research values and the institutions which implement them are rewarded; and codes which point to the role of academic communities for creating and validating work: ‘team work’, ‘multidisciplinary approach’, knowledge communities’, ‘authority’.

Interviewees’ accounts generally accorded with the findings reported in the literature on openness and multivocality (§2.3.2): that these were ideals which in practice were often

difficult to fulfil. One curator with responsibility for a particular museum collection reflected on the nature of their professional role:

> …you think, all these people are coming to me for advice, because I'm so pivotal in this community. And then you realise, oh, no, it's because of the collection that you get that kind of position, because a lot of it is about access and information and connecting people.
>
> …A huge amount of my job is persuading other people to do things and playing my role as one little cog in a big complicated team. (Interviewee 6, 2020)

In this case the 'community' was one made up exclusively of specialist academics and other curators. The curator's engagement with the 'public' was via exhibition content and talks, principally as a teacher. Another interviewee highlighted the constraining influence of an institution on a software project:

> …[it] failed miserably, not because we didn't build an interesting system that worked and was usable and all the rest, and was focused on the community, which it was, nor was it because curators and the museums weren't interested in helping—it was mostly the institutional infrastructure, the museums simply could not accommodate multivocality at the institutional and infrastructural level, it simply was not possible. (Interviewee 2, 2020)

Mentorship in archaeology and practical experience in the field both appeared to be a strong influence on the critical approaches adopted for archaeological work, and there were frequent mentions of former teachers and places of HE study, with the implication that there were distinctive local practices centred round authoritative practitioners and university departments. Institutions' archaeology departments were referred to as sites of authority, and sources of trust:

> I'm gathering a lot of information about that [archaeological object]. And it comes in lots of different forms and from lots of different people. Some of it, I'm absolutely fully confident in. It's an object which was excavated by a very skilled and expert archaeological field unit, based out of [university], who I know very well, so all of this stuff about the actual recording of the find spot and the stratigraphy and all of that,

> I'm completely confident in. […] So I can look at it, I can weigh it up, I know they're a good team, no worries about that. (Interviewee 6, 2020)

There was a mention of distinctive fieldwork practices which were a consequence of institutionalised instruction practices at a national level; these led to scepticism about archaeological fieldwork practices in some other countries:

> The [country X] teams tended to excavate and plan a method, which, frankly, I thought was not worth the paper it was written on. And the data that came out of it was almost unusable—the data was fine if you wanted a plot a wall line, but pretty well everything else, including what date that wall was, was highly suspect. Because they didn't understand formation processes, and they didn't understand something that came out of a bottom of a pit, but was therefore lower, in an OD sense, was in fact later than something that was higher than it that wasn't in the pit. That kind of basic understanding of formation process. (Interviewee 1, 2020)

I interpreted the different pockets of practice in archaeology as examples of what Fleck has described as 'esoteric thought communities' (1981), specialist groups with their own respective sets of epistemological and ontological commitments. The methods of the profession were evolving and there was much cross-fertilisation of expertise, but certain established 'tribes' persisted. For example, some archaeologists used scientific equipment or evidence as a matter of course, but were adamant that they were not scientists—indeed some found it difficult to identify with technical scientific research, seeing it as extremely narrow or esoteric in its concerns—something for 'boffins'.

The epistemological and ontological commitments of these communities were articulated most clearly in their reports, websites, and journal articles: in other words, in the ways they discussed and set out knowledge; their *discourse* (Foucault, 2002:120). Hacking's (1992) notion of a 'reasoning style' is useful for framing these models of understanding and communication, because it more clearly encompasses non-textual or non-linguistic means of reasoning, such as software tools or measuring instruments, and is a better fit with the boundaries of an 'esoteric' academic discipline. I will use the concept of 'thought communities' with their own distinctive 'reasoning styles' throughout this thesis.

4.3.2 Data structure and access

If institutions provide the overall context for the work, much of the work itself consists of the processes of documentation: retrieving and translating evidence into negotiated structures to achieve the goal of 'preservation by record' and in some cases, to allow for further interpretative phases of documentation in the shape of reports, articles and books. An interviewee summed up the overall focus of the technologically mediated recording process:

> …in terms of excavated data, we've always been very reliant on stratigraphic matrices for that understanding of the control of deposition, and sequence and also the ability to see what's missing within that record. So in terms of technologies, I think we're increasingly interested in geospatial data, that ability to pattern things in three dimensions. So being able to relate material culture, environmental data, and stratigraphic data together. And to be able to visualise that in a three-dimensional context. And I know that's still something in many cases, we're groping towards the utilisation of the technologies, to enable us to do that effectively. (Interviewee 1, 2020)

The interviews touched on some of the negotiated aspects of translations, involving the authoring of database structures and fields, classifications of the appropriate granularity, and the delineation of the edges of areas of 'archaeological interest'. One interviewee discussed strategies for handling uncertainty:

> In single context recording and planning, we have drawing conventions for the difference between where you are confident about the boundaries of a deposit, and where you are less confident. So where there are ambiguities that the context definitely finishes at this point and you can see a clear edge as opposed to, 'well, it's finishing around about here and it's definitely a good 20 centimetres further east, it definitely is a different deposit but exactly where the edge is', and that blurring of edge is a blurred situation, we have drawing conventions to reflect that. Ceramicists, ones I've worked with closely, have conventions between their levels of confidence about dating, or provenance or form. […] And that's something I think is fundamental in good documentation systems, is recognising uncertainty, and being able to reflect that in that record. (Interviewee 1, 2020)

This seemed to be the exception however, and another noted that overall, the sharing of data related to uncertainty was uncommon, suggesting a tailing off of interest in initiatives seeking to formalise its documentation (such as the London Charter, see §3.4.1):

> …there's still some debate about, you know, presenting uncertainty within the image itself or within the model. But in terms of providing it outside of that, I don't think so. (Interviewee 8, 2020)

Much of the discussion in interviews centred around the research processes related to the management of records themselves, reinforcing the centrality of documentation in the discipline:

> I've been researching that through the archives about the excavation. I wasn't there, I wasn't digging, but archaeology is, as you will know, about preservation by record. So we're going back to the record. And this is, I think, quite typical. (Interviewee 6, 2020)

A repeating theme was that of frustration with access to existing records of archaeological projects, and the failure of existing digital storage solutions to solve this problem so far, echoing some of the concerns set out in §2.3.1. Repositories such as the ADS required a fee for deposits to be made and many projects did not have a budget for this cost. Therefore project coverage was patchy.

> The problem is the ADS is expensive to deposit data with. […] But most developer funded projects don't build that cost in. So who's going to deposit that data? (Interviewee 1, 2020)

Archives were not always easily usable because while they were stored in open and accessible forms to guarantee future readability, they did not have a common, easy-to-use interface. The model of the Open Context repository in the USA was lauded by one interviewee; this paid service maps all submitted projects to a common interactive format and incorporates basic semantic relations between items.
Other archives presented bureaucratic hurdles to data access:

> …they have no system for accepting digital data. Because they don't have the resources, they don't know how to. So the whole issue of managing it, migrating etc., is outside of their current resourcing. They neither have the expertise, nor the people on the ground, nor the resources to cope with it. […] So you have to do it through personal contacts and things like that. …it's more laborious than it should be. (Interviewee 7, 2020)

The archive records did not always have the required searchability or granularity. For example, final reports did not include sufficient contextual information, or failed to include matrix representations created during the original site excavation. Reports could not, as a result, be properly 'unpicked' for research purposes. Rather than seeing these cases as failed examples of 'total' documentation, they can be understood as expressions of the difficulty in making records interoperable, as recommended by the FAIR principles (§2.3.1), or commensurable: the authors apparently brought different priorities to the project than some of those seeking to make use of the results.

Other practical issues were mentioned, which had an effect on the documentation of projects and their subsequent retrieval; attempts to standardise records using semantic relations had proven to be difficult, according to one archivist:

> …operationally, I just don't know how to really put it into practice in a way that makes a huge amount of sense, given the fact that so much of the data that we get is just built without that in mind at all. (Interviewee 5, 2021)

Imposing a common semantic scheme was therefore seen as 'compliance for compliance's sake' and not a useful means of making records comparable. One of the difficulties was the eclectic nature of archaeology:

> …there's no real firm boundary around archaeology anyway. And so we're going to always have inputs, and our data are going to be wanting to be used outside of archaeology—we're going to have an interest in data from outside of archaeology. And there are just so many different institutional players inside archaeology anyway, that there's not going to be one central place that's going to have everything. So then yeah, the aspects of trying to promote good practices in a distributed world is something that you want to do. (Interviewee 5, 2021)

These experiences had inspired a degree of pragmatism in the archivist interviewed,

resulting in his adoption of the use of lightweight semantics in archives and an emphasis on the portability of digital resources.

There was a general sense from the contributors who were invested in this issue, that while access had been much improved by the advent of digital storage and repository use, the hoped-for vision, over many years, of digitally-enabled access to highly searchable resources, had failed to materialise. This was a source of some disillusionment to some, and I had a strong sense of a gap between the informal verbal discourse in interviews related to this topic, and the literature, with the former being more open to recognising fundamental difficulties. A few interviewees, however, still felt that the goal of comprehensive digital access to records was close to being reached.

4.3.3 Technology

For interviewees working with digital documentation, database-oriented data storage was often of defining importance for the organisation of research data, and as a consequence, for its collection and maintenance. For some participants this was one of their direct working concerns as they worked personally on the design of databases and on setting up web servers to host applications. Others were impacted more indirectly, for example in dealing with the selection of suitable digital publishing platforms, trying to secure long-term server space with technical support, or simply by the interpretative choices they had to make of what to store and what not to store.

The research agenda expressed by interviewee 1 above was broadly representative of that of most participants: an interest in geospatial data, and in relating it to 'material culture, environmental data and stratigraphic data together' (see §4.3.2). This vision was based on the idea that the creation of a more complete 'total record' (see §2.2.1) could be facilitated by technology use, and that this use of tech would be a source of convenience. The promise of the 'total' digital record was also one of archives which could be more easily accessed and searched (see §2.3.1). Further specific aspirations for the use of digital archaeology were described by other participants, including:

- the creation of links from narratives to data, in documentation (cf. §3.3.2)
- the ability to allow people to 'come at data from different perspectives' (cf. §3.2.2, §3.3.3)
- the use of flexible databases to allow researchers to record whatever they want to, thus acting as freeing agents in the process of fieldwork

As indicated in the previous section, participants found that long hoped-for potentials of new technologies had not been completely fulfilled. Successes were balanced against frustrations. In settings where ownership of the technology was necessarily devolved to institutions who were required to set it up and maintain it, control of resources was often compromised, capacity limited, and support slow to arrive.

One archaeologist spoke of the aspiration to share materials digitally in order to 'bounce ideas around', adding that in practice, people were only 'playing at the edge of that idea'. The Arches[18] software platform for publishing cultural heritage projects was much appreciated for its powerful search functionality, which, it was hoped, would encourage widespread use from a variety of users, including managers of heritage sites. Balanced against this perceived benefit was disappointment at the lack of GIS functionality in the platform, technical issues related to missing features in updated versions of the software, and concerns about its long-term viability, leading to a decision to locate content outside of the platform, and limit its use to acting as a 'front-end' for data sources. Collections of image files with large storage requirements were not supported in Arches. Expectations about ease of use in setting the platform up and providing access were not met. The software platform was 'heavy' and not suitable for international partners with poor internet connectivity. These factors broke the intended collaboration model for the interviewee in question.

The narratives about technology for research documentation were stories of negotiation and compromise: negotiations with institutions, technical staff and with the capabilities of the software platforms themselves. Where open-source software was being used, some

[18] https://www.archesproject.org/

researchers had engaged with adding code features themselves, but this was a bureaucratic process, also requiring negotiation, patience and stoicism; because while it is possible to adapt software for local use, if long term support and compatibility are to be secured in such cases, the community of users must be persuaded to adopt the same changes. Otherwise, the burden of maintaining a specialised version ('fork') of a software platform can become impractical. Therefore at almost every level, projects of any size were heavily dependent on the broader academic and technical communities and institutions for success.

For one interviewee, there was frustration at museums' use of digital technologies as referential media: his view, chiming with that of Llobera (see §2.5), was that they were used as intermediaries for other forms, but their own intrinsic strengths as media were not being exploited.

> I think that a lot of what we do is basically just reproduce, especially in archaeology, what are essentially 18th and 19th century modes of understanding the past, in a digital form. (Interviewee 2, 2020)

The limits imposed on the applications of technology were partly seen as related to institutional politics and a desire to maintain control and ownership of data:

> One of the biggest things museums could do tomorrow, to make their collections meaningful to vast communities, was to ensure that all of their collections were searchable on Google. How many collections are searchable on Google? Practically none. (Interviewee 2, 2020)

Another interviewee was concerned about the implications of the use of 'black box' (Latour, 1987:2) software packages:

> I think one of the challenges is we are increasingly using tools that we don't necessarily fully understand how they work, or who produced them, or what their motivations were in producing them. And I think that has gained potential implications for what the outputs actually are. (Interviewee 8, 2020)

Interviewees were highly conscious of the limitations they were forced to confront in their uses of technology for research, as the details of such issues clearly occupied many hours of their working lives. This did not seem to deter them from engaging positively with the potential of innovative practices, and often technical innovation was a central theme in one-off research projects. While interviewees seemed somewhat jaded with the everyday experience of technology use, as a result of the gaps between aspiration and practice, this was balanced by stoicism and pragmatism. As one interviewee put it: 'we kind of just shrug our shoulders and get on with it anyway. Because it's what we've got' (Interviewee 8, 2020).

4.3.4 Publication, sharing and engagement. Includes reuse and reinterpretation

For the archaeologists interviewed who were active in fieldwork, the relationship to digital technology was often about data quality and accessibility for themselves and others:

> I believe in preservation by record, in the sense that what we record is as good as we can do, but it's not as comprehensive as leaving the stratigraphy in the ground as it were. But in doing so, in, in trying to produce that quality of record, I want to make that available to others to research. If we're substituting real stratigraphy and real artefacts in context with a record of that, then I want other people to use that information. I'm very keen on the quality of access into archival data, which has drawn us a lot into that digital world, of making that accessible, because of the problems of making the complexity of that record available through paper and conventional means. (Interviewee 1, 2020)

Access was important not just from the point of view of making results available to researchers, but also in terms of sharing the 'complex information' (ibid) that those results were based on, with the implication that this information could be useful for other research agendas. One interviewee was deeply invested in the use of technology for site recording and archiving, but also saw the future use of database systems as a potential means of developing a greater level of reflexivity about research agendas and research progress in general:

> We do intend to link back to broader research. So I think it's going to take us 5, 10 years before we actually get back to that part of it. But there is a part of this database, in which when we do risk investigation, we enter in our research questions

> for the projects. And we're trying to link those research questions to regional research agendas. […] And that will then allow us to look at which research questions are we pursuing on a project-by-project basis. And we can identify, oh, we're always looking at the later Iron Age into Early Roman interface. So often we've never asked a single interesting supplementary question […]. Are we really wasting our time with this? Or is it actually still cutting edge? And we will be able to document which research questions are never being applied to development in field work, we will be able to work out which research questions are always applied, because they're nice and generic. But to spot the children of our research questions, which are generating new and improved […] questions is interesting. (Interviewee 7, 2020)

However, there were a number of practical difficulties in achieving the ideal of data reuse: 'bad data', 'incoherent datasets', incomplete data and data contributed from different specialists with different emphases, all resulted in highly inconsistent datasets.
It was observed that while reuse of data was an ideal for those working in the discipline, it was something which occurred only rarely. The variances in how research projects were documented, and the use of lowest-common-denominator formats (such as CSV files) in order to make them storable and readable in the long term, discouraged researchers from revisiting them—because their basic forms, while highly accessible, were not always the most user-friendly. In addition, data standards and structures were not static. One archivist commented:

> Updating and changing and things, that's something that has to happen all the time, because the landscape changes, expectations change, and you learn a lot. And what's hard about that is that operations have to continue. You know, you're publishing more data continually, but at the same time really trying to refactor a lot of stuff internally. (Interviewee 4, 2021)

The standardisation of repositories and archives was a long-term problem:

> There's been endless attempts to get things like archives to link up. None of which have really worked. And have shown up the limitations of that, because you feel that it would be it would be good to do this. But then when you *do* do it, you think well, actually, what is that? What is that showing me? […] They had a connection whereby you could search both catalogues at the same time, but really, it seemed to show

> that the archaeology in both countries was completely different. So why would you actually want to? (Interviewee 8, 2020)

Incompatibilities were not so significant when reuse was focused on 'putting dots on maps' but became more of an issue for in-depth analyses:

> …as you start to drill down into the data itself. For that, you really do need that contextual information. And we don't really have a means of collecting that in a structured reliable fashion. (Interviewee 8, 2020)

The formats in which archives were stored also caused problems for retrieval:

> I don't think our archaeological publications have been as good at really helping people to drill into that data as they could be. […] So even when you've got good archives of material, like London, with the archaeological archive, very little of it is available digitally, only basic catalogue summaries and things. So you can't digitally search for that. […] We need that kind of ability to use this digital data and to get to that data effectively. And it's kind of half there, but only half there. (Interviewee 1, 2020)

The potential difficulty in serving the requirements of both discoverability and providing full context was highlighted by the approach of one repository, where individual objects were given 'independent utility from the whole', suggesting a common perspective with that looking to leverage 'big data' (see §3.4.2):

> Future iterations will have the ability to, for example, cross search for raster images that are photographs, and to cross-search the metadata so to allow people to do a lot more customizable searches of the objects themselves. So the objects can be found outside of that little wrapper that we call a collection. For that to happen, each object needs to stand on its own two feet, needs to be discoverable, needs to have metadata, and thus serving it up in a kind of in a wrapper is generally something we don't do. (Interviewee 4, 2021)

One interviewee highlighted the limitations of conventional site documentation for the support of reuse and reinterpretation:

> I kind of feel like the nuts and bolts of the colour of the soil and the data and that stuff is not particularly useful to reinterpret with, but the interpretations or site diaries probably are, or will be at some point, because they tend to have lots more marginalia and stuff you might think is insignificant, but might actually not be insignificant, you know, when you're revisiting the sites. (Interviewee 3, 2021)

An archivist noted that reuse of the archive resources was detectable via downloads, but the nature of the reuse was largely unknown. The uncertainty about this was in part because the use of DOIs (Digital Object Identifiers, unique ids), which might have been useful for tracing the use of resources, was very difficult to enforce upon those making contributions to the archive. Multiple participants identified education or teaching as one known purpose for the reuse of site documentation.

The main barrier to meaningful reuse was lack of time (or a lack of the prioritisation of time) on the part of would-be re-users, recalling the primary difficulty identified with reuse in the literature, the absence of motivation or incentive (see 'Reusability', §2.3.2). Interviewee 3 pointed out that the notion of reuse was not a practical proposition for many archaeologists, in particular commercial archaeologists, who 'seldom have the time and funding' to 'rework' existing sites (see also §4.3.9). This was likened by interviewee 5 to the ideal of reproducibility in the science disciplines, and described as 'an economy of time and attention', both in terms of the additional effort required in authoring documentation, in order to make it reusable, and the interest in revisiting it. Even for academics it was difficult to find colleagues willing to making contributions to iterative work:

> There's been many, many projects that have tried to put their data out there. And we've had to do things like offer grants and things like that, to try to get people to reuse the data, just like please, we're desperately trying to get you to look at and reuse this. (Interviewee 9, 2020)

An alternative perspective on this was given by an archaeological archivist, who saw reuse as a natural occurrence, though this was in the context of specialists making use of quantitative archive data in order to build up hypotheses which transcended individual projects.

There was positive feeling about the opportunities which digital media might present for publishing research results in different formats, for example by representing uncertainty through the presentation of 'alternative phasing constructs'. The relatively low cost of storing and publishing media electronically in comparison with print opened up new possibilities for exploring alternative hypotheses by including multiple graphics, for example (cf. §2.3.2). Similarly the idea of collective iterative authorship of a project using digital publishing models was an appealing one. Set against these possibilities were institutional constraints:

> In the post-excavation process and publication process, there's a lot of pressure to come to a final conclusion. […]
>
> …anybody else can join in and we can keep on knocking it around and it could grow and develop. People could bring in other examples, you know? Wouldn't that be fantastic? Yeah, but, but the big problem is, for anybody based in academia, there's still a ridiculous amount of pressure to publish in a finished form, for REF purposes. If I've got a paper that I started and now 20 people are contributing to, and it's not finished yet, and it'll maybe run for 10 years as it grows—you know, that gets you diddly squat points in that one. (Interviewee 1, 2020)

The academic reward system acted as a strong disincentive to an open, iterative approach. It was noted that outreach in academic projects to non-academic audiences had been encouraged by a new emphasis on impact case studies in the REF:

> But in terms of the REF, the impact case studies has changed universities' attitude towards that broader communication, because it's perceived to be part of that impact. Up until then, academic publication was the only thing you got any credit for within the university system. And so the pressure in terms of performance, I would say, was strongly upon academic peer-to-peer scholarly publication. And the REF impact case studies has actually made universities more enthusiastic about some of those outreach activities. (Interviewee 1, 2020)

This had resulted in more enthusiasm for such activities from universities, and in an improvement in engagement with wider audiences. In the museum context this was also seen as a desirable outcome, but there was doubt as to whether this was being achieved in any meaningful way:

> But there's a kind of an underlying, if you like, social disbelief, that this is really about engagement rather than about just remarking the boundaries. […] And probably there are ways of engaging with other audiences, but maybe we just need different institutions that are not called 'university' or 'museum'. (Interviewee 10, 2020)

Interviewees 10 and 2 perceived an entrenched institutional resistance to change in the museum sector, and a lack of multi-perspectivism. Museum collections functioned as crucial, central nodes in expert academic communities, but their attempts at inclusivity with broader communities (and the fostering thereby of reinterpretation of collection artefacts, or different models of knowledge) were mostly superficial. These organisations were understood as sites of cultural significance, and therefore also as seats of power, which wished to retain control over their assets for reasons of authority and/or practical economics. For interviewee 2, the structure of museum collection documentation meant that it was often simply not suited to being repurposed. It was authored to be a static archive and could not conceivably function usefully as an interactive community resource. In other words, there were fundamental difficulties for establishing alternatives to the 'top-down' model of Community Archaeology described in §2.3.1 (p.52), difficulties which were a direct consequence of the archive's representational form.

Finally, for those producing digital outputs, or specialising in the study of digital archaeology, there was an acute awareness of the issue of the long-term viability of specialised software packages and digital outputs:

> It's all very well, building these tools. But maintaining them and developing them is a real problem, partly because I guess the funding tends to be available for project that might last 2, 3, 4 years, but then the funding stops. And you're left with hopefully a very interesting tool, but nobody can afford or is willing to pay for its maintenance and upkeep. And so particularly when you're dealing with things like linked data systems, they break very easily and do need to be maintained. And that does seem to be a big problem. […]
>
> It happens with a lot of projects at a smaller scale as well, for example, there was the virtual research environment that was set up […] some years ago, which was a really interesting project, it was really pushing the sorts of things that you could do with

> the technology. […] But it was predicated upon the software skills of an individual, an individual person. And when that person retired, they stopped developing that toolset. (Interviewee 8, 2020)

4.3.5 Research philosophy

There was a division in the interviews between those who were actively concerned in their own work with the philosophy of archaeology as an end in itself, and those who though not uninterested in questioning their own theoretical assumptions and practices, had other priorities. Those in the former group were in a minority and were academics in one shape or form. For some of these, there was dissatisfaction with the political or ideological status quo in the discipline, and an interest in alternative practices or ways of knowing. In other cases the focus was on the potential of digital archaeology and on developing a better understanding of the agency of digital tools. Multiple interviewees expressed a desire to increase community ownership or authorship of museum collections, and to increase access to them.

Members of the philosophy-oriented group generally had some investment in the ideals of the London Charter and the Seville Principles (see §2.3.3), particularly in relation to the goal of increasing reflexivity in research:

> …maybe the Charter itself came out of our existing research practice, which is to be worried about where our evidence is leading, although we want to deliver results, and we're told it should be delivering results. And we know it's all about process as well. Something we fall short in still is holding up our results, our processes to scrutiny. […]
>
> In the further development of the London Charter and the Seville Charter, they sort of have to present themselves as solutions. And that was not the original intention. It was about problematizing, which is what we do in research, and maybe presenting as an unresolved problem. (Interviewee 10, 2020)

It was suggested that the analytical methods of archaeology might be usefully turned inwards onto the practice of archaeological research itself, to this end:

> Well, I think if we could maybe rethink, using the old châine opératoire of archaeology, an operational chain where you have various ingredients and put them

> together: if we could use tools like that—this is just talking from within the archaeological context where you might be talking about many people working with fieldwork, archaeology, or finds interpretation—you can find a concept or a method or tool that people are already using, and just more or less say, try doing it this way, what are all the points [in the process]? I think that's what the London Charter was doing with the paradata. (Interviewee 10, 2020)

The Digital Archaeologists in this group applied their critical focus to the influence of computing-based practices in shaping research outputs (for example the uses of 'big data', or the unquestioning use of opaque algorithms for data processing), in addition to exploring the unique affordances of such practices for enabling improvements to documentation and access to records.

For those not directly focused on archaeological philosophical themes, there were ambitions to improve current practice, for example to make the excavation and documentation processes quicker and more efficient, and to make archives more complete, more discoverable and more 'joined up'. For this group, introspection about epistemological validity was not a primary concern. One archaeologist expressed faith in the inferential power of empirical archaeological research to synthesise hypotheses:

> And I like the way in which archaeology is quite honest to its primary material, and how we layer up from a pot sherd, an observed post hole, a bit of stone, up into 'and that's why the Roman Empire failed'. There's lots of steps in that as one abstracts, but I like the fact that we structure our data in ways that we understand how we build it. And the wonderful thing about the digital, now, is the ability to start being able to link synthesis to primary data in novel ways. (Interviewee 7, 2020)

Other participants working in the same area of practice shared the same discourse for conceptualising their work. When questioned they were in some cases happy to acknowledge the influence of the researcher's context for producing meaning. One explained this in terms of reading the work of Soviet archaeologists and making sense of their Marxist interpretations of the past in the context of their ideological perspectives and the political pressures they worked under. There was a general acceptance that the context of research was significant, but little interest in exploring the implications of this for their

own research. The closest thing to a 'default', 'common sense' position for these participants seemed to be a 'soft' empiricism, as in the example above: 'soft' because the influence of context was acknowledged but not actively engaged with (for context, see §2.4, 'New Archaeology').

4.3.6 Data capture and production

For those working in site excavation, technical improvements and efficiencies in fieldwork were valued as a means of improving access to knowledge:

> We are photogramming now, anything of interest, and we are drone flying... And we've got good UAV fliers, and we've got a fleet of half a dozen now. We've got CAA licenced pilots as well. So we fly both legally and high res. And we generated lovely 3D models from the drone flights. […]
>
> You ought to be in a situation where the primary context sheet data has become actually your free text interpretation rather than the nuts and bolts record, because the nuts and bolt record is digital. But we're four or five years away. (Interviewee 7, 2020)

This interviewee's priorities were very much related to access to data, either in the context of collecting and visualising data in the field, in a timely and efficient way as described above, or from archives, after the fact. Once the pieces of the jigsaw puzzle were available, they could be fitted together:

> this is why I'm so passionate about the building blocks from data capture, to eventual synthesis. (Interviewee 7, 2020)

The quality of the data retrieved from sites was considered a product of excavators' expertise and of knowing where to 'give their attention' (ibid) in the field.

Other interviewees with more specialist concerns were more contemplative of the mediatory effect of the methods used in data collection and management:

> I'm particularly interested in how the way that we handle, collect, manage archive data has implications upon how we then subsequently use and interpret that data.

> And what sort of changes might be brought about by the introduction of sort of very large datasets or the accessibility of data in increasingly large quantities from all over the place, but also what the implications are for taking data and fitting it into the sort of infrastructures that we're becoming increasingly reliant upon. Does anything get left out in the process? (Interviewee 8, 2020)

There was also wariness of a gap opening up between archaeology and data management, for example in the creation of semantic relations in data models (see §3.4.3):

> …they were trying to produce semantic networks ontologies. […] Whilst they started off by sitting down and talking to all the people that worked with this data, thinking that they needed to get them to create this ontology, actually, they found that people had no idea—they couldn't understand what on earth they were talking about. And so they actually had to get the semantic specialists to deconstruct from the domain experts to produce the ontology, and even then the experts were struggling to make much sense of what they were doing. And I think that is a big challenge. I guess in some areas, people will actually start turning around and saying, well this isn't archaeology anymore, this is computer science, this is knowledge management. And it takes you far away from the archaeology itself, which some people find problematic. (Interviewee 8, 2020)

This is an interesting observation in the context of a discipline which habitually embraces diverse specialisms. It suggests that while researchers are comfortable in working with specialists, they have basic shared expectations about the building blocks of knowledge and the research methods which can be deployed. It may be that data management using semantics might be more readily embraced in those cases where it can share its outputs in familiar or actionable ways, and does not threaten to displace existing ways of working. Alternatively, its continued use may encourage the development of new schools of archaeology which feature hybrid expertise.

4.3.7 Interpretation

Interestingly, while the mainstream perspective on data collection emphasised the exacting capture of geospatial data in multi-dimensional models, via processes which remove some of the inconsistencies of subjective human recording, one of the proponents of this approach spoke about the interpretation of stratigraphy in very different terms:

> I think you can teach stratigraphy in an environment. But it's different to actually getting out in the field and working with people who learn in the field, the texture of different deposits, and how to record them, and how to distinguish between them and how to look at a sequential process. And it's a skill, it's a manual, technical skill. That was kind of interesting, going back to an old Bauhaus model. The artists there said you can teach about art, but in the end, you know, you've got to do it, you've got to mix the paint and feel the different texture of how you apply it with a different implement, and different types of brushes and palettes, and teaching had to be part of that—and certain people learned that feel, and other people never got that. And digging is, I think, a good example of that. Some people learn how to feel stratigraphy and understand that formation process and other people, they can go on as many things as you like, but they'll never really kind of get that understanding, and then how you communicate that understanding through the record. (Interviewee 1, 2020)

This almost mystical faith in the 'feel' of certain practitioners was raised by the same participant elsewhere, when discussing judgement and trust. He was clear that he would accept the stratigraphic interpretation of certain skilled colleagues as being definitive—that because they had superior 'feel', they could simply 'know' better than others:

> Within post excavation, very often, one had situations of say, 'How well do you trust the observation made by people?' And I certainly know that within teams I worked with there are certain excavators, who if they said this was the relationship between various deposits, I was absolutely convinced that that was the best possible record of that relationship, because they knew. Absolutely, and they had that feel for it. There were other people, particularly on large projects, who you said, 'Well, maybe they thought that's what was going on. But I'm not necessarily as convinced by that record, as I would have been if somebody else had done it.' And so, yes, I think that that ability to put people into that recording process without saying that you throw away the recording process itself, is very important. (Interviewee 1, 2020)

Interviewee 9 described the skill of stratigraphic analysis as consisting of 'learning to see', also observing that this skill was hard won and not attainable by all. I interpreted this as a reference to the discernment of meaningful patterns from partial archaeological or geological remains in observations of site stratigraphy—an ability to switch gear into a different perceptual mode, to perceive a historical, alternative reality. The idea that this was an almost innate skill did not seem to take account of the pre-learned nature of the

hypothesised reality, or to ask questions of its authenticity: the assumptions about what could be discussed about traces of the past seemed to be based on a consensual ontological model developed by an esoteric thought community. These examples also served as reminders that the 'craftwork' aspects of archaeology tend to remain tacit and undocumented (see §2.3.3). I discuss tacit knowledge in more detail in §8.5.2.

One archaeologist saw a strong link between the quality of implementation of site documentation and the extent to which on-site interpretations were included:

> the single context recording system had a lot of space for interpretation. And one of the things I did as a supervisor on excavations, when checking records, is going back to people and saying, 'it's not a complete record and I need to know that you think that this is a post hole or a pit or how it's formed, or how it relates to other things.' Whether anybody else subsequently coming to that record agrees with your interpretation of that, what's very important is we understand that you thought it was a posthole, while you were excavating, and that you thought this about how it had been formed. And so a context sheet was as badly filled in if it lacked interpretation, as it was if it didn't have a soil description or the stratigraphic relationship. (Interviewee 1, 2020)

Relatedly, it was deemed important that skill levels and experience should be taken into account when documenting sites:

> we need to recognise that it is a skilled profession, being conducted by people who are at different levels of skills, different levels of understanding different qualities, qualitatively different abilities to, to see and understand what they're recording. And we needed to understand that within the record. (Interviewee 1, 2020)

The suggestion then, was that interpretation is given some context by the inclusion of information about the interpreter, and insight into the privileged aspects of their perspective. However, it was not clear that there was any means of capturing this relationship in the standard documentation forms (context sheets), and while the perceived authority of an interpreter might be obvious to a site supervisor, it would not necessarily be a retrievable feature in the record for later readers.

It was clear that the free-text space given for interpretation in the single context recording system was deemed to be of great importance by interviewees, but my impression from the range of interviews was that it was not a good fit in a system which otherwise favoured empirical measurements and an emphasis on typologies (see 'Classification', §2.3.3). One interviewee captured this well, stating that while reflexivity was a widely accepted ideal in modern archaeology, it was not straightforward to capture it adequately in site records:

> in commercial archaeology, obviously just write this stuff down and get off site and you know, figure it out afterwards. And in the academic archaeology I think that reflexivity actually happens in the field before they write anything down. That makes sense. And then they put their final interpretation in the system. (Interviewee 3, 2021)

The difficulty in capturing reflections was a consequence of the formal aspects of the recording system, but also at times, of limits to the perspective which an archaeologist might be able to bring to the work:

> The majority of stuff is form bashing or context recording where you fill in a box, whether you fill it in on a computer, or whether you are on an iPad or with a pen. […] But at the end of the day, you're digging a ditch yourself, and you're not necessarily in contact with everyone else on site. So you're digging something which is reddish brown, and it's got some brick inclusions. How reflective can you be about that, really, until you've seen everything else to do with what's going on? (Interviewee 3, 2021)

There were references made to specific projects which had tried to implement more conscious approaches to the documentation of reflexive practice. In one case, a large-scale commercial site, the project had been innovative in its data sharing and highly inclusive, but ultimately at the expense of the completion of the primary documentation which would have enabled the commercial development to go ahead. It was therefore deemed to be a failure. Another example was the excavation led by Hodder at Çatalhöyük (Hodder, 2000) in what is now Turkey, which was highly innovative in its recording practices and its attempts to reimagine collaborative interpretation. In spite of the novel research methods used, a conventional published output was still ultimately required:

> ...that's the issue, actually, you can have these fantastic practices. But in order to get recognition for it, there's still a very traditional way of presenting the data that people aren't getting out of at the moment. I don't really know how to change that. […] Ian Hodder still had to publish something as a result, even though it's a huge academic project, and it is just three volumes of pottery report. It's because even as an academic, you have obligations to the academic community to publish these things in a certain way. And I don't know whether they would have gotten away with it, actually, if they hadn't have done. The Turkish authorities probably wouldn't have been very happy about it if they hadn't put out a proper excavation report. (Interviewee 3, 2021)

Another contributor highlighted the difficulties which can accompany attempts to open up interpretation, and moves to make understandings of the past less hegemonical:

> …archaeology, as I'm sure you know, is just losing the battle, we are losing badly in pseudo-archaeology and nationalism, and various aspects of people trying to use the past for nefarious ends are just incorrect. And so whereas there was a big push towards multivocality, and opening up interpretation, there has been some interest in regaining some authority, or at least, trying to figure out what the sources of things are again. And so I think that's a really interesting and really fairly new and productive tension within archaeology. (Interviewee 9, 2020)

This theme was taken up by a participant who regretted the 'deactivating', 'demotivating' effect of museum presentations, the sense that they induced passivity from viewers, even in those cases where they explored alternative hypotheses of visions of the past, following the recommendation of the Seville Principles (§3.4.1):

> And what happens is you've asked people, 'so here are two 3D views, exert your critical faculties, which is more convincing to you, how would you respond to them?' And people would put them both on a completely equal level and remain passive to both, they would not engage with the evidence. […] Here's one version, here's version A, here's version B. I was inviting those open responses. But did the people coming along really think that I was inviting them to join the job of reconstruction? So I think that's where this kind of realism comes in. Let's be realistic about what we're really asking people. Are we really asking for wider engagement with the evidence? I guess museums would say that they are. But the museum history has also always been one of opening all the doors, but not the one to the engine room. (Interviewee 10, 2020)

The explicit handling of uncertainty, or the admission of inconclusiveness, was deemed acceptable in the circumstances of certain research projects, but it was more common to commit to an interpretation while including qualifications:

> So I think sometimes that that debate and uncertainty can actually be the focus of the article. It is actually what the article is about is debating form, structure, function, chronology, having lots of uncertainties. And so the article isn't trying to come up with a definitive vision of this, but rather, to air that debate. I think we can do that. In a lot of other circumstances, it's much more likely that we'll be saying, well, here's an interpretation of a particular phase of a site. And what we do is point out to other resources, which offer caveats or alternatives. (Interviewee 1, 2020)

This 'airing of a debate' was not unusual, another participant suggested, if the topic of a publication was a new find, and part of the purpose of the publication was to spread awareness of it. Otherwise, research articles without conclusions, or at least reasoned hypotheses, would be deemed to be of questionable value:

> So if you're publishing an object, which has come up, that's the new novel thing, you're saying 'This has been found. These are some questions. What do we know? Nobody knows'. And that's sort of acceptable. If I had come up with a research question that was like, 'How did people wear gold in Iron Age Britain?' And my conclusion was, 'We don't really know', then people might think, why did you write this article? If you don't know any of the answers, you might be more expected to suggest a likely resolution, or at least to point out broad things, but I do think it's still okay to say: 'These are two the two main possibilities. Either this or this—more research may help to draw that out.' (Interviewee 6, 2020)

4.3.8 Specialist knowledge and skills

My discussions with archaeologists brought home to me the range of specialisms in the profession and its dependence on multidisciplinarity:

> …nobody can be the sort of polymath that that existed perhaps in the 1960s version of archaeology. So I would rely heavily on building teams for any archaeological endeavour, so whether you're dealing with people more strictly within an archaeology discipline, ceramicists, environmental archaeology, pathology, etc. […]

> It's taken us into the whole area of climate science, hydrology, changing agriculture, the impact of the climate crisis... So it relies on that in a multidisciplinary approach. (Interviewee 1, 2020)

> It's kind of little ghettos. (Interviewee 2, 2020)

A useful insight was that archaeologists, as specialists of region, time period or method, look beyond single sites when constructing knowledge:

> …archaeologists themselves, typically, they're specialists. And as a specialist, they're itinerant, between multiple projects. So they already have a sort of an orientation or professional set of research goals that are typically aligned to look at multiple sites, regions and whatnot. They also look at a set of materials that are broadly similar, in time and space. And there are a lot of common conventions in recording, too. You know, anatomical elements, taxa, certain measurements. And there's a lot of areas where there's *not* a lot of alignment, and convergence. Especially for recording taxonomy and processing breakage, that type of stuff. But there are certain areas that do align across many datasets. And they also tend to have more of an inclination towards some sort of quantitative analysis. (Interviewee 5, 2021)

For these reasons, the interviewee suggested, data reuse was not surprising. This perspective is helpful in painting a bigger picture of the search, in some schools of archaeology, for broad patterns, often using quantitative means: an approach which contrasts with claims that archaeology has become increasingly concerned with the cultural and other particularities of individual sites (§2.2.1). This tendency of working across projects, looking to build up generalised patterns to support hypotheses, along with the danger of 'xeroxing' (see 'Classification', §2.3.3), might have the potential to undermine neutrality in recording and interpretation of local evidence.

The existence of a range of expert perspectives in archaeology raised the prospect of misalignments in understandings in multidisciplinary team projects. This was something certain interviewees were aware of, and they factored it into their understandings. Interviewee 1, when working on evidence with ceramicists, could see the 'different levels of interpretative assumptions layered onto that data.' At the same time he insisted that colleagues should adopt a 'critical approach to what they're seeing', question the

assumptions of their specialism and relate them thoughtfully to the context of the current project. One participant discussed the practical implications of specialists working on the same projects together by referencing Hodder's (2000) attempts (see §2.3.1, p51) to integrate diverse interpretations in a site excavation:

> …there's two terms we can use: multidisciplinary, which is often how people work, which means you have different specialists working, somewhat separated. Think of Ian Hodder's [project]. This was supposed to be the magical collaborative project of all time… but they just created ghettos. You know, intellectual ghettos and knowledge ghettos and communication ghettos, so nobody ever worked together even when he tried to get them to work together in an engineered way. The best way I find it working is doing something which I prefer calling transdisciplinarity. In other words, that you have all these different people from different backgrounds, different specialisms, but you share problems. And because you're sharing a problem, then you meet over the problem. (Interviewee 2, 2020)

The implication was that this kind of transdisciplinary approach was rare.

Trust and judgement when working with other experts came down to the reputation, of individuals or sometimes university departments, or extrapolation from areas of familiar knowledge:

> I'm very reliant on colleagues here who have expertise different than my own. If it's a question about material culture and art styles […] I'm well placed to weigh up the pros and cons of a particular argument. But ancient DNA, or just DNA generally, is increasingly feeding into archaeological narratives about the past. I have an almost zero understanding of DNA analysis. But really, very little, and I am not competent or qualified. If I read two genetic studies, and this has happened, I will read two genetic studies in two different journal articles, which will make different arguments about movements of people in the past. And I am not capable of weighing up the scientific part of the argument. And which, if either, is better. The social part of it, I can weigh up. But I just have to rely on the expertise of those individuals' authority. (Interviewee 6, 2020)

This highlights the primacy of thought communities for the validation of expertise and knowledge claims (see §4.3.1). Academic excellence and engagement with the scholarly

community were seen as important traits for project managers, who should be able to focus on sites and features of interest and worth, rather than pursuing an excavation by rote:

> If you bring in people who are at the top of their game, because they're going into conferences, because they're giving the papers, they've got the articles and journal and virtual sites in there. That peer review process, that engagement with the outside world of specialists, it's gold dust you know, and then they can run projects, and they know the difference in what matters and what doesn't matter. Rather than pursuing a dead boring project into the ground following step A, step B, Step C, when it isn't worth the candle, they can spot the things that stand out and matter and are worthwhile. (Interviewee 7, 2020)

At one level, this ability to pick out what 'matters' is self-evidently useful. At another, it points out the contradictory nature of a recording process which aims to be objective and neutral, and shows how disciplinary priorities might be self-reproducing.

For one archivist, the specialisms in archaeology made it very difficult to conform project archives to a standard format. In addition to dealing with the common issue of what he perceived to be 'incomplete data', he had to deal with different specialist approaches to the creation of documentation. In terms of mapping data from such diverse projects to fit into a common semantic structure such as CIDOC-CRM, he pointed out that:

> I just don't know how to really put it into practice in a way that makes a huge amount of sense, given the fact that so much of the data that we get is just built without that in mind at all. (Interviewee 5, 2021)

This serves as a reminder of the difficulties faced by those attempting to make data computable at scale (see §3.4.2).

4.3.9 Practical research considerations

In general, it was noticeable that while most interviewees were happy to discuss practical considerations, these processes were not what animated their interest in the discipline. Their research interests had theoretical forms and narratives which seemed to exist independently of the practical challenges encountered in pursuing them and the messy

processes of giving them shape. One commercial archaeologist interviewed had a more holistic view of research work however, discussing economics, project and people management alongside the development of archaeological hypotheses:

> So, and because the financial is linked to the archaeological, there is going to be a very clear relationship—you'll always make money on a Bronze Age site because we know what they do, and you will never make money on a Late Mezo site because we never do. And as you say, brick, earth—'up, piece of piss', gravel—'not bad', clay—'oh can we not bother?' But the biggest variable on whether we make or lose money is how near to our office the site is. (Interviewee 7, 2020)

Funding of research was of course a strong determinant on the lifecycle of a research project:

> …funding cycles limit this, so you hit a funding limit and then you want to carry it further but in the direction that institutions find uncomfortable […], so it doesn't get funded. Yeah, those kinds of things. Rarely do we get to do what we said we're supposed to do, which is actually follow through research and see if it was effective. That doesn't really happen. Very much. Of course we talk with friends—most of the curators are still friends so I talked with them. You know, we get feedback that way informally. (Interviewee 2, 2020)

The suggestion that institutional bias could be seen as blocking certain strands of research was rare in the interviews: more common was discussion about the influence of the parameters of research funding and the allocation of credit. It was pointed out by multiple participants that recognition for authorship of research was important for reputation and therefore funding opportunities. As noted in §4.3.4, this made iterative, long-term, multiple-authorship models of research problematic.

Naturally, income, costs and funding were important factors for archaeological work, but the limited availability of time was mentioned even more frequently. This was particularly relevant to the reinterpretation of sites, both in terms of authors spending more time to make site documentation more reusable and reinterpretable, and in terms of potential readers revisiting site archives:

> Commercial archaeological units realistically seldom have the funding and the time to be in a position to go back and rework and, and so it's much more likely to come in the context of a research project. (Interviewee 1, 2020)

Overall there was an acknowledgement of the incentive deficit noted in 'Reusability', §2.3.2: that researchers were generally short of time and more likely to invest resources into original research than revisit previous projects:

> Putting everything out there for people to reinterpret is fantastic. But people don't even have time to create their own sites, let alone someone else's. (Interviewee 3, 2021)

For one contributor working in the commercial sphere, the processes of fieldwork were designed specifically with efficiency and time savings in mind.

> …we're capturing data on site, through paper records for the most part. We will transfer across to digital capture, we've got some quite clever ideas for how we're going to do that. The aim is always, everything we do is to make the bloody thing quicker and better. It's never to create a layer of data entry that no one sees the point of, because then the system dies. (Interviewee 7, 2020)

One researcher suggested that the extreme shortage of time was a reason to move to a standardised documentation system, in order to streamline the process of working with existing data. Another commented that the creation of additional contextual information in site documentation was seen as time consuming and potentially onerous:

> …one of the drawbacks with all of these approaches is that it takes time and energy to do that. For the most part at least, it can't be automatically generated. […] And essentially, the whole argument for providing that contextual background, whether you call it paradata, or something else, is predicated upon somebody having the time and the energy or the inclination to actually capture it in the first place. Given that at the moment, there's no requirement to do it… there's no sort of ethical rule or whatever that says, 'you must do this'… and people have had enough difficulty getting used to the idea of providing the discovery metadata, let alone the paradata to go with this. (Interviewee 8, 2020)

Technologies presented new opportunities for capturing evidence on sites, but this brought its own difficulties in the shape of data volume and management:

> If you're thinking about technology, you can have 3D scanners or 360 degree cameras or whatever, and start capturing everything that's going on all of the time. Now, there could be something in there that would be useful. But that is a shitload of data that someone's gonna have to go through and store and timecode. […] But there does come a point where sometimes it's better to just keep it as simple as possible. Because at the end of the day, if you overload someone with data, you're not necessarily helping. (Interviewee 3, 2020)

A key concern with regard to archives, and digital records in particular, was access. This took the form of a desire for greater searchability and discoverability, as has already been discussed (§2.3.1); but also worries about the longevity of digital resources and availability and access in the long term. The long-term sustainability and funding of software platforms was a real concern, particularly in cases where they were specialised in function and developed by individuals, but also in cases such as Arches where there was a large community of contributing developers:

> …by definition, these packages tend to be something that's capable of doing what is a very complicated thing. […] But actually maintaining that on an even keel, so that there's a development programme that goes on for more than two or three years at a time—this is a real problem. And the nature of the hardware and software is that unless you do that, then it's got a very limited lifespan. (Interviewee 8, 2020)

4.3.10 Research management

While narratives about the goals of research were commonly expressed in terms of the ideals of understanding past cultures or recovering and sharing evidence, the interviews helped to reveal that this work was heavily shaped by the circumstances of its production. One interviewee commented about the compromises required when trying to undertake innovative research:

> So there's a there's a real tension there between interesting innovative archaeological practice and actually what your end product needs to be as governed

> by your academic REF publishing schedule, or your client, or the planning process, or whatever it is. (Interviewee 3, 2021)

A commercial archaeologist noted that a significant amount of his team's time was taken up with the management of the financial aspects of their work, and handling of the financial bureaucracy of the universities they worked with. The organisation of excavations and completion of documentation was highly contingent on practical financial considerations such as transport costs and the complexities of book-keeping.

A museum curator observed that while she was employed for her archaeological subject expertise, a significant part of her job's importance was a consequence of her position, as the 'keeper' of a collection, at the heart of a knowledge network, a community of experts. She was paid to manage and maintain a collection of objects, but the day-to-day reality of that job entailed managing and enabling a community of colleagues and collaborators.

4.4 Discussion

4.4.1 Facticity and certainty

In the interviews there was enthusiasm for the potential of technology as a tool for collecting and working with evidence. For those in the mainstream of the discipline, the relationship between evidence collection and the formulation of interpretations was a straightforward one, epitomised by a moderate form of processualism (see §2.2.3) which was cautious in its conclusions due to the limited nature of available evidence. Difficulties were identified by some in assessing the validity of scientific data and methods, as a result of the divisions of responsibility in archaeology. These tended to be partially resolved through consideration of reputation, and extrapolation via assessment of practice in familiar fields, such as standards in site excavation and data documentation. There was evidence of archaeologists developing skills in multiple specialisms, effectively resulting in new 'types' of archaeologist who were comfortable with data wrangling or drone flying, for example; but there was also evidence, for example in the references to the use of linked data, of resistance to engagement with specialisms which seemed too 'distant' from the goals of archaeology.

Conflicts of interest were also reported between specialisms: in one case, materials specialists found an object to be worthy of research because it was constituted of an unfamiliar material; but the object in question was not relevant for another archaeologist who framed her research questions strictly in terms of stratigraphic context, and therefore disregarded it on the basis of its find spot.

Concerning the proposals set out in the London Charter and Seville Principles (see §2.3.3), there was a sense that while they had not been set aside, they had faded from the forefront of archaeological research, and that there was little evidence of their influence in much current work. Interviewees reported that while the principles were sound, 'pragmatic' considerations tended to prevail. For example there were philosophical difficulties in representing paradata adequately, as well as resistance to the perceived burden of recording additional data. The relative lack of evidence for reuse of data or reinterpretation of evidence showed that this was happening infrequently. Multivocality, supported by innovative research and publication methods, was seen as a highly desirable goal, but one which has been hindered in academic publishing by the incumbent systems of reward and recognition, and resisted by museums (though not always individual curators), as representing a potential threat to their positions of authority and centrality.

There were examples given of how uncertainty was managed, and in some cases it seemed that measures of uncertainty would be preserved in the documentation of projects, for example in the shape of drawing conventions for boundaries, or even at times in numerical scales denoting confidence of interpretation. It was reported that discussions were still somewhat active among practitioners about the inclusion of indicators of uncertainty in images or data models, but not outside of these. The sense was that uncertainty information was not commonly included in final publications, where the priority tended to be to arrive at a conclusion or narrow range of possibilities, with the inclusion of caveats or qualifications if required; and that uncertainty was certainly not recorded systematically in the way which had been recommended by the London Charter and Seville Principles.

Those specialising in Digital Archaeology expressed concerns about the 'black-box' nature of software platforms and cataloguing systems, and the opaque nature of the transformations they performed on data in certain cases.

4.4.2 Understandings of technology in support of archaeology

Measurement, communication and representation using digital means have become a core part of modern archaeological practice, and thus in one sense, digital archaeology is commonplace. Those concerned with 'upper-case' Digital Archaeology, on the other hand, are focused on the philosophical implications of technology use for knowledge construction. This latter perspective is shared by those who are concerned with the affordances of networked communication, data processing at scale and speed, and algorithmic data analysis, and the impacts of such practices on archaeological knowledge; and those explicitly considering political issues such as feminism, diversity and equality in representation through the lens of digital tools, representations and channels of communication.
The role of technology in archaeological research was understood by interviewees in ways which reflected their respective professional concerns and priorities. These included:

- a relative lack of interest in the novel potential of technology: its use was integrated into everyday work, but tended to be shaped by or existed as an adjunct to non-digital practice, for example in the publishing of academic papers as PDFs
- an interest in using technology as a facilitator for improving established practices or enabling new ones, in order to increase accuracy, efficiency, access and accountability, both in the field and in publications
- the subject of research looking at how technology shapes or constrains practice
- a core part of experimental research outputs designed to explore the enabling affordances of technology for new archaeological practices, for example the creation of an immersive game environment which recreates a site from prehistory.

Of particular interest were two perspectives which were commonly discernible from the accounts given in interviews. The first, which I will call the 'techno-optimist' perspective, was based on a view of technology as a means of augmenting or streamlining existing

research practices, increasing efficiency and clarity and making data more analytically useful and discoverable. This tended to be a data-centric approach and associated technology with advances in collecting, analysing and otherwise processing data. When innovation happens in a field, this opens up new opportunities for research, and there is a natural migration of researchers towards those unexplored lands. Technological innovations in computing and other areas such as DNA and remote sensing have opened up such opportunities in archaeology, and there is no sign of this trend slowing. The dominance of techno-optimism may therefore be linked to this narrative of technological novelty and reinvention, and the sense that it makes available new resources to be mined, in the hope that, as Clarke has it, the empirical content of the new observations will have explanatory and conceptual significance (1973:11). It could be considered to be a kind of academic cross between surveying and prospecting—mapping out the land while at the same time hoping to find rich new seams of material.

For interviewees, an optimistic perspective had tended to prevail in spite of the fact that some of the promises of technology for research had proven extremely difficult to fulfil. Access to archaeological archives was still difficult, due to inconsistencies in the way project documentation was structured, and because of financial or other practical barriers to sharing documentation via repositories. Institutional provision of technology services was patchy. Ironically, the difficulties encountered were, in some important cases, a consequence of the continuous reinvention of technology—they represented the other side of the coin of the opportunities provided by innovation. For example, in the context of the fast-evolving nature of the web and web applications, it had been difficult to create sustainable software solutions for data collection, storage or publishing; and there were difficulties with hosting research projects online, caused by impermanent web server provision, patchy technical support and the uncertain long-term sustainability of software packages.

The overall perception, however, was that there had been real benefits from digital practices in terms of efficiency, handling data at scale, and improved access to records. The issues of sustainability arising from the constant cycles of innovation in software standards, and the fact that, unlike paper records, digital records such as databases require ongoing inputs of various kinds to remain available and in a state of good preservation, did not seem to be a deterrent for those embarking on ‘digital’ projects. There was realism about the

deficiencies in current technological practices but a willingness to believe that existing problems might be solved in the near future.

The second perspective, which I will call 'techno-scepticism', was one which was more aware of the political character of technology as a mediator or facilitator of knowledge—for example in its potentials for communication, decentralisation, access, diversity and 'speaking back' or 'speaking alongside'; or in its uses for classification and all that that implies. 'Scepticism' is not used here to denote rejection of technology, but to accent a critical approach to its use. The techno-sceptical approach tended to problematise the notion that data is a neutral resource to be collected and objectively analysed.
There was a consensus from those with this perspective on technology that work in this area was happening at the margins of archaeology and did not tend to have a significant impact on mainstream practice. In more than one case this was the source of some disillusionment and frustration. However, successes were reported in individual projects, and there was optimism that their effects might be felt more widely in the future.

Interesting insights were to be found in discussions with interviewees who had a foot in both camps. One archivist had been inspired to develop a digital archive platform in order to improve searchability and discoverability in research project documentation, but also to foster openness and inclusivity. This idealism was to an extent frustrated by the practical difficulties of conforming extremely diverse datasets, maintaining software and securing ongoing funding. There was a sense that the archive model used was relevant in practice mostly to projects cataloguing quantitative data, and that the service, while fulfilling a useful purpose, was not successful in all of its theoretical ideals.
A commercial archaeologist who had sought to innovate technologically based on concerns shared with techno-sceptical theorists, still felt inescapably obliged to produce 'conventional' outputs when documenting sites, and could see no way of bypassing this requirement.
From all perspectives there was hope that technological integrations into research could lead to a better version of archaeology. For some, 'better' meant greater efficiency, and more complete and accessible records; even after numerous missteps or dead-ends in decades of computing-based research, the goals of digital archaeological research

workflows were within reach and would be a reality within 2 years, 5 years or 10 years. For others, the priorities were openness, or the opening up of the discipline to different cultural and political perspectives.

Pragmatic considerations around the technical implementation of archaeological projects were seen as a filter on the development of technology use from both the techno-optimistic and the techno-sceptical perspectives. Sophisticated and temporarily successful technical projects were cited which had lacked durability due to the lack of long-term funding and over-reliance on the expertise of individual software developers. One interviewee's own software project had been successful in his community of users over the course of many years, but due to a lack of resources for updates had become technologically obsolete, as the version of the software language used to build it was no longer actively supported—confirming Latour's insight that 'For technology, there's no such thing as inertia' (1996:86). Software projects, like the fireboxes of steam engines, require continuous inputs to keep them running. Indeed, there are likely to be many agencies, at any given time, conspiring to 'unmake' them.

The interviews helped to illuminate the fact that there is resistance at institutional levels to technical and philosophical practices which might be seen to undermine the authority of existing experts and expert groups, or challenge their priorities. This is not necessarily an indication of the conscious assertion of power and control by individuals, though based on the experiences of some of the interviewees, the possibility of such actions are not to be discounted. It can be better understood as a kind of systematic inertia, which interviewees were conscious of, and sometimes frustrated by, as in the comments about the difficulty of securing research 'points' for publications which do not have conventional authorship profiles. The conclusion I take from this is that each piece of research produced with the expectation of academic recognition is a contribution to an edifice of knowledge which is both situated (in an 'esoteric thought community') and highly protective of licenses to contribute. It might therefore be concluded that the enthusiastic uptake of *new* digital tools would require those tools to *reinforce* existing institutional or thought community research priorities and embody the community's 'thought style'. Or to put it another way, they should share the same epistemological and ontological commitments. Based on the

accounts in these interviews, this seems very likely to be the case. (This is developed further in §6.) A key requirement put forward by some interviewees for new digital tools was that they should 'make life easier'. This might be understood as another way of saying that they should enable or streamline *existing* practice, not mandate new ways of working or the performance of additional tasks. However, this interpretation does not fully account for the evolving nature of academic communities. I have already suggested that new technologies can *open up* new landscapes for research exploration. My understanding is that for an academic community to flourish, it must operate in tension between the poles of established knowledge, expertise and authority on one hand, and the allure of the unknown, unverified and undiscovered on the other. There must be a prospect of renewal or development in order to make new and ongoing work in a discipline meaningful and worthwhile. Therefore, there will always a place for research initiatives working at the edges of mainstream practice, exactly because they help to guarantee this prospect of renewal. At the same time, they help to clarify the definition or essence of the dominant practice, by outlining its boundaries and setting them in relief. The *dissatisfaction*s of those interviewed can be read in this light, as part of negotiations to determine the identity of a thought community. They can be seen as an expression of the necessary tension which is required to keep a discipline current and vital.

At what point, then, are those practices which are in tension with the mainstream of a discipline able to move from the margins to the centre ground? And do technological research tools simply hold a mirror up to the research assumptions of the day, or can they themselves be enablers of or catalysts for change in archaeological understandings, as well as in the technical execution of research? Clarke, a 'New Archaeologist' in some respects (Trigger, 1998), pictured the archaeological tradition as highly stable, built on a broad and many-pillared base and able to absorb the shock of the new—which in his case, consisted of the implications of empirical scientific analyses—but only up to a point, at which point it must adapt. He emphasised the 'revolution' brought about by emerging technological innovations in data analysis, leading to a passage from consciousness to self-consciousness in the discipline. He described what he saw as a further potential move from self-consciousness to *critical* self-consciousness as 'a philosophical, metaphysical and theoretical one' (Clarke, 1973:7), brought about by the consequences of the technical revolution:

> Once again, epistemological adaptation to the empirical content of the new observations is of no less significance than the explanatory and conceptual adaptation now required to understand them. (Ibid, p11)

However, Clarke 'explained the development of archaeology in evolutionary and adaptive terms' (Trigger, 1998:695), in keeping with his approach to archaeological understanding. There was no role in his account for ongoing traditions, since 'better adapted systems of knowledge obliterated less well adapted ones' (Ibid). Trigger sets out a different perspective:

> The history of archaeology also demonstrates that, because the specific understandings of each period are what are transformed by new understandings, changes in archaeological interpretation and practice must be understood historically, rather than simply developmentally, as Clarke had assumed. Established interpretations often survive unquestioned long after the theoretical presuppositions that gave rise to them have been refuted or become unfashionable. (Ibid)

Are the archaeological pillars so strongly rooted in existing traditions that they have managed to withstand the *epistemological* implications of new perspectives, in spite of Clarke's claims? According to some of the archaeologists I interviewed, any movements away from processual approaches in archaeology have only been temporary or superficial ones. Some participants argued that the predominant epistemological mode in archaeological work now is a 'soft processualism': a moderated version of the positivist New Archaeology, more aware of the implications of post-processualist approaches which reject positivist readings of the past, but still caught up in the mechanics of practices which embody a processualist world view.

Clarke's account of the development of a critical self-consciousness in archaeology is based on the rejection of 'the mistaken belief that there is one universal form of archaeological explanation structure appropriate at all levels, in all contexts' (op. cit., p15). He separates the discipline into a series of interconnected phases of theory: pre-depositional, depositional, post-depositional, retrieval, analytical and interpretive. Taken *together* these constitute archaeological interpretation, and:

> with archaeological metaphysical theory, epistemological theory and logical theory they clearly together constitute the nucleus of that theory—currently intuitive or unsatisfactory but gradually being specified—which makes archaeology the discipline it is and not merely the discipline of its operations, whether artistic, mathematic or scientific. (Ibid)

This breakdown of the anatomy of archaeology enables Clarke to note where 'social theory' predominates (pre-depositional and interpretive phases), as opposed to say, 'environmental studies', and allows him to observe that:

> new ancillary methods do not alter the intrinsic nature of the discipline and we must not suppose that because we can display an archaeological relationship mathematically and analyse archaeological data scientifically that the discipline itself necessarily assumes a mathematical or scientific status. But equally neither may we assume that, because we describe archaeological observations in a literary form and interpret our data imaginatively, the discipline is a free creative art. (Ibid, p11)

The 'fundamental lesson' is that 'the consequences arising from the introduction of new methodologies are of far greater significance than the new introductions themselves' (ibid). While this concern was echoed by some interviewees, they also reported that it was not obviously manifested in the standard practices of archaeology. A focus on the technical, technological and empirical side of archaeological practice seems to be a comfort zone for many practitioners in the field, at the expense of the other 'pillars' of the discipline. Sørensen's scepticism of the idea of a new scientific revolution in archaeology seems particularly appropriate in this context (see 'Data at scale', §2.3.3). He believes that the discipline's 'approximation to science has produced a growing suspicion towards interpretations that cannot be scientifically proven or quantified objectively' (2017:102), and relates this, and the 'fetishisation of data', to a 'Scientific Turn' (ibid). I would reframe this 'turn' in terms of Manovich's 'computerization of culture' (Manovich, 2001:198), and suggest that the instinct to *instrumentalise* evidence, to make it computable or technologically processable, has tended to trump other ontological understandings, diminishing, in archaeology, the nuance provided by Clarke's differentiation between 'social' and 'environmental' studies. Clarke notes that the archaeological excavator with a 'more explicit theoretical awareness' may 'contribute to a qualitative increase in understanding

rather than simply a quantitative increase in data' (op. cit., p18), while those who 'believe themselves to be quite exempt from any intellectual influences are […] usually the unwitting slaves of some defunct theorist' (ibid). The implication, from those interviewed, was that Clarke's vision of a *technologically* engendered *critical* self-consciousness has not, after all, been arrived at.

Trigger points out the intransigence of research practices, even as theoretical ideas and ideals change. If, as Renfrew and Bahn state, the 'the key elements of modern field research' are those introduced decades ago by the New Archaeologists (2016:42), we might consider the extent to which technological choices made by archaeologists are in fact an expression of their professional identities, and any ongoing reluctance to change them a sign of the importance of the conception of what archaeologists actually *do*, which is another way of understanding what they *are*. Lemonnier cites the case of French wine-farmers who continued to plough their land after the introduction of weedkillers:

> Since the efficacity of weedkillers is undisputed in this case, one is led to ask people about this technically non-required plowing. As the reader will see, what is at stake here happens to be the local representations of a 'well'-plowed field (supposed to be perfectly clean), also the idea that the ground must be 'worked,' and, above all, the status of the plowman in this particular rural society. (1993:17)

The ground should be 'well plowed' (sic), and the 'plowman' should be *seen* to work the ground *properly*. This brings out, by analogy, an aspect of research work which I will discuss later: there is an *ethical* dimension to the process, a need to be *seen* to be doing the *right* thing, and a need to demonstrate the correct *character* as a researcher. These considerations are, I suggest, deeply entwined with accepted methods and conventions of research and concomitant resistances to alternative practices. In a 'computerised' culture, the ethical aspect of technology-enhanced instrumentalization of data is likely to be strongly linked to the ideal of objectivity.

* * *

The practical implications of what emerged in the interviews was that for digital tools to enjoy popular uptake, they should in some way make people's lives easier (which means supporting what they already do and know, but more efficiently); they should not be expensive or technically complex to implement and maintain; and they should not be too closely contingent for their ongoing development and sustainability on the expertise of an individual or a small team. It would seem that at least for now, digital tools which seek to embody new *epistemological* perspectives in archaeology may find short-term funding and provide useful contributions to the academic discourse, but are not likely to be welcomed into the 'body of the kirk'.

5. Study 2—Observations

5.1 Introduction

A key part of my original PhD plan was to observe archaeologists first-hand, to get a view of how evidence is recovered and transformed into documentation, and subsequently how interpretations are made. The goal was to observe, as much as possible, the whole 'archaeological pipeline' of knowledge production. As things turned out, this plan was complicated by a number of factors. When studying for an MRes qualification as a precursor to my main PhD studies, I interviewed a range of professional archaeologists about their research practices. As a consequence I became fully aware of the very wide range of roles which constitute the profession of archaeology and the sometimes distant connections between them. A philologist with a background in linguistics has little in common with a museum metallurgist who studied chemistry, except for the fact that they are both concerned with finding out about the past. Their paths may cross occasionally in a project which brings a range of experts to bear on a shared pool of evidence, but otherwise they operate in quite separate academic fields which have their own traditions, conventions, institutions and publications.

The implication of this highly-developed level of specialisation is that it is not straightforward in modern archaeology to find researchers who are actively involved in every stage of a project, from trowel-work in the field all the way through to spell-checking a report or article for publication; and in fact this is a selective ideal of what the profession consists of. Some types of archaeological work do not begin with or proceed from the results of excavation. Many in the profession will spend a career focused on one aspect of archaeological research, analysing historical records or evidence using a specialised technique, or developing database schemas to organise archives, for example.

The mature specialisms which have emerged as a result of the eclectic scientific and critical practices encompassed by the discipline make life complicated for the would-be observer of archaeological research. Viewing the whole 'pipeline of production' may be possible when following a field project from dig through to publication (though even in this case, the practicalities of observing deskwork and the 'writing-up' process present philosophical and

technical difficulties), but my MRes research showed me that this notion of an integrated process, conducted from start to finish from a single unified perspective, is an idealised concept of the work of archaeology. It may function as a useful conceptual paradigm for those looking on from outside, and for those working in specialisms and contributing to a research programme; but it is not representative of the sometimes disjunctured work which constitutes archaeological research. To make sense of the workings and assumptions of this broad field, an understanding is required which can account for the separation of evidence collection from interpretation, and the handing off of discrete resources to specialists.

The other complicating factor for my planned observations was Covid-19.
The Covid-19 pandemic resulted in periods of restrictions on social interactions in the UK, including on research fieldwork, with the first lockdown starting on the 26th of March 2020. In April 2020 it became apparent that observations of archaeological fieldwork for my project were likely to be problematic, and that due to continued uncertainty about the future relaxation of lockdown rules and other restrictions, deferring observations until later would be a high-risk strategy.
As most 'non-essential' workers were working from home if they could, the Zoom[19] and Teams[20] meeting became, out of necessity, the default mode of communication in many spheres of work, including academia. The next best thing to observing archaeological fieldwork, then, was to observe the continuation of research projects via the medium of video-conferencing meetings. I was very grateful to have the opportunity to observe the regular online meetings and workshops of an archaeological project which brought together international partners to document archaeological remains in an extensive landscape, primarily via the interpretation of satellite imagery. This opportunity came about through the recommendation of academic colleagues who suggested project leaders to approach, none of whom I had prior knowledge of. One of those who I approached speculatively was generous enough to give me observation access.

[19] https://zoom.us/
[20] https://www.microsoft.com/en-us/microsoft-teams/log-in

5.1.1 Terminology

I will refer to the research project I observed as the Remote Sensing Documentation Project, or RSDP, in order to preserve its anonymity. Throughout this chapter I will discuss one of the main activities of the documentation project team as 'mapping'. This makes use of the team's own terminology, and does not refer to cartography, or the creation of maps; rather the 'mapping' being carried out is the addition, to existing maps, of references to archaeological features. In practical terms, this consists of using Geographical Information System (GIS) software, such as QGIS[21], which has a map-based user interface. Vector shapes are drawn over maps to delineate landscape features which are taken to be evidence of items of archaeological interest. Viewed in aerial photographs via the GIS interface, these features might be the remains of buildings, infrastructure such as aqueducts or roads, or distinctive marks in the soil or in crops which indicate historical human activity, for example the buried remains of field systems. The GIS software's interface allows for a series of selectable layers to be superimposed upon each other, not unlike the marked-up transparent sheets of an overhead projector (or for a more up-to-date digital analogy, the working layers of a Photoshop image). One layer contains a base map, another an aerial photograph, a third the drawn-in vector shapes which outline archaeological features, and so on (see Figure 4 for an example).

[21] https://www.qgis.org/

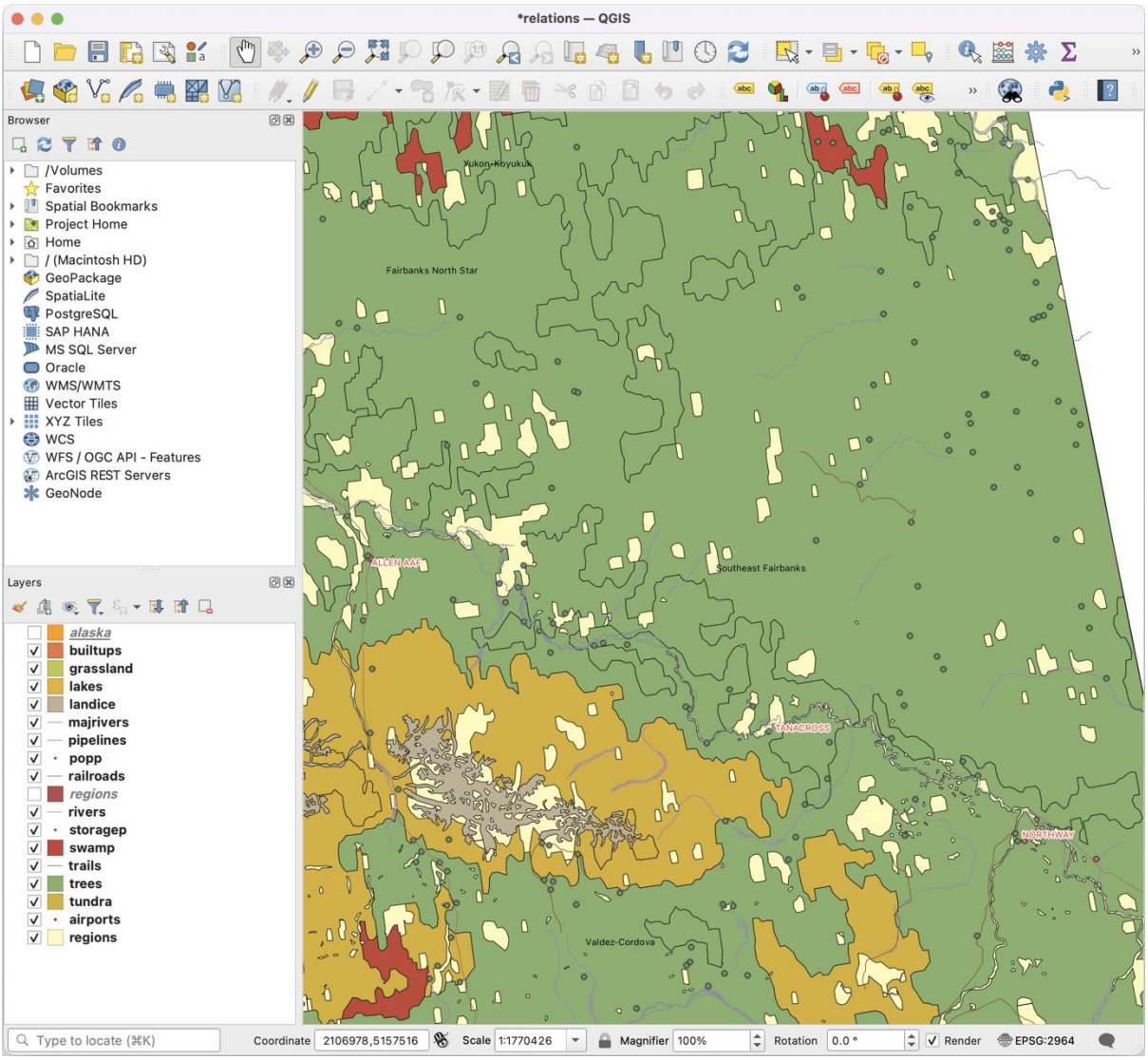


Figure 4. QGIS software interface, showing layers and vector-based shapes.

5.1.2 Remote observation

Limiting my observations to this project meant that the ideal of monitoring the whole pipeline of archaeological research would not be practicable. However in view of my revised understanding of the nature of archaeological research, this did not seem to be an unsurmountable drawback. Observing fieldwork and the capture of evidence would have been useful and interesting, but the requirement to make sense of evidence produced by others is clearly a common if not typical one in archaeological research. The RSDP was therefore representative of the working methods of many in the discipline. It also provided some advantages: its outputs converged around the population of an online database-

driven software platform for documentation, making it particularly interesting for my consideration of technologically mediated research. It involved a wide range of contributors, with different skillsets and levels of expertise, from various geo-political backgrounds. As I was privy to planning meetings, I could garner information about the management of people, skills and resources, and their agencies with regard to research outcomes. The project made use of student contributors, so it included online training workshops on interpreting and mapping archaeological features using aerial and satellite imagery, giving insight into technical procedures used, and setting out expected standards and philosophical assumptions about what is knowable.

Indeed, the fact that most of the project's meetings took place in the online space (partly due to Covid, and partly due to the international nature of the team) meant that I was able to observe more conveniently, freely and frequently than I would have been able to in an 'offline' or hybrid project.

The drawbacks of observing remotely are more difficult to enumerate, as I have no archaeology-related on-site observation experience to use for comparison. It is reasonable to assume that some of the intra-team signalling which might have been normally communicated by body language, seating arrangements, informal groupings, or other cues, was muted or obscured by the online meeting format. Almost all participants were working from home so there was less insight to be gained from the physical context of meetings than would have been the case were they in offices or labs, though professional or economic status was in some cases hinted at via webcam, by the living spaces of meeting members. Institutional surroundings might have been more useful for imparting information about working practices or technical equipment.

In some cases participants did not normally activate their microphones or webcams in meetings, and were therefore both muted and invisible, aside from a text-based placeholder avatar, unless they chose to turn on their mic to make a verbal contribution. As an observer this gave me little or nothing to go on, other than some insight into the varied commitments of team members to the video-conferencing format and their levels of comfort with it; or their own sense of agency and importance in the meeting.

Presenters in online meetings often shared their computer screens to discuss mapping techniques or examples of satellite imagery, and in workshops there was some detailed discussion and demonstration of software techniques and the documentation process. This

was mostly with previously completed work however, and there was no other opportunity to observe the everyday mechanics of the mapping work and the embodied or tacit aspects of the management of documents, digital artefacts and software tools.

5.1.3 Participants—Project Team

The team had dozens of international members, but the administrative core consisted of colleagues who worked as academics at the same institution and had in-depth subject expertise. In the project's online documentation, only the primary investigator was given a named role, but based on my observations there were three colleagues with special organisational responsibilities. Of these three, the primary investigator (who was also the project leader) was the liaison with project partners and funders, and set out the overall methodological expectations, technical goals and limits of the project; one colleague led on the technical aspects of the mapping effort and data integration, and coordinated the team training; while a third provided expertise on the archaeology, landscape and culture of the area being mapped. In spite of this allocation of specialised responsibilities, all three had extensive general experience as archaeologists including international fieldwork.

Other team members fell into the following categories:

- Archaeologist or cultural heritage expert with a designated project role (e.g., leader of Climate Research subgroup, head of Condition and Risk Assessment, etc.)
- Working archaeologist (often in an academic role) with an interest in or specialised experience of the region, who had been assigned a geographical area to map.
- Student archaeologist who had been assigned a geographical area to map.
- International partner participating in the mapping effort, providing local expertise and data.
- Technical specialist, for example:
    - systems administrator for the software platform being used
    - cultural heritage data scientist
    - remote sensing specialist brought in to train participants in the interpretation of satellite imagery

- graphic designer tasked with producing maps and diagrams

The three core members of the team were male, and of a sufficient age to have accumulated the professional experience and expertise which granted them authority. Otherwise the team was quite evenly divided between male and female membership, with research subgroups in the project most often led by female members.

5.2 Observation report

5.2.1 Details of observed meetings and workshops

In my analysis I differentiate between the regular project management meetings and one-off workshop and topic-focused discussion meetings. Broadly speaking, the former were most useful for insights into project management and the diverse institutional agencies shaping the project, while the latter gave insight into mechanics of the project, software packages, detailed research practices and expectations around interpretation of evidence.

19 management meetings were observed, between November 2020 and July 2021. In the same period, an additional 15 workshops or focused topic discussions were also observed. Of these workshops, two were presented by guest speakers from outside the team, and were less discursive, though they included question and answer sessions at the end.

At the beginning of my period of observation, the project management meetings took place online weekly, and were generally well attended, with typically around 20 participants. These meetings typically lasted between 70 minutes and 90 minutes. After a few months they moved to a fortnightly frequency, as the project progress slowed due to Covid restrictions.

Workshops also took place in the online domain. These were one-off events which took place at various times in the week as required to suit schedules, and had fewer participants, typically up to about 10. The function of these was to upskill team members and review or quality assure the instances of mapping work which had been carried out. Occasionally

visiting speakers would give presentations about specialist knowledge and these sessions were more widely attended.

The hierarchy of expertise was most apparent in the 'upskilling' workshops. These consisted of a group of 3 or 4 experts providing examples of interpretative work for the benefit of the other attendees, who were students or post-graduates. As the project progressed, the 'apprentice' members were asked to present the mapping work they had completed, and comments and advice were contributed by the expert group. The expert group and the apprentice group were of different age generations, the latter mostly in their 20s, the former more or less middle-aged or older, and this cemented the impression of an apprenticeship model. Other team members contributing mapping efforts, who were of the same generation as the expert group, were either not invited to have their work assessed and discussed in this group context, or chose not to attend.

In the case of visiting speakers, the wider range of attendees was a tacit acknowledgement of the visitors' specialised expertise, and the fact that even experienced professionals would have something to learn from such events.

5.2.2 Recording and coding meetings

It had been agreed in advance that I would audio-record online meetings which I was observing. In addition, workshop sessions, which often involved screen-sharing, were video recorded by the team themselves, and these recordings were stored in an online repository for the benefit of team members, which I was given permission to access.

My method of audio-recording and taking notes at meetings was as follows. Using the digital audio workstation software Reaper[22], I was able to record the monitor output of my own computer, which during Teams video-conferencing sessions, consisted of all meeting-related audio. The audio from each meeting was recorded as a separate Reaper project. As a given meeting unfolded I would use a keyboard shortcut to insert an automatically numbered marker on the Reaper timeline, whenever there was a point of interest in the discussion. In dedicated notebooks, I would simultaneously summarise the point of interest corresponding to that numbered marker. This gave me a simple coding system for topics of interest and a set of virtual time-referenced bookmarks with which to find them in the

[22] https://www.reaper.fm/

recording. I subsequently went through the paper notebooks highlighting the most salient material. These physical notebooks, inspired in part by Latour's example in 'writing down risky accounts', and foregrounding the study itself as a source of uncertainty (Latour, 2005:133), were kept separate from digital notes which fulfilled the functions of a project log and 'writing trials' (ibid).

As the workshops were of particular relevance to my understanding of the project team's interpretative processes, I coded transcriptions of audio recordings of the most interesting of these, using the Atlas.ti qualitative analysis software package. This amounted to 815 codes from 11 workshop and focused discussion meetings. Due to the large volume of total material—well over 40 hours of recordings—transcribing and coding all of the audio recordings was not practical. I therefore referred to my notebooks when analysing the team meetings.

5.2.3 Procedures

Permission to observe sessions was first of all obtained in writing from the project leader. He in turn circulated an information sheet which I had provided about my research to the rest of the team, to give them an opportunity to object to my presence at meetings or the terms of my participation. No objections were raised.

When discussing the possibility of observing meetings with the leader of the research project, I made it clear that I proposed to sit silently 'in the background' without interfering in any of the discussions or activities, with the goal of minimising my own agency in the proceedings being observed. With my agreed status of passive observer, it would have been inappropriate to interject in any of the discussions. There were a couple of events which were exceptions to this: the first being at the start of the period of observations when I was introduced to the team in one of the meetings, and gave a brief introduction to my PhD research project; and the second when I raised the possibility of a software-based collaboration with the project leader, and was invited to summarise my areas of interest to the whole team, in one of the meetings. This latter was at a point after I had observed many meetings over the course of about 6 months, and felt that I had ample material to report on the project before any collaborative involvement took place. As it turned out, there was no clear opportunity for a collaborative phase with the team, so the 'passive' observations

resumed as before for a further 2 months. The desire to be a 'neutral' observer, to whatever extent that might be truly possible, discouraged me from pursuing the option of interviewing RSDP team members in a separate context. I felt that my own research agenda might, however indirectly, find its way back into the team's work, in the case that interviews were conducted in parallel with the observations. I had also, in the previous phase of my research, interviewed archaeologists with similar areas of specialism to those being observed. I therefore found it useful to maintain a clear distinction between the modalities of the reflexive, explorative interviews, and the everyday dynamic processes of a research project in action.

The process of observing meetings and workshops over an extended period felt in some ways unnatural. My practice was to join meetings with my microphone muted but my webcam enabled, to make it clear to the team that I would be participating in the session, and to project a friendly and accessible presence. After giving a quick wave to those team members who acknowledged my presence I would disable my webcam for the rest of the meeting, with the aim of being as unobtrusive as possible and minimising my agency in the meeting. Over the course of many months, this minimal level of engagement with team members started to feel uncomfortable. Undoubtedly in the case of face-to-face observations there would have been opportunities for small talk at the beginning and end of meetings, but this did not take place in these online meetings, due to the liminal nature of my engagement. At the same time I became increasingly familiar with team members, their verbal quirks, senses of humour and areas of interest—I developed the feeling, in short, that I 'knew' them. This led in the final months to an increased sense of strangeness and self-consciousness about my continued participation, when I considered that they did not in turn 'know' me, yet I was continuing to join and regularly observe them. The sense of discomfort was presumably because of the one-way nature of the relationship and the feeling of being an 'outsider' in a close-knit community. This feeling was later assuaged by having one-to-one discussions with some of the team members after the period of observation was completed.

To what extent was my presence in fact a 'neutral' one? Given that I had shared the themes of my research with the team—the emphasis on capturing subjectivity, the uncertain and

the interpretative aspects of archaeology—it is possible that my presence at times encouraged a more conscientious or measured treatment of such considerations than might otherwise have taken place. There were occasional jokes or references made about my presence and the notion that judgements might be made of the team's work, though these were few and far between, and mostly at the start of the observation period. Given the consistent nature of the team's approach to research over many months of observation, I concluded that my presence in the online meetings had little effect on contributors' behaviours, and indeed that most team members probably had little interest in my perspective; unsurprisingly their focus was on the project's goals and progress and their own contributions to these. My Teams avatar (an abbreviation of my name, 'MK', rather than a portrait photograph) was often one of many which remained inactive throughout meetings. At times there were discussions about inter-departmental politics and administrative frustrations, and again, it may be that, especially at the start of the observation period, these discussions were moderated slightly because I was 'in the room': on the other hand I had made it clear that I would maintain strict confidentiality and anonymise all content in my research reporting, so this may have had a reassuring effect if participants noticed my presence. I got the impression, rightly or wrongly, that I was trusted on this score, because I was a mature student with professional experience, my motives for participating were transparent and my concerns likely appeared to be more with philosophical matters than with political ones.

5.2.4 The Remote Sensing Documentation Project

The first team meeting I observed happened to be one of the earliest full-team project meetings. The primary investigator laid out his vision for the project and invited the meeting participants to contribute their perspectives on goals, and how they might be achieved. This highly inclusive approach, pitched as an invitation to participate and to help define the project agenda, set the tone for a complex project which was at a formative stage and required input from a number of specialists and other contributors in order to be able to succeed.

The overall aim was to synthesise knowledge about the areas being surveyed, to publish it in accessible forms, and broaden access to existing scholarship on the regions. Finding the correct balance between online and printed publications was identified as a central issue to

be solved. In an indicator of the importance of inclusiveness to the project, it was noted that the partners abroad were more likely to see career benefits from printed publication credits than from the creation of digital resources. The interests of international partners and support for regional scholarship continued to be high priorities in the project throughout my period of observation.

In this first meeting, various members shared their preliminary thoughts about the design of the project, through the lenses of their own specialised interests: graphical representations, database design, regional scholarship, and so on. It was agreed that all design questions should proceed from the foundation of the research agenda—the nature of the narratives to be told. Some possibilities were explored for this—for example, the use of biographical accounts of cities was suggested as a basis for narratives—but it was acknowledged that different scholars would approach the documentation process with different goals in mind. This was seen as a positive feature rather than an issue to be solved. Somewhat contradictorily, the same conversation generated a consensus that standardisation in documentation would be vital for a project working at such a large scale to succeed.

Based on the sum of my observations of team meetings and workshops, I have set out in Table 2 what I understood to be the key goals of the project:

Table 2. Key goals of the RSDP project.

Key Project Goals	
1	Identify and document archaeological features in the specified landscape, including previously undocumented features
2	Assist regional teams with the digitisation of their archaeological records and provide a platform for the publication of their scholarship
3	Link to existing publications about region, improving access to this body of work
4	Create an international research network of scholars specialising in the region
5	Assess condition of archaeological monuments and other features
6	Assess future risks to preservation of archaeological monuments and features
7	Demonstrate the value of remote sensing imagery for archaeological interpretation
8	Train and empower local communities in documenting and conserving cultural heritage
9	Publish and share data online and in printed form

5.2.5 Team Meetings

Team Management

The general team meetings were a rich source of evidence of the complexity of the project, as the members attempted to coordinate the creation of a technological output from diverse inputs. The multi-levelled processes of coordination and translation involved in moving information from various international team members into a coherent and consistent database format was akin to sliding the parts of a complicated and sometimes temperamental machine into place. Members joined from various time zones around the world, and the meetings were scheduled to accommodate this, though for some participants the best available compromise meant that meetings finished in the early hours of the morning in their local time. For some, English was not their first language, and there was some difficulty and evidently some degree of discomfort in reporting to the group verbally in English. In such cases a bilingual member of the team would sometimes intervene, have a short discussion with the contributor in their native language, then report back to the team on their behalf. The tone of these meetings was informal and supportive,

and such complications were accepted as a normal part of such an international project. However these linguistic differences did at times affect the quality of the information conveyed from regional teams, and it was clear that in spite of an inclusive policy in the project, the expectation was that *lingua franca* of the project should be English.

During the months in which I was observing the project, its progress was complicated by international Covid outbreaks, and regional reports often became a mixture of updates on the numbers of staff who were ill, in some cases extremely seriously, or in isolation, along with news of the everyday work related to mapping tasks or administration. In such unusual circumstances, where the majority of team members were adapting to new working conditions and restrictions, it was unsurprising that expectations of project progress were to some degree relaxed.

Some international partners were conspicuous by their absence in these meetings: one small international team had a representative who was present in every meeting, but the head of her team, a senior academic, continuously provided excuses for failing to join. This raised interesting questions about the ownership of the project and the benefits to partners. As noted above, the project was explicitly designed to be inclusive, to make use of local expertise and to represent local voices. There was also the intention to provide training for regional partners and share expertise with them, and the documentation of archaeological sites was pitched as a potentially valuable resource for local policy-makers such as those making planning decisions about developments in the regions being mapped. No doubt these proposed benefits held more promise for some partners than others. I was not privy to budgeting arrangements, so have no idea if the allocation of funds was a factor for partner engagement. Discussions between core members of the team made it clear that preservation of cultural heritage in the region being studied was one of their top priorities, but inevitably for those living in these areas, the relationship to local monuments and archaeological remains was nuanced, involving, as it would, potential conflicts between conservation and development.

In Callon et al.'s account, if a project such as the RSDP is to succeed, it must make itself somehow indispensable to the implicated actors, either by 'seduction', 'pure violence' or 'simple bargaining' (1986:26). In the case of this project, the general management approach was a very relaxed, consultative one, which assumed the prior engagement of (human) partners and motivation on their part to contribute. At one level this was an

acknowledgement of the need for regional partners' participation for the sake of the project's credibility, and a recognition that it depended heavily on their good will to succeed, even though it might also have represented indirect career-advancement opportunities for some participants. Underlying this non-coercive approach was a strong aura of authority located both in the primary investigator and the host institution, which meant that the vision for the overall project was never strongly challenged by other team members. It was also noticeable that in spite of the consultative style of the team meetings, the less senior members of the team rarely contributed unless asked to. While these meetings were very relaxed, they were an example of specialists operating in an expert domain which was for some participants, it seemed to me, daunting to participate fully in. Wary of appearing ignorant or breaking protocol, or perhaps simply not keen to take on unwanted responsibilities, many preferred to remain silent.

Research design

The first meeting I observed was oriented towards the documentation of archaeological features in cities. Discussions and negotiations about the final shape of the project outputs commenced almost immediately, and some of the significant agencies which would contribute to this became quickly apparent.

One of the first points of discussion was the amount of detail required, both textually and graphically, for the effective documentation of archaeology. It was noted that for some sites there were large bodies of existing scholarship, and that therefore a process of *filtering out* information would be required rather than one of gathering evidence. The goal in such cases was to provide a synthesis of scholarly knowledge while including a comprehensive bibliography of more detailed sources.

The principle of standardisation asserted itself at once. If mapping imagery was to be used, a set number of scales would need to be decided upon, suitable both for large cities and smaller areas, to make resources easily comparable. If graphics were to be used for the condensed presentation of information, certain categories would need to be used to decide what features to include and how to visualise them, for example using colour codes. Categories might be based on hierarchies of building types. Hierarchies and categories might be split into presentational layers, to make complex data easier to navigate. If chronology

was to be the basis of the research narrative, settlements could be categorised into periods of foundation, development or end date. And so on.

The primary investigator and others were at pains to point out the need for the research narratives to drive the shape of the documentation objects:

> …we do need to find a place within our narrative structure to be able to talk about that bigger landscape, which is really where we were going with the archaeological maps […] And they'll end up when they're digitised as a whole series of components, and we can link them all together. But somewhere, one needs to talk about the survival and the condition of that whole thing. […]
>
> And we've talked about this in Arches, in QGIS terms of how we link things together, and how we can have overarching groups of things. […] Are you writing a condition assessment about every little bit of an aqueduct? Yes, but are you also writing something somewhere which talks about the whole thing? (Team member, 2021)
>
> …we can […] merge the more quantitative approach that we are taking with a qualitative and observational approach, which makes it an even better regional assessment… (Team member, 2021)

In parallel with these discussions, certain software tools for manipulating and representing data were discussed, namely Adobe Illustrator[23], QGIS and Arches. Could they 'talk' to each other? How could a map be made queryable (searchable)? To what extent was the database structure of QGIS compatible with that of Arches? Quite quickly, these software packages emerged as the central mediators of data in the project, and sites of negotiation for deciding what to keep, what to exclude, and how to mark out difference. QGIS in particular, as the main working tool of many of the participants, became the primary template and yardstick against which content was developed. Together with its layer-based UI for handling complex graphical and numerical data, it also provided a sufficiently flexible database structure for designing content which could then be exported to the final presentation platform, Arches.

[23] https://www.adobe.com/uk/products/illustrator.html

5.2.6 Translations

The discussions in the team meetings helped to reinforce Lucas's point, that:

> the shift from site to archive should not really be viewed as a shift from the material to the conceptual or representational realm at all, but as a translation of material properties from one material form into another. (Lucas, 2012:238)

Some of the documentation in this project was far from being a 'total record' (see §2.2.1)—for example the collections of archaeological traces detected in satellite imagery. Nevertheless, at each stage of the documentation process, each representational mode became subject to its own realm of logic and dependencies, circumscribed by the affordances of its media and the concerns of its authors. To take one example, the graphics specialist prioritised clarity, aesthetics and usability in her work, which collectively resulted in a wish to use a limited colour palette for cartography, one which was clear, attractive and accessible to viewers with colour vision impairment. Thus, in the translation of site maps to Adobe Illustrator, conceptual filters and augmentations were applied which had no relation to the ontology of the original site. The distinctive ontological nature of the document objects was further reinforced by the principal investigator's guidance that the scale and scope of presentations of sites should relate to the number of *existing* studies about each site, rather than the *physical* scale of the original sites, or any other notion of their historical significance.

Following the example of ANT, we can understand the documentation process as part of a 'translation', a consequence of actions to stabilise the identities of archaeological features and sites:

> …all the actors we are going to deploy might be *associated* in such a way that they *make others do things*. This is done not by transporting a force that would remain the *same* throughout as some sort of faithful intermediary, but by generating *transformations* manifested by the many unexpected *events* triggered in the other mediators that *follow* them along the line. (Latour, 2005:107)

In fact, Latour suggests that ANT should be more accurately labelled 'sociology of translation' (ibid, p106). Callon et al. make it clear that such translations set limits on how interactions can take place:

> An actor-world associates heterogeneous entities. It defines their identity, the roles they should play, the nature of the bonds that unite them, their respective sizes and the history in which they participate. (1986:24)
>
> Each entity is thus reduced to a few properties which are compatible with the relationships established between the entities. (Ibid, p34)

'Translation' in the ANT sense refers to the enrolment of 'interests' (*interessement*) of other actors: this can also be understood as the setting of an agenda or the imposition of an itinerary (Callon et al., 1986). The translation here is from one set of assemblages to another; from archaeological site to documentation site; and the instruments used to implement movement or change are, in this case, inscriptions, that is, documents and arrangements of data. The actors in this actor-world are archaeological remains, researchers, institutions, software packages, software development teams, maps, photographs, and various others. The translation must make itself indispensable to succeed. If the RSDP *is* to succeed (and it is ongoing at the time of writing), it will do so through a combination of seduction and authority: by appealing to international partners through the use of status and funding to encourage participation, offering them access to a prestigious publishing platform and the possibility of future research network-related opportunities (or fear of the potential ignominy of being left out); appealing to local agents, such as the host institution and the charitable funder with the promise of broadening and sharing knowledge and thereby increasing prestige and authority; and by persuading the host institution's IT department of its obligations to support research projects over its obligations to other institutional needs.

Even if it *is* initially successful, it will still need to persuade its allies to stick around if it is to ensure its ongoing existence. Latour discusses this requirement in relation to Aramis, a proposed public transport system in France:

> …even an ordinary user can make Aramis less real by refusing to get into one of its cars; or, if she's a local official, by refusing to get excited about it; or, if he's a mechanic or a driver, by refusing to work for it. No matter how old and powerful, no matter how irreversible and indispensable, thus no matter how real a transportation system may be, it can always be made a little less real. […] These enormous hundred-year-old technological monsters are no more real than the four-year-old Aramis is unreal: they all need allies, friends, long chains of translators. (1996:86)

In the same way, many actors have an ongoing influence on the 'reality' of the RSDP, not least those touching on its technological aspects. The IT department determined which computer operating system should be used for the web server hosting the final installation of Arches, and this had a knock-on effect on the viability of the installation of various required software packages. The international partners decided how much of their data to share, and how often to attend meetings, which influenced the content and timescale of the project. The import and export functions available in Adobe Illustrator and QGIS determined the order of production of digital resources. The column types available in the database circumscribed the nature of the data which could be saved and searched.

From an ANT perspective of the overall project goals, these entities were conceived of in terms of the simplest functioning state which would produce compatibility with the desired result of the final publication. Where this was successful, these constituent parts could be treated as closed 'black boxes': where a resource failed to produce the expected outputs, the black box had to be opened and the contents worked through. Many of the team meeting discussions were about the processes or implications of translating (in the more straightforward sense) information between different media or representational modes: this was the design and construction of the black boxes which could be used to produce consistent and manageable results dependably. Much of this discourse was related to the simplification and codification of data: it was both a filtering process and a process of classification. At a fundamental level, the mapping work converted landscape geography to polygons, then to a unique database identifier, in the form of a long string of characters. Complexity was reduced and irrelevant agencies removed, until there was a dataset which was sufficiently graspable either by human cognition, or by the physical (storage and retrieval) and processing capabilities of computing machines. The degree of filtering

employed was influenced by the practical resources of the research project, such as available time, budget, human resources and computing power.
All through the research process, activities were measured against the final proposed form of the outcome: the database and software platform used to share the research. Negotiations took place against these yardsticks and documentation was designed to fit within their constraints. The overriding significance of the data structuring process was made most obvious at those points when the data schema was revised and existing records had to be updated. The details of the data entering and editing process were strongly dictated by the intricacies and peculiarities of the mapping software's interface, and team members were given highly detailed instructions on how to do this efficiently:

> …the main thing for migrating data is the monument type because in the old system it was not hierarchical. […] So I need to find a way of migrating the terms in the right hierarchy. But after that, hopefully I shall redistribute the geopackages with your to-date data and I will ask you to start going back to the records that you did already. What needs redrawing, deleting and drawing again […], what needs editing without deleting the records, and migrating […]. There is no shortcut really, if you need to transfer something from polygon to line, the quickest is just redraw it. And then we can cut and paste the description and all that. So that's not an issue. And then you delete the record from the polygon layer. So you will receive, of course, all your data in the polygon feature, but some of it might go to the lines. Some of it might just need re-editing and tidying and drawing a bit better. (Team member, 2020)

To be included, all data had to be made to fit through these obligatory points of translation. Such detailed accounts were the closest access I had to the everyday work of inputting mapping data. The majority of workshop discussions were related to the interpretation of individual archaeological features and the technicalities of representing them in maps. The documentation objects (the digital archives) became the focus of the research activities, and the negotiations which took place were often about the internal logic of those objects, what was included and excluded, and how they might fit into the digital 'cabinets' being used to store and display them. My impression from these discussions was that the overarching narratives would have to be designed to fit the affordances of the keystone software packages —to be added *on top of* the information structures they provided—in spite of the emphasis from team leaders on the importance of *leading* with research narratives. This

impression was reinforced by the decision to associate narratives with the layer-based data structures inherent to the GIS software structure (though of course a layer-based approach is characteristic not just of GIS software, but of the foundational archaeological concept of stratigraphy). In fact, by the end of the observation period, I had been privy to very little discussion of broad archaeological narratives. This aspect of the research was not prominent in the team meetings and workshops, which were devoted respectively to management issues and the technicalities of interpreting and representing individual archaeological features.

The documentation process started from the *visual traces* of archaeological features, and the tools used were a reflection of this, starting with the map-oriented interface of GIS software. The images which were the object of study were themselves designed to give a map-like perspective on the world, looking, as much as is possible, directly down onto the Earth from an orbiting satellite or from an aeroplane or drone overhead. A key requisite of the accurate use of GIS software with RS data is the careful matching of imagery with existing map data, without the introduction of a degree of stretching or distortion which is deemed inappropriate. The matching of map projection and photographic perspective, each presented as a commensurable superimposed layer in the same interface, is usually automated using specialised rectification software (e.g. AirPhoto[24]) which lines up matching points between the two media based on algorithmic calculations, introducing distortion to the photographic images which *is* deemed appropriate and acceptable.

The process of marking archaeological features in superimposition over the photograph, and therefore the map, consists of drawing vector lines on an additional graphical interface layer, analogous to drawing marks on the transparent sheet of an overhead projector. These are made by a series of mouse clicks which delineate either a single point, a line made of an arbitrary number of joined sections, called a polyline, or a polygon. Circles and ellipses are also supported but there is no support for Bézier curves. Therefore any shapes which are not strictly linear or symmetrically curved can only be approximated by linear geometries (many small straight lines joined together). Each vector feature has an associated table of editable data attributes, with keys and values, including at a bare minimum a unique id

[24] http://www.baspsoftware.org/airphoto.html

value. Each of these data attributes is stored in a database, and therefore must be one of a range of predefined types. In QGIS these are:

- Whole number (integer)
- Whole number (integer 64 bit)
- Decimal number (real)
- Text (string)
- Date

The assessment of the level of risk threatening the preservation of archaeological features in the RSDP for example, had a table entry of type 'Whole number', again with possible values ranging from 0 to 5. Polygons outlining archaeological features which had been assigned such values could then be colour-coded based on their classification, in addition to being subject to computational analyses. The highly complex landscape data was thus reduced to a series of minimum usable characteristics in the processes of translation (see Table 3).

Table 3: Partial simplified list of translations in documentation of interpretation of satellite imagery.

TRANSLATION	FROM	TO
1	3D physical landscape	2D map, using a conventional projection[25]
2	2D map	GIS software format, using conventional Coordinate Reference System (CRS)[26]
3	Remote-sensing obtained images	Map tile images (with software rectification[27] and destriping[28])
4	Landscape features	Hand-drawn linear vector paths and polygons
5	Attributes of vectors and their signifieds	Database fields of limited types
6	Database fields	Formatted HTML pages, UI or lists for user consumption (optional, could be used for computation)

Discussions about translations were common. Certain maps were 'worthy' of being imported into Illustrator for design work. Some paper maps had mistakes in their scales, and had to be 'fixed' when translated into the digital domain, to make them conform to others. Suitable relationships had to be established between Illustrator and QGIS, and between

[25] All projections involve some degree of distortion, involving as they do the representation of 3D objects in 2D space.
[26] At the time of writing, QGIS has support for approximately 7,000 standard CRSs. https://www.qgis.org/en/docs/index.html
[27] 'Orthorectified images have been processed to apply corrections for optical distortions from the sensor system, and apparent changes in the position of ground objects caused by the perspective of the sensor view angle and ground terrain' (Brown and Harder, 2016:42).
[28] Satellite image sensors 'do not have identical transfer functions. As a result, images produced in this fashion show undesirable, regular "striping". This effect can be removed if the transfer functions are accurately known' (Horn and Woodham, 1978:1).

QGIS and Arches. A single layer in QGIS had to be translated into multiple Illustrator layers, hence determining the direction of travel between applications. The outputs of QGIS had to be adapted to fit the emphasis in Arches on monuments, and its parent-child relational data structures. Arches used a thesaurus to produce conformity between architectural terms, and it was reported that this needed 'scrubbing' (cleaning up). Throughout the course of the project, the custom database structure in QGIS was actively updated as the project requirements changed, particularly as expectations around the recording of site condition and risk changed. This entailed the manual re-entry of existing records to reflect the changed database schema. Gradually, the black boxes took on more solid shape.

For the researchers working purely within the domain of RS image interpretation, some of these translations were dependent on processes which were already 'black-boxed': for example, the mapping data in GIS was preconverted from a 3D physical landscape into a 2D approximation of it (Table 3, translation 1); the level of permissible distortion to introduce into an image in order to match it to this map data was determined by an algorithm in a rectification software package (Table 3, translation 3). In workshops, these steps were taken for granted, already absorbed as conventions. The pressing work consisted of creating standard processes for the creation of vector paths, the association of data fields with those paths, and the design of the database schema to capture those attributes. These areas were where were most of the debate, discussion and design work took place, always with the shape of the final output in mind. Part of the work was to fit evidence into pre-existing categories in a thesaurus of terms provided by the Arches software:

> So if we say, digitise the [burial mound], okay? Classic, it will be under ritual, religion and funerary and then monument type two, depending on what you pick here, you have a different selection, okay, so these are all ritual, religion and funerary and you will find where it's a burial. Okay. Now depending on this, again, you have a difference. So you have different burial, assemblage, chamber, mounds and you have [burial mound]. And then under [burial mound], you have two types. If it's neither of the two types, you just leave it [burial mound]. If you have a subdivision, then you pick one of the two. Okay? Now we'll make available the whole hierarchy. […] The reason why we're doing this is for being able to select higher levels of categories. For instance, if you want to search for anything to do with ritual, religion and funerary at the moment, you have to search in three fields because you can put anything in the three fields. Whereas in this way you do one search… and the same

> goes for the other levels. This is just a more proper way of categorising and characterising the monument type given that the thesaurus is actually hierarchical and in Arches it works in this way. (Team member 2020)

The team often appeared to work from existing standards made available through Arches. However, there were some discussions about the custom design of classifications and representational standards:

> Speaker 1
> …we may have some of these big sites already as monuments. So let's say that settlement and burial mound are already registered as two different monuments with two different monument passports, we are not suggesting this, but we may consider having one single polygon, within this area—we have a fortified settlement first, and then the burial mound later. And then from the monument passport, we can distinguish the two. I'm not suggesting this, but this is one option.
>
> Speaker 2
> I'll be arguing for something different to that!
>
> Speaker 1
> We have to consider the fact that we also have other datasets. So we have to think in that respect. As I said, I'm not suggesting that but it's just one option.
>
> Speaker 3
> Lots of options!

Such discussions seemed to reach their resolutions 'offline' or in smaller meetings, by those leading up the team, after which the standard approach would be communicated to others. The negotiations at which I was present were characterised by joining opinionated archaeological interpretative approaches to technical considerations; but the freedom to innovate or express an interpretative ideal was heavily constrained by the affordances of the Arches software. The software had of course been selected in the first place for its capabilities and its overall paradigm, so to some extent, expectations of its use were built in.

The movement of information between actors was not limited solely to software formats. Translation between languages, and the representation of multiple languages was a vital

part of the project, not just in team meetings, as previously described, but in the use of existing literature and maps. As an international project, the use of multiple languages and language scripts was a core requirement, but this was not fully met by the most recent version of Arches. The upgrading of Arches to a new version resulted in a number of problems due to previous features becoming unavailable. This was a case of a black box which remained stubbornly open, one which caused prolonged delays to the project and threatened its successful completion.

5.2.7 Technology Infrastructure

The online team meetings laid bare some of the difficulties of working with technology and helped to highlight problems with accessing technological infrastructure. The fact that the main repository for working project documents was hosted by a UK university within an institutional authentication system meant that external partners had difficulty in accessing it. The host university's official video-conferencing software was simply not available for use in the countries of some international partners, and an 'unofficial' system had to be used. Some international teams had ongoing problems with internet access, due to the poor quality of local internet provision in their areas. Team meetings were frequently beset by communication difficulties, for example with sound dropping out, or connections failing. At times participants would be forced to disconnect from meetings and rejoin using their mobile phones rather than their laptops. Some of these issues can be understood as teething troubles for researchers who were new to the suddenly enforced practice of working in isolation and having meetings online. Others were symptoms of fundamental inequalities with regards to access to technology. For the most part, meetings proceeded successfully, but these regular issues were an important reminder that the ideal of digital communication as an enabler of equal access to knowledge was and is far from being wholly fulfilled.

Access to technological infrastructure was also problematic with regards to the provision of the final software platform for publishing the project outputs. Team members were able to use desktop-based software like QGIS or Adobe Illustrator (purchased licence permitting) independently, to make progress with their documentation efforts. Arches, the final publishing platform, was, on the other hand, a web-based application rather than a desktop-

based one, and required a web server with suitable storage space and ideally a long-term maintenance contract. This was to be provided by the university employing the core team and part-funding the project. Throughout the duration of the project, negotiations between the research team and the IT department of the university were ongoing, and at the point of my final observation, still not fully resolved. This was a familiar echo of my own experiences in working with software projects at HE institutions. The interests of researchers (cutting-edge software, often customised; a high degree of administrative control and access to web instances) rarely coincide with the interests of IT departments (stability, continuity, security, limited access, low maintenance). In the case of this project, support requests to the institution were very slow to be answered, or were ignored, leading to a great deal of frustration and many delays. This was perhaps exacerbated by the fact that the Arches software has many 'moving parts', or software dependencies[29] and that setting it up entails an 'Extract, Transform and Load' (ETL) process of moving data from an external source into its database. This can involve trial and error, and ideally should be tested on a 'staging' server, a parallel installation of the web software with the exact same configuration as the 'production' server. These complex requirements, and what was probably a complicated hierarchy of professional accountability in the relevant IT department, made it difficult for the RSDP team to enrol the 'interessement' of the IT specialists managing the web server resources. There was also concern in the team about the long-term viability of the software platform. For this reason, they took the approach, wherever possible, of storing data in an institutional repository with long term support, and using the Arches instance primarily as a 'front-end' for that data.

5.2.8 Workshops

The workshops were focused on the identification and representation of archaeological features via the interpretation of satellite or other aerial imagery. At times classification was discussed (see §5.2.6), but more often they acted as a medium for the team leaders to impart details of adopted standards. They provided useful insights into the factors which shaped the research processes being used. Firstly, they were run on the basis that there were best practices which should be observed in the mapping work, and that these could be

[29] See https://arches.readthedocs.io/en/stable/requirements-and-dependencies/

passed on from expert members to inexpert ones. Such unambiguous statements of standards and intent were useful to me as an outside observer, seeking to understand the basis of interpretation. Secondly, the assessment of team members' work-in-progress made apparent the number of paths available to the archaeologist, and the negotiations between commitments—ontological, technological or practical—which were required, as I will discuss presently. The compromises required to make progress were brought into focus at this point before receding from view later in the process, at times to be lost completely in the final documentation artefact.

Interpretation

One of the narratives of the project was to illustrate the value of using remote sensing (RS) data such as satellite imagery to identify archaeological features. The policy for assessing the condition of features, and risks to their future preservation, was that interpretation should only be made on the basis of the evidence available in satellite imagery:

> …for now, the assessment is based on satellite imagery. What we're doing is to take the historic satellite imagery, and see how the threat is evolving around the site. So in this case, for instance, you have the village next to it. It's there, but it's developing towards the south, and it's not affecting the monument. So you have a low risk of urban expansion. But in terms of the cemetery, probably the risk will be medium, because the site is quite filled up with burials. And so therefore, there might be a bit more but not too many more. It's all based on satellite image assessment. For now. (Team member, 2021)

In other words, the documentation was to be a translation of the satellite imagery, rather than the landscape it depicted. This was a practical as well as a philosophical decision, as it set limits on the evidence-gathering aspect of the project, and simplified the number of variables to be taken into account when making interpretations. These practicalities were imperative for a project which had the highly ambitious goal of documenting a vast landscape stretching across many countries.

However, this was a highly artificial limitation with internal contradictions which, as I discuss below, resulted in negotiations about interpretation and led to some uncertainty in the practical aspects of the representation phase. First of all, one of the key tenets of aerial photography (AP) interpretation, as continually highlighted by the experts in the team, and

by a visiting specialist who gave a workshop in AP interpretation, was that *all available sources should be used*. In practice this usually meant making use of a range of image sets, for example historical surveys of the area under study, as in the case of photographic surveys from aeroplanes, or satellite imagery where multiple image sets from years of surveys were available from the major providers. The value of making comparisons between images of the same landscape was clearly demonstrated: different lighting conditions, seasonal variations in crop growth and the presence of snow all had an impact on the visibility and interpretability of archaeological features. Comparing multiple sources was a central principle and of particular importance when making assessments of the extent to which archaeological features were at risk, as degeneration could be detected over time in some cases, or nearby urban spread could be seen to be increasing, as in the quotation cited above. But should *other* types of sources be used? In the case of condition and risk assessment in particular, the usefulness of additional data about conditions on the ground became obvious. This then, was a source of ambiguity. Which order of reality was the starting point, and where were the boundaries drawn?

In addition, a level of artifice arose from the perception that RS interpretation is a learnable skill with its own internal logic, which can be taken and applied to any landscape. In some respects this is true. The data is often derived from the same sources, for example satellite imagery licensed by Google or Microsoft (though in this project, other historical satellite images were also drawn upon). The relationship of sunlight to landscape is consistent, once the global hemispherical position of the camera has been taken into account, and it is therefore possible to learn how to analyse highlights and shadows to ascertain whether a landscape feature is a mound or a depression, a ridge or a ditch. Through practice and experience it becomes possible to recognise common landscape features such as ploughed fields, quarries with nearby waste dumps and so on. Importantly though, a vital part of this practice is informed by findings from the associated process of ‘ground truthing’. Sites of potential interest in a satellite photograph are marked for investigation, and then visited on the ground for closer examination. An expert body of knowledge is therefore built up iteratively by this two-stage process of high-level surveying and ground-level checking, until there is a canon of broadly defined archaeological features which can be fairly confidently identified by recognisable patterns in imagery alone: ‘burial mound’, ‘soil mark’, ‘crop mark’,

‘field system’, ‘settlement’, ‘fortification’, etc. Aerial and satellite image interpretation, while continuously evolving, is thus intimately tied to the lessons of historical fieldwork in specific landscapes.

A contradiction arose in the RSDP when the prescription was made that interpretation should be based on RS imagery alone. Treating it as a transferable skill with its own internal logic, based on an established ontology, produced difficulties and compromises when it was deployed in an unfamiliar setting. RS image interpretation was more or less robust when applied to the analysis of commonly occurring landscape features such as rivers, which were investigated for their potential influence on or relationship to archaeological features; but its frailties became apparent when considering features which were in any way an expression of human culture. (The challenging nature of making such interpretations is unsurprising: Sørensen has argued that human culture represents a move from the material to the immaterial, and therefore, in the archaeological record, ‘the more human, the less intelligible’ (2016:744).) Examples of this arose in the workshops when the team's specialist in RS imagery interpretation shared examples of interpretation. His experience had been built up over many years of studying UK and other landscapes. While he expanded his canon of archaeological features to account for cultural practices local to the site of observation, at times his identifications proved to be problematic: they were the best interpretative options based on his experience; but a team member with intimate experience of the region being studied was able to make more plausible identifications of the features being discussed, based on extensive local research, including fieldwork, as in this exchange:

> Speaker 1 (RS specialist)
> I'm way out of my comfort zone as you know. But if I was looking at this in other parts of the world, certainly in the UK, I would immediately be looking at this and thinking these are periglacial features, these would be frost cracks, ice wedges, that sort of thing. I strongly suspect that is also the case in this environment. There are a couple of decent mounds in the middle of this—I even hesitated to identify these as likely burial mounds—I suspect they are. But if I'm identifying these as periglacial features then of course there are periglacial features that can cause mounds and bigger ditches. If anyone's familiar with the idea of a pingo—it's a mound created by frost heave, and it did cross my mind for a moment that that might be what that is, but I don't think so. So if anyone knows more about that sort of phenomenon in this area please let me know. Or comment now.

Speaker 2
You mean those-tower like features, the burial-mound-like features?

Speaker 1
Yeah.

Speaker 2
No, absolutely they can be one of those watchtower-like structures, at regular intervals with the fire on top and everything else, but they do exist in the flattest landscape, and they basically are signal towers and watch towers.

Speaker 1
So that's another possibility. You know, it comes back to this idea that there are many, many things that can look like round mounds. You know, that's what it amounts to.

Speaker 2
We have very good ethnographic evidence, the oral histories from 19th century, for example, how they were used because at that time, there is no controlling power and lots of tribes fighting against each other, and they still use them. And we also have a little bit of information from earlier historical sources as well. I'll show you some in context we marked recently, for example, some of them are still very visible when you visit them, you know that they are watch tower, control tower type of things.

Speaker 1
Oh, I'd love to see some of those that that would be fantastic. Okay, moving on. (Team members, 2020)

In this exchange, Speaker 1 acknowledged Speaker 2's superior local expertise by moving the discussion on rather than continuing to argue the point. In my interpretation, he accepted his colleague's reasoned interpretation as the most plausible one, but avoided any direct challenge to his authority by leaving the matter unresolved. This (diplomatic and good-natured) clashing of colleagues on matters of interpretation and authority took place multiple times, illustrating the artificiality of limiting interpretation to the RS photography sources alone. This served to highlight the uncertainties which can arise when multiple sources of information can be drawn upon for the creation of models, models which for pragmatic reasons must be ringfenced for consistency, manageability and completability.

The team members were aware of these difficulties and discussed them. The normal practice in the use of RS interpretation was to posit it as the first step of a hypothetical multi-step process: 'if we see something interesting, we can mark it for ground-truthing.' However, for such a vast geographical area, in-depth analysis of every potential archaeological feature using a range of data sources was simply not practical. It was assumed, reasonably, that making identifications of the geographical location of features which had not previously been documented would in itself be extremely useful for local policy-makers, and in particular for the conservation of heritage, even if the interpretative detail was not comprehensive. The negotiations then, were around whether to limit identifications and assessments to the broadly defined canonical features which were defined by established RS interpretation practice and inferable only from the photographic evidence—or to make use of more detailed models, informed by in-depth knowledge of local history and culture, and/or 'external' datasets such as those derived from excavations:

> It's really difficult, I have to say, from satellite images, and that's the limitation. So we work with that limitation at certain point. We will go and see the sites and get the point of view from the terrestrial assessment, and then we can compare. For now we are dealing only with the satellite images. (Team member, 2020)

Those mapping archaeological features were expected to include an assessment of condition and risk, on a numerical scale, and include textual justification for their reasoning, even when only making use of RS imagery as an evidence base. On one hand, the structured approach of RS image interpretation was evidence-based, systematic, and also achievable given the available time and human resources, making it a good fit for credible academic reporting. On the other hand, as we have seen, it was in practice proven inadequate at times by the availability of 'ground-level' local expertise which contradicted its expectations. The approach seemed to contribute to more basic misunderstandings: one student reported on some highly detailed interpretative work they had carried out on imagery of a particular region, only to be told by the leader of the workshop that the region had already been subject to extensive study and documentation, and that the student had misidentified some features:

> Okay, so this area is really, really well documented. […]

> I think in this case, you are spending time trying to analyse things that have been already analysed. But as the project goes, we should find these ways of linking data, not only data, knowledge together somehow, because otherwise, we might end up doing this over and over again. (Team member, 2020)

In my interpretation, there was consciousness at some level that the ambiguous project requirements about the use of data sources put the ontological model of the studied resource into a state of flux. This in turn undermined the project goals and it was therefore minimised in discussion. There was, in other words, at one stage, a lack of a clear epistemological programme, and the negotiations about what could be 'known' were indirect and worked around, rather than stated in plain terms. The need for compromise was resolved by the most practical and obvious means: archaeological features were documented using high-level RS interpretation alone unless there was authoritative and *readily available* ground-level evidence which could be brought in to help understand those features. Such evidence could not be ignored in those cases where it was easy to access and could prevent rudimentary interpretative errors from being made: thus team members had to 'learn' to start from a consideration of available documentation, in spite of the previous emphasis on an RS imagery-first approach. The contradiction that this threw up about the reliability of interpretation *without* the use of external datasets was dealt with by the creation of a research narrative which conceived of the project in terms of long-term goals: those features interpreted with the aid of ground-level evidence were given the status of 'case-studies' and were considered the ideal outcome for all documented sites and features. The other features documented only on the basis of RS analysis took on the status of provisional interpretations, with the potential to be documented using more exhaustive sources at some future date.

This deferral of a final conclusion, the appeal to a teleological narrative of 'filling in the gaps' at an unspecified later point, was a common pattern of negotiation in workshop discussions. Where definitions of knowledge or procedure proved troublesome, it would be stated that 'I don't know if we should be talking about this just now, but…', or 'we should have a discussion about this'. In one sense these were cues to prompt discussion and seek consensus, but in practice they were often used to allow difficulties to be acknowledged but left unconfronted. In some cases an authoritative team member would clarify policy,

providing resolution; in others, the fallback position was the concept of the work-in-progress, with detail to be fully coloured in at some point in the future. This had the potential to highlight the sometimes artificial nature of 'disciplining the data':

> If you try to record things individually, obviously, the main fortified settlement with its fortified structure on the corner there is an entity. We can pretty much happily say that is interpretive, but it does look like that's an entity and they should be recorded together. But does that want to be recorded as a perhaps a later burial mound? And then, you know, we have the issue of these, are they recent, do we record them or not? And so on, lots of different issues. But these are all things for discussion, in probably our next session… (Team member, 2020)

It was not clear to me whether the choices in this specific case were later worked through to a satisfactory conclusion. Certainly team members were highly conscientious in assessing evidence to arrive at their interpretations. My impression of this kind of exchange, and the strategy of deferral, however, was that one of its functions was to convey, perhaps indirectly, that in the case of ambiguous evidence, various interpretations might be considered valid. There was a strong sense in these formative discussions of an ongoing need to develop a consensual position on what could actually be 'knowable', the group's working assumptions, and what could or should be discussed; the formulation, in other words, of a 'thought-style', a consensual mode of thought, particular to a specialised community. As Fleck puts it:

> The organic exclusiveness of every thought commune goes hand in hand with a stylized limitation upon the problems admitted. It is always necessary to ignore or reject many problems as trifling or meaningless. Modern science also distinguishes "real problems" from useless "bogus problems." This creates specialized valuation and characteristic intolerance, which are features shared by all exclusive communities. (1981:104)

The visiting expert RS advisor was a representative of another distinctive professional community, one specialising in interpretation of RS imagery. He was a man of long-term experience who was semi-retired. This gave him a strong aura of authority in workshops, and as seen from the exchange above, negotiations in the assessment of evidence were strongly informed by contributors' authority status. The negotiated use of multiple sources

of information can therefore also be seen in this case as an example of an encounter between thought communities: the expert RS interpretation community, characterised by the RS advisor and other visiting RS interpretation experts who delivered workshops; and the fieldworkers and other archaeologists who specialised by region and historical period, rather than by technique. Research traditions have their own well-established procedures for managing data—their own 'reasoning styles'; for example the processes described here for the translation of RS imagery (Table 3); or the documentation of dig stratigraphy; or the coding and theming of ethnographical observations. When traditions are *combined*, concordances and disagreements between them need to be worked through, a new set of translations and filters need to be made, and ground rules agreed upon. These negotiations could be seen playing out in the RSDP workshops. Arguably, conducting negotiations across these thought communities is not just about assessing the relative merits of evidential material, but to also to an extent, about deciding what kind of archaeologist you *are*. Which specialist tradition you identify with? Does your contribution shore up and further work in that field? The workshop groups worked to build up understandings of the past, but they may have had different priorities. A 'technique specialist' might have had the ulterior motive to prove or improve the efficacy of that technique (and their mastery of it). A 'period specialist' might have been motivated to prove or improve a prior hypothesis about the development of a local culture at a given point in history.

In the discussion described above, the RS expert developed an impressive hypothesis based on specialised knowledge of periglacial landforms, but this was easily refuted by a colleague with knowledge of local historical architecture. This inspired a somewhat defensive response, in spite of the acknowledgement from all parties that RS interpretation has interpretative limits. But the purpose of the exchange was to work through which research method was most plausible in any given case, or to put it another way, which mode of interpretation was *least worst*, in the face of limited evidence. The nature of this kind of practice, a 'negative epistemology' (see §7.3) emphasising avoidance of error, is not easily captured in conventional documentation forms.

Language of Interpretation

When listing archaeological features, one expert contributor used language which implied an emotional connection to archaeology. Features were 'lovely', 'nice', 'ugly', 'exciting', or

simply ‘good’. These evaluations referred variously to the state of preservation of a feature, the degree to which a feature matched the platonic ideal of its kind, or the rarity of the find. Significance might relate to the scale of a previously undocumented find, and significant finds were ‘exciting’. Significance could also be related to novelty or the potential for instrumentalising observations, for example as part of a conservation effort. This kind of language tended to be used mainly by senior team members; its informality was an expression of a confident familiarity with a wide range of similar material—the mark of an expert afficionado. In one respect this was a means of enculturating junior team members, who generally limited themselves to more technical descriptors. The emotive language hints at the fact that there are hierarchies of interest and attachment to archaeological features, which while not formally documented, are likely to influence the allocation of resources to various findings, even in projects which champion an ideal of objective neutrality. Some of these may occur at an individual emotional level: others may relate to documented cultural and scholarly values. These could be self-reinforcing, in that the level of engagement could be linked to the extent to which a find confirms an existing hypothesis.[30]

The language used by experts in workshops at times highlighted the opportunities for making inferences from limited evidence through the use of carefully applied logic. Archaeological features were ‘puzzles’, accompanied by ‘clues’ which allowed researchers to make ‘best guesses’. Expertise was therefore presented as heightened awareness in combination with the skilful use of inferential logic and accumulated experience—a kind of archaeological detective work. This underscored the significance of the RS interpretation ‘thought-style’ and the credibility it brought to making knowledge claims. What was noticeable was that in spite of this vocabulary, the language of interpretation was always couched in extremely conservative terms, emphasising that due to the limited nature of the available evidence, interpretative claims or suggestions were highly uncertain:

[30] This was illustrated in my MRes research, when I interviewed a team of archaeologists, also analysing RS data. It was noted that the most difficult to identify archaeological traces were some of the most intriguing, but because they did not fit into an established taxonomy, and therefore could not be tallied in the annual list of research achievements, they were unlikely to be the subject of further research. The danger was of investing research resources, often using public funds, into projects which might result in ambiguous outcomes. This would not be considered good value for money.

> So everything is a little bit ‘if’. (Team member, 2020)

> …that is then something you can mark with a line, of course, with very low certainty. (Team member, 2021)

> No, I'm not sure, my best guess would be something maybe like pistachio, something along those lines… (Team member, 2020)

> What stands out most strongly in this is actually the suggestion of a surrounding ditch. Now I'm not sure it is necessarily a ditch—the way that these mud brick walls dissolve over time probably means that you get build-ups of more organic material at the base of the collapse. I'm surmising this for my own benefit, I feel like this is how I am rationalising what I'm seeing. It's not necessarily true. (Team member, 2020)

> Now there's no direct reason to associate this, shall we call it a farmstead, a farm, a small settlement, whatever we want to call it. There's no direct evidence that that's associated with this field system. It may be wise to keep them separated when you record them. No one I think would criticise if you made a suggestion that they were contemporary, okay, but there is no hard evidence for that here. (Team member, 2020)

This highly qualified language was mostly lost in the translation of features into data points. There was scope to add discussion or assessments of uncertainty in relation to interpretation, via a free-text entry field in the table of attributes, but discussions in the workshops suggested that this was not a useful field for search, and certainly not for performing computations:

> you've got a comment field, or free text field already in your database. And to my mind, that is all you need, to discuss intelligently the relationships between the feature you're recording, describing, and features that may or may not be associated with it. That field is not that useful in terms of a database, because it's free text, it's not actually going to be a usefully searchable field. So the key to the database, and your GIS mapping has to be the descriptive stuff, the, you know, ‘it's an earthwork’. It's, you know, the searchable stuff. (Team member, 2020)

Features therefore became fixed in the database by their categories or titles, irrespective of the subtleties of the interpretative text accompanying them. In their account of classification systems, Bowker and Star refer to 'Other' or 'residual' categories (Bowker and Star, 1999:150), suggesting that such categories 'tend to fix the maximum level of granularity that is possible': they provide an advantage where 'forcing a more precise designation could give a false impression of positive data' (ibid), though they ultimately undermine the statistical usefulness of the system. In this case, the qualifying information was residual but lacked the advantage of being a distinct category. The additional text was rather an addendum to a category selected as 'best-guess'. This meant that in some cases the communicated sense of epistemological validity was significantly transformed in the translation between the phases of interpretation and recording, from highly nuanced to much more straightforward identification. The vocabulary of knowledge, which was floating, unfixed, and full of qualifiers in speech, became—via negotiations and consensual agreement on what constituted 'real' problems and 'bogus' problems—fixed in the database. Certain aspects of the object of study were highlighted for the purposes of identification, at the cost of other interactional properties which may have been rich in opportunity for creating understanding. The butterfly was pinned down in the display case. This is not to say that misleading or unfounded claims were made, but that the nuanced status of 'best guess' had no good representative in the database design. There was no space to capture the unsettled movement of the debates which informed the interpretation, and the range of possibilities they hinted at. These debates were accepted as a necessary part of the process, but the final *shape* of the research outcome determined which parts of the process were retained and carefully documented, and which parts were sidelined after making their contribution.

As the presence of a 'best guess' entry for archaeological remains was perceived as more useful than no entry at all, this was a de facto process of winnowing out ambiguity and uncertainty, as described in Gero's account of the 'cleaning of data' in archaeological research (see 'Classification', §2.3.3). For the sake of the integrity of the record, without which there is no successful outcome, the unstable has to be made stable. Bowker and Star, drawing on the work of Goodwin, observe the same process in the archaeological practice of recording the colour of patches of earth against standardised colour charts, known as

Munsell colour charts:

> The archaeologists constantly compare the pieces of earth against the chart, negotiate with each other, and transform their everyday terms for the earth into the formal numbered categories on the chart. The uncertainties they face along the way are removed once the numbers are selected and reported. (1999:65)

A feature of the pragmatic nature of projects involving classification, as Bowker and Star point out in their analysis of the ICD (International Classification of Diseases) (1999) is that while they may be conceived of in terms of Aristotelian ideals, in practice they often adopt a 'prototypical' model of categorisation:

> The classical beauty of the Aristotelian classification gives way to a fuzzier classification system that shares in practice key features with common sense prototype classifications—heterogeneous objects linked by metaphor or analogy. (Ibid)

The ICD and its instruments developed and adapted to become a 'workable epidemiological tool', rather than a 'net to capture all knowledge' (ibid, p72). This 'loosened up their implicit causality' and made more complex narratives possible (ibid, p73). At the developmental stage of the RSDP on the other hand, during my period of observation, the categories remained firmly Aristotelian, arranged in database-friendly hierarchies. The use of 'other' categories was firmly rejected, and for one senior team member, instances which did not fit into available categories were deemed irrelevant:

> Well, again, if you can't fit it into the selection you've got there, it probably isn't worth having. (Team member, 2020)

As the project progressed, some of the categorisations were revised, for example to reflect updates to the scheme for recording risk levels in relation to monument conservation. Some categories were philosophically abstract, and open to challenge; for example the numerical scale used for assigning a measure of the condition of archaeological features. The number 0 signified 'completely destroyed', while 5 was 'excellent', meaning 'completely unchanged'.

It was pointed out that neither of these two extreme states could actually exist in practice, and that therefore only the numbers 1–4 would be of practical use:

> If we want to be very accurate, the two extremes are not really the case. So you will never have an excellent condition where the site is unchanged, since it's used. […] So conceptually, there is no 'excellent' or 'destroyed', because the two extremes are not there. […] But in this exercise we work with the limitation we have, of the satellite image. So when we say destroyed, or we use the two extremes, it is in reference to the satellite imagery, because we can't possibly assess whether there is subsurface remains in these examples. […] If we're happy with the assessments within the terms of condition that we set, then it's okay. But I totally agree that there is no 'destroyed', and also not 'excellent' in the sense of untouched. But it's within the framework of our remote sensing assessment, we can decide to assign those two levels in this in this way. (Team member, 2020)

For workshop participants, then, interpretation was sometimes messy and highly contingent, but these aspects were not easy to capture. There was at times, therefore, a mismatch between the language used to set out the ideals of the project and that used to mobilise data in order to make it productive. This was at times a source of uncertainty for team members, in particular those 'learning on the job'. There was a field to indicate 'certainty' in identification, which was required, and optional provision for adding an interpretative narrative when creating records. These fields were the best available option for adding nuance to a system which was mainly geared towards the creation of 'computable' data about archaeological sites.

Vector paths

Team members were not always sure when they should be using polylines as opposed to polygons. This partly stemmed from uncertainty about whether features were single or part of a series or larger group:

> Two stones don't make a structure. Three in a row is different… (Team member, 2020)

In general polygons were used to set out the borders of a feature, where they clearly existed, or to mark the boundary line of a group of features, so that they could be entered

collectively as a grouped entity. Polylines might be used to mark the sections of the incomplete traces of a wall, for example. A polygon would be unsuitable in this case because as one senior team member stated as a first principle: ‘we can’t just put in things which aren’t there’, even where it might seem reasonable to infer, for example, that two adjacent wall sections were formerly joined in a space which is now a void. A separate GIS layer was introduced for ‘conceptual groupings’, where collections of things were identified as culturally determined systems or concepts: as for example where a series of collocated burial sites was identified as a cemetery; or fields, irrigation systems and buildings were identified as a farm:

> whereas with the feature polygon line you're actually mapping a physical object, with a group polygon, you are mapping a conceptual object, if you allow me this term—so an idea that a number of physical objects features are conceived together. Okay? […] So you're not actually mapping anything physical, but the concepts. That's the idea of this new level of interpretation, is to include more features into a consistent sort of system. (Team member, 2020)

The conventions for vector drawing were a good example of a translation process which could not easily be black-boxed, because the distinction between individual features and grouped features was one produced solely through human perception, subjectivity and cultural experience or study. There was therefore no single ‘objective’ or correct perspective. The ‘conceptual groupings’ data layer introduced some nuance into the record, but also brought the limitations of the recording system into focus: it could not easily capture the sophisticated nature of the relationships between things. A series of collocated burial sites may be identified as a cemetery, or connections may theoretically be made between an historic irrigation system and a system of fields, and this may be captured in semantic data relationships, but these relations are binary, switched either on or off. They do not have a conditional or quiescent state. It is not possible to say ‘if A then B; but if C then D.’ Instead, the convention is ‘best guess plus qualifying footnote’. In some cases, the documentation of conceptual links was clearly highly subjective, and multiple interpretations were considered valid:

> Again, at your discretion, feel free to use the feature line, the polyline, to record where it seems more appropriate. And in this instance here—it's a very fragmentary site. […] …there is a very clear rectangular arrangement of buildings and courtyard, just here with a few fragments of field boundary, in close proximity to it. So in that instance, I felt that was worth using the polyline approach. Whether you then place a group polygon around that to associate these with the settlement, again, is a case-by-case basis. Here, I think it's probably justified, I haven't done it. But you could equally decide that that is not related to these, perhaps, and keep them separate. And in which case, they would stand perfectly well recorded. (Team member, 2020)

There was no obvious means of capturing this aspect of subjectivity and the scope for different valid interpretations, aside from the use of an interpretative footnote. This particular translational filter also demonstrated a tendency to obscure chronological variation, reuse, development or reconstruction. In a relevant case, Chapman and Wylie discuss the use of lead isotope analysis (LIA) to determine the provenance of metal artefacts (2016:164) (see §7.5.3). One of the objections to this method, which was ultimately effectively abandoned as an archaeological technique, was that it did not take into account the frequent reuse of metal artefacts through melting them down, or their use in the creation of alloys. This case illustrates the danger in thinking of archaeological remains in terms of an idealised snapshot: the just-finished, fully intact building or the newly-minted coin. This may be a symptom of the mode of thought which separates interpretation from 'fact' (Huggett, 2015b:11), thereby fixing the fact in place and making it simpler to instrumentalize evidence for the production of meaning. As Sørensen points out (2016), human experience is that of something in perpetual formation, rather than fully formed; things are constantly 'in the making' rather than ready-made or at a point of stasis.

Time constraints

Polylines were quicker to add than polygons, so this also had a bearing on the choices made. In some cases they were deemed as the best option because it might 'make your life easier'. Time constraints were another case where compromises were recognised as an inevitable and normal part of the process, and were discussed informally, but were generally left undocumented:

> Although I understand the benefits, finding the date of the satellite image will not be straightforward. […] I think we may end up with extra complexity that will take lots of time for a smallish gain as well. So I wonder if there is a kind of a middle ground somewhere. […] How much time are we prepared to spend? In order to do that, to figure out the date? (Team member, 2020)

It is clear that a combination of factors other than the 'official' listed goals of the project affected the research process: evaluations of significance, time constraints, emotional responses to features, subjective understandings of conceptual groupings, and importantly, the perceived integrity of the final software platform and its presentation of the data.

5.3 Discussion

In trying to capture some of the complexities involved in the RSDP documentation work, I have tended to focus on those areas which required negotiation due to ambiguity, contradiction or uncertainty. This does not give a full picture of the team's work, which was highly professional, skilled, mutually supportive, and remarkably harmonious and good natured, given the daunting scale of the task at hand and the diversity of contributors. The negotiations I have highlighted, or ones similar to them, are, I would suggest, an absolutely normal and essential part of any similar project.

The project's final outcome, a gazetteer of archaeological sites and features, was potentially a resource of great utility. It represented not just a collection of carefully curated data, but a series of opportunities for those who had contributed to it or might do so in the future, including normally under-represented international partners. Yet at the same time, it seemed to me that it was not always a good fit for the archaeological ambitions of team members; these were better expressed in academic papers or in some of the case studies developed for the project's web site, where evidence could be weighed up more carefully and ambiguity and contingencies could be explored in greater depth.

The limitations of the gazetteer format were extremely useful *for me*, however, in bringing the processes of collection and translation of data into sharp focus. It is inevitable that a great deal is *lost* when documenting a phenomenon; choices about inclusion have to be made, and certain qualities can only be roughly approximated or alluded to. But the *significance* of the phenomenon is pinned down in the documentation, and this is a process

of active *production* rather than just selective inclusion, just as Latour and Woolgar demonstrate that the practices of science *produce* its realities as well as *describing* them (2013). In the same way that they saw a laboratory as an 'unusual factory' (ibid, p47) for the production of academic papers, the RSDP can be likened to a factory production line for producing digital outputs. The diverse inputs fed into it had to be translated and constrained in various ways in order to become 'processable', with the output consisting of the final documentation artefact, a web-based instance of the cultural heritage software platform called Arches.

Viewed from this perspective, some of the findings from the observation become more explicable. An important one was that the negotiated aspects of knowledge construction were poorly captured in the final documentation. The discussions, debates, workshops and learning processes which gave the project and process meaning were not represented substantially in the final database-oriented software platform, and the uncertainties and subjectivities which often heavily informed these phases of the work were usually replaced by 'best guess' entries in the digital record. This is ironic, given that the processes of translating evidence into database records made up the bulk of the work of the project; but it is a reflection of the fact that the evidence had to some extent be coerced to fit into the final representational form. This goes against the standard narrative of research, which is that we follow the evidence wherever it takes us. It would seem that to reconcile this disparity, and for the final documentation to appear credible, it must give the appearance that the data in it has been revealed in an unforced, 'natural' state.

In the case of the RSDP, the inscriptions used, the media 'through which mobility and immutability are increased' (Latour, 2017:13) could also be considered examples of what Star (1989) calls 'boundary objects' (see also §8.7). These are 'objects that are plastic enough to be adaptable across multiple viewpoints, yet maintain continuity of identity', and in so doing can meet community goals (ibid, p37). The forms in the GIS software and the repository-like nature of Arches resulted in two levels of boundary object in the RSDP: both provided the advantage—and disadvantage—of eliminating local uncertainties. They were sufficiently plastic to allow for contributions from different specialists and international partners, all with their own priorities of interest; and they allowed the project to *make progress* in stabilising knowledge. From my perspective, however, these were not a good fit for the representation of *interpretative* knowledge. The GIS paradigm is based on the

affordance of the instrumentalisation of data, the ability to perform computations on data points; and this depends on the data being stable and safely 'pinned down' in the first place. Observation of the RSDP showed that some properties of archaeological features could be captured with a reasonable degree of certainty, but that others eluded clear definition. Representations of them in a GIS-inspired format partially obscured that ambiguity and largely excluded the sophisticated interpretative processes which had taken place in workshops.

I suggest that as a result of archaeologists' extremely limited access to primary evidence, there is a tendency for them to look for certainties and consistencies elsewhere when they can, for example in the uses of categorisations based on typologies, or in the technical details of data collection and management, and to use these as the platform for building an edifice of knowledge. This allows research to proceed in a conventionally 'scientific' or accountable way. Law describes 'the most common Euro-American metaphysics' in social and natural science as 'the sense that the real is relatively stable, determinate, and therefore knowable and predictable' (2004:144), but notes that:

> attending to the practice of its methods reveals, first, that these assumptions are systematically breached, and, second, that the fact that this is happening is repressed or displaced into Otherness. [...]
>
> The suggestion is that the realities enacted in Euro-American method assemblages are complex, but also that most aspects of that complexity are denied. (Ibid, p145)

I have described some of the contradictions between the RSDP's ideals and practices in terms of the *coercion* of data to fit into a software platform with a particular structural design and affordances. Based on this and my descriptions of the various black-boxed aspects of the documentation process, the specific agency of the relevant software packages should not be underestimated; but following Law, it would be reasonable to assume that they were selected as co-actors in the project exactly *because* of their fit with a certain type of metaphysical model; one which is stable, determinate and knowable.

6. Study 3—Analysis of Journal Articles

6.1 Introduction

In this chapter I look to gain further insight into the progression from archaeological research and the handling of evidence through to knowledge claims, by viewing the process from a perspective that contrasts with my previous studies: that of the completed, 'polished' research output. I seek to explore the understanding of facticity in archaeological journal articles, the degree of certainty which authors feel empowered to express, the roles of technologies in collecting, processing and representing research, and how these find expression in the academic article form.

I chose to analyse journal articles which had similar themes to those explored by the project I had been observing, with the aim of comparing like with like, to the extent that this was possible. The main intended outcome of the RSDP was an online resource for sharing data about the archaeological features of a region, but its team members also produced formal academic outputs in the shape of journal articles. However I have not included any articles authored by RSDP team members in this analysis.

Based on my main areas of interest, I developed a set of 'template' codes against which to consider and compare the content of the papers (see §4.2.5). These were:

1. Goals
2. Claims
3. Witnesses
4. Certainty

The codes 'goals', 'claims' and 'certainty' were inspired by the focus I brought to the articles as a researcher: 'witnesses' was inspired by my readings of the articles themselves and their commonalities.

In order to compare like with like, I selected articles for analysis using search keywords which were relevant to the observed project:

- 'Archaeology'
- 'Cultural Heritage'
- 'Remote Sensing'
- 'Satellite Imagery'
- 'GIS'

These were entered collectively as a single search into the UCL university library search engine (chosen for its wide access to archaeology-related journals), and four articles were selected from the first thirty results in the list, which was sorted by 'relevance'. These were chosen on the basis of commonalities with the technologies and methods used by the RSDP, and diversity in their authorship and geographical areas of study. Case studies were favoured over more broadly theoretical papers, with the objective of seeing the working application of theoretical approaches to specific sites, and the handling of conclusions.

The selected articles were:

1. Agapiou, A., Lysandrou, V., Alexakis, D.D., Themistocleous, K., Cuca, B., Argyriou, A., Sarris, A. and Hadjimitsis, D.G. (2015). Cultural heritage management and monitoring using remote sensing data and GIS: The case study of Paphos area, Cyprus. *Computers, Environment and Urban Systems* 54: 230–239. DOI: https://doi.org/10.1016/j.compenvurbsys.2015.09.003

2. Elfadaly, A., Shams eldein, A. and Lasaponara, R. (2020). Cultural Heritage Management Using Remote Sensing Data and GIS Techniques around the Archaeological Area of Ancient Jeddah in Jeddah City, Saudi Arabia. *Sustainability* 12(1): 240. DOI: https://doi.org/10.3390/su12010240

3. Menéndez Blanco, A., García Sánchez, J., Costa-García, J.M., Fonte, J., González-Álvarez, D. and Vicente García, V. (2020). Following the Roman Army between the Southern Foothills of the Cantabrian Mountains and the Northern Plains of Castile and León (North of Spain): Archaeological Applications of Remote Sensing and Geospatial Tools. *Geosciences* 10(12): 485.
   DOI: https://doi.org/10.3390/geosciences10120485

4. Luo, L., Wang, X., Liu, J., Guo, H., Lasaponara, R., Ji, W. and Liu, C. (2017). Uncovering the ancient canal-based tuntian agricultural landscape at China's northwestern frontiers*. Journal of Cultural Heritage* 23: 79–88.
   DOI: https://doi-org.libproxy.ucl.ac.uk/10.1016/j.culher.2016.04.013

In addition, two other articles were selected which had been recommended to the RSDP team by one of their senior members, on the basis of their relevance:

5. Jia, P., Caspari, G., Betts, A., Mohamadi, B., Balz, T., Cong, D., Shen, H. and Meng, Q. (2020). Seasonal movements of Bronze Age transhumant pastoralists in western Xinjiang. *PLOS ONE* 15(11)
   DOI: http://dx.doi.org.libproxy.ucl.ac.uk/10.1371/journal.pone.0240739

6. Rayne, L., Gatto, M.C., Abdulaati, L., Al-Haddad, M., Sterry, M., Sheldrick, N. and Mattingly, D. (2020). Detecting Change at Archaeological Sites in North Africa Using Open-Source Satellite Imagery. *Remote Sensing* 12(22): 3694.
   DOI: https://doi.org/10.3390/rs12223694

The abstracts of all 6 articles are reproduced in Appendix 2.

6.2 Code-based analysis

6.2.1 Goals

The primary goals of the research projects described in these papers can be summarised as follows:

1. **Cultural heritage management**. Develop a remote sensing-based methodology to measure change and risk related to cultural heritage monuments, or to help predict future risks (papers 1, 2, 6).

2. **Understand the past, based on RS**. Use remote sensing to identify archaeological features in the landscape and enhance understanding of the past (papers 3, 4).

3. **Understand the past, based on RS and ethnographical research**. Use a combination of ethnographical research and remote sensing data to enhance understanding of the past (paper 5).

Arguably, in addition to these declared goals, there is a further common goal in all of these papers, which is to demonstrate the efficacy of the use of remote sensing data for the purposes of research into archaeology and cultural heritage. This is stated explicitly in some cases, for example in the abstract of paper 2:

> The paper aims to detect the environmental changes, assessing the geo-environmental status, and creating some of the innovative solutions while using the integration between remote sensing and GIS techniques. The combination of SRTM, Corona 1966, Spot 1986, Landsat 1987, Orbview 2003, and Sentinel2A 2017 data will help in monitoring the changes around the study area. The Bands combination and the spatial statistical analysis are considered to be the most effective methods in the examination of the new built-up indices. GIS techniques and some models would be suggested as solutions to protect the archaeological area, according to UNESCO recommendations. (Elfadaly et al., 2020)

In others this emphasis is implicit in the context of the journals of publication and their thematic concerns: ‘Computers, Environment and Urban Systems’, ‘Geosciences’, ‘Remote

Sensing'. In spite of their diverse objects of study, all of these papers are *technique-led*—they proceed from the novel possibilities afforded by the availability of remote imagery, and tools for processing these. Their shared claim is that these uses of technology have the capability of furthering knowledge in the spheres of archaeology and cultural heritage. I conclude that that this was as much the starting point of these research projects as any gap in the archaeological record or hypothesis about the past.

6.2.2 Claims

The following series of extracts provides summaries of what I take to be the key concluding claims in the respective papers. I quote them verbatim to avoid misinterpretation or misrepresentation:

Paper 1:

> In conclusion, it should be stated that remote sensing data sets and the technological tools used in the study, provide a non-destructive, cost effective and systematic method for management and monitoring cultural heritage sites.

Paper 2:

> The analysis of multi-temporal satellite data processed using GIS, SNAP, and Envi software provided invaluable information and provided some of the innovative solutions for risk mitigation.

Paper 3:

> The results presented in this paper are convincing proof of the critical importance of Geosciences and remote sensing approaches to expand the archaeological knowledge about the expansion of the Roman state in the Iberian Peninsula.

Paper 4:

> Satellite remote sensing data were used to extract and map Milan's tuntian system and to uncover traces of an ancient Chinese military—agriculture landscape. Based on the GF-1 extraction results and historical records, the hierarchical irrigation structure of Milan's tuntian landscape was revealed and the functions of the different types of canal were discussed.

Paper 5:

> This study, using detailed ethnographic fieldwork and analysis of modern snow and grass cover, has shown that there is a strong correlation between modern patterns of seasonal movement and those apparently practiced in the Bronze Age.

Paper 6:

> Although future enhancement is necessary to improve accuracy, this initial version of our change detection workflow highlights issues which are specific to each of the case study regions. […] EAMENA's change detection methodology allows the most at-risk sites to be identified rapidly and targeted for recording and preservation.

In summary, the claims in papers 1, 2 and 6 are concerned with the efficacy of satellite imagery and its processing in GIS software, to optimise the detection of change across large areas, thereby providing support for the management of cultural heritage sites.

The claims of papers 3 and 4 relate to the importance of the use of satellite imagery and GIS processes to identify archaeological features.

Paper 5 claims that ethnographical analysis, satellite imagery and GIS were used in combination to successfully make a link between the historical and modern seasonal movement of transhumant pastoralists.

* * *

The 'archaeological identification' claims of papers 3 and 4 are the most straightforward to substantiate. Satellite imagery gives a new perspective on areas which have not previously been surveyed from above, and tried and tested techniques such as crop-mark analysis can be used to identify sites of interest. Therefore, in paper 3, the application of this surveying technique allows the authors to claim the discovery of 66 'possible' new Roman military sites of archaeological interest, along with 'many other archaeological features' (Menéndez Blanco et al., 2020:10), which were not otherwise noticeable on the ground. Their identification of the sites as Roman military installations is based on the architectural aspects of the features, and their match with known sites. A match is assumed based on the existence of ramparts, defensive features at some entrances and the 'playing-card' shape of

site layouts. While the shape of layout is taken to be a defining feature, it seems that the interpretation can accommodate other layouts influenced by landscape topography:

> The playing-card shape has been recognised as one of the most characteristic features of the marching camps of late Republican and early Imperial times [57,59]. Nevertheless, these typological models were adapted where necessary. (Menéndez Blanco et al., 2020:14)

One might expect the ramparts to be the defining characteristic in the absence of the distinctive playing-card shape: but in the list of identified sites, 4 of the 66 lack the regular outline shape and were also ambiguous in their defence features. In two of these cases, qualifications are given that 'further research is needed' (ibid, p20) or that the site 'demands an in-depth archaeological exploration' (ibid). In the other two, the 'typological models were adapted' (ibid, p14) with regards to layout (in other words, the site layouts did not fit the models), but other distinctive similarities to well-documented sites are noted. In spite of these inconsistencies, these 4 sites are positively identified as Roman sites. (The authors are more circumspect with other sites where canonical features are lacking or are ambiguous, stating that 'those do not show morphologies that are clearly recognizable as Roman military structures' (ibid, p24), and excluding them from the list of 66).
A close reading of the paper, therefore, finds justifications listed for the claim of 'possible' military sites in most cases, albeit with some inconsistencies: but it is striking (at least to the non-expert) how much faith is put in the presence of highly generic architectural features (e.g. rounded corners of buildings) to link archaeological features to the period of interest. Authority is established partly through the mechanism of bringing in independent witnesses to the validity of these architectural criteria, in the shape of academic references. These criteria are closely adhered to in order to find positive results, but can be conveniently ignored in the case of anomalous results which nevertheless have a documented precedent.

Paper 4 also looks to exploit the functionality of GIS software, in this case to develop an understanding of an ancient Chinese military agriculture landscape. The authors' use of GIS allows for the convenient manipulation of a series of remote sensing images in a map-based interface, one which allows for the creation of hand-drawn vector graphics to delineate archaeological features. The image and data-handling GIS software interface acts as the

medium which enables interpretation; aside from the use of an NDVI[31] map there is no use of data processing for spatial analysis, though this is mooted as a future research goal. The paper's claims relate, therefore, to the interpretation of the remote sensing images and the classification of sites into mountain-oasis-desert ecoystems. Beyond describing the mapping process and putting forward the claim that a 'hierarchical irrigation structure' has been revealed as a consequence, the authors modestly note that they have 'discussed' the functions of the different types of canal (Luo et al., 2017:87): no conclusion is asserted. The future research goal hints at the untapped potential of spatial analysis. Again, this functions both as a roadmap for future research *and* as a means of condoning and building consensual approval for a particular research approach.

The claims of papers 1, 2 and 6 are related to semi-automated data-processing operations which detect change over time in satellite imagery, thus providing efficient and timely analysis of factors affecting the conservation of monuments. The claims in these papers are substantial: namely in 1, that the techniques provided time-saving, cost-effective improvements over traditional monitoring methods; in 2, that innovative solutions for risk mitigation were provided; and in 6, that the methodology used allowed for the rapid identification of the most at-risk sites. These claims are defences of the methodologies described, including specific algorithmic formulae used for processing the visual data, as well as of the use of remote-sensing technologies in general.

Paper 2's main concern is the creation of an analytical system which can quantify threats to cultural heritage, thus lending authority to recommendations for damage prevention or mitigation. It makes no claims for time-saving or efficiency benefits. In pursuit of these goals the authors make use of a combination of 'unsupervised' procedures (a computer executing a predefined task without intervention), classification in this case, and 'supervised' statistical analysis, a process requiring human intervention. Unfortunately no detail is given about the automated classification, and the results of the statistical analysis are presented uncritically as proof of changes to urban, barren land, desert and waterbodies areas over

[31] Normalized Difference Vegetation Index, an indicator of the presence of local vegetation derived from a remote image.

the period studied. The methods adopted are 'considered to be the most effective' but no alternatives are presented with regard to the calculation of building, soil and water indices from satellite imagery.
On the basis of the results a recommendation is made to build dams in three local streams to minimise the risk of flooding, a strategy of potentially very wide-ranging impact. The thinking behind this recommendation is not discussed. This paper, abbreviated in its justifications but highly interventionist in its recommendations, is an illustration of just how high-stakes some CH research projects can be, and of the related need for maximum transparency.

In Paper 1 the methodology is discussed in greater detail. Again, the goal is the quantification of risk, and this paper describes a method of producing an overall risk assessment for sites based on the aggregation of *individual* identified risks. The paper proposes and demonstrates the use of a particular data processing method, the Analytic Hierarchy Process (AHP) for the production of a final GIS-based 'hazard map'. Any consideration of the benefits or limitations of AHP, or alternative methods, are omitted: the 'proof' of the method's usefulness appears to be the existence of the final map, which reveals that:

> almost 40% of the archaeological sites considered in this study obtained a classification of HH [High Hazard] and VHH [Very High Hazard] monuments, clarifying the need to take certain actions for the protection and preservation of the monuments. (Agapiou et al., 2015:238)

It may be that other statistical approaches would have resulted in broadly similar results, and that the main benefit of the research is the efficient collection, analysis and presentation of risk-related data. However, as with paper 2, there is no context given to justify the use of the statistical method. In other respects, the paper is transparent and detailed about the research processes used.

Paper 6 gives a highly detailed account of the successes and challenges of a change-detection method using satellite imagery, relating the incidence of successful risk and damage identification along with false positives and negatives, verified by ground-level

checks. Specific examples are discussed where the selected method does *not* deliver good results, and it is acknowledged that overall, the levels of false positives and negatives are not ideal. Hence, the authors propose that the project will:

> continue to develop this methodology in the next phase of its research, including introducing classification of the types of changes, as well as focusing on improved accuracy. (Rayne et al., 2020:25)

Rayne et al. are admirably clear about the strengths and weaknesses of their methods, and their proposals for further research are concrete ones. The status of their research as a 'work-in-progress' is a positive and practical feature, rather than a general article of faith in future scientific progress.

Interestingly, in the context of the authors' measured account, this paper is one of the few cited which mentions the broader circumstances of the research, beyond the 'anthropic' and 'natural' threats to monuments: for example the impact of the 'slow internet' in Libya is highlighted, as are the colonial origins of the national mapping programs of the 50s and 60s in North Africa. The project has pragmatic goals and seeks to establish strong working links with local communities:

> Because the methodology uses open-source data and tools, it can easily be used by our network of heritage professionals who have been trained in its use and in remote sensing and GIS more generally. We urge other heritage management projects working in the region to also adopt open-source philosophies in the interests of democratising access to these technologies and preserving archaeological sites. (Rayne et al., 2020:27)

The discussions of the broader context of the research can therefore be interpreted as a recognition of the importance of *localised* agencies which can contribute and shape research projects just as surely as the observations from distantly orbiting satellites, or the statistical formulae used for translating image data into charts.

The authors of paper 5 look to develop a model for the seasonal movement of pastoralists in Xinjiang, based on the environmental variables of snow cover and water supply. They acknowledge the extremely simplified nature of such a model, but claim that its limitations

will allow them to validate the model with ethnographic accounts, and 'identify mismatches and problems for which additional explanations are necessary' (Jia et al., 2020:13). After taking remote sensing-based measures of the region's snow cover and vegetation index (NDVI) in order to attempt to correlate environmental conditions with ethnographic accounts of seasonal movement, they go on to compare the distribution of archaeological sites with the model-identified pastures.

Interestingly, the authors are careful to justify their use of the ethnographical approach for developing hypotheses—presumably because they assume the journal's audience to be unfamiliar with this type of research methodology. This makes them the only authors from the 6 papers cited who explicitly discuss the epistemological validity of their selected research methodology. Drawing on the work of Gould, they conclude that the circumstances of their study meet the rare criteria of an instance where the same invariant constraints on human behaviour exist in both past and present populations (Jia et al., 2020:11). They refer to the recommendation of Wylie that where 'the reconstructed behaviour is, by nature, a direct and exclusive consequence of impinging ecological or material conditions', the inferences 'may be raised to the level of deductive security' (ibid). In using this justification they emphasise the influence of environmental conditions over other considerations, including cultural factors. This is a useful statement of the authors' philosophical position. It is difficult however to evaluate their assertion without more detailed insight into factors such as the modern-day pastoralists' access to technology, transport networks, modern fabrics, etc.—factors which seem likely to change their relationship to environmental conditions. (Not to mention increasingly rapid changes to the climate itself.) Furthermore, the authors have a tendency to erase any uncertainties related to this approach: for example, they talk about 'proving' hypotheses using the ethnographical record:

> A body of evidence shows that the houses were used on a rotating seasonal basis; investment in the construction of permanent buildings was worthwhile because the houses were used regularly over many years. This can be proven in various ways, one of the most valuable of which is the recent ethnographic record. (Jia et al., 2020:4)

While a significant part of the project consists of ethnographical research, no detail is given of the methods used: we are simply told that:

> several field seasons have been devoted to study of recent camp sites and herding practices in order to gain a deeper understanding of Bronze Age land use patterns and economic decision making. (Jia et al., 2020:4)

The subsequent details of the modern-day herders' seasonal movements are recounted from a 'God's-eye view', as though reported from an invisible, all-seeing perspective. There is no suggestion that the researchers' narrative may in any way be constrained, contingent or partial, or that the presence of researchers may have affected the behaviour of the observed community, or influenced their recounting of their traditions and history. The highly opaque nature of this reporting is problematic, and presumably would not pass peer-review in a social science journal. The assumption seems to be that the audience for the paper is primarily interested in the remote sensing work: but there is no reference to a more detailed published account of the ethnographical research elsewhere.

In spite of the apparent orientation of the paper towards the scientific use of remote sensing, the conclusions drawn from that aspect of the work seem mostly inconclusive. The authors turn to explanations derived from their application of ethnographical research to make sense of behavioural patterns which are not predicted by the remote sensing data model:

> The Wenquan pasture seemed to only provide meagre grazing throughout the entire year. The area lies to both sides of a river. Water has a very low NDVI value approaching -1 and could considerably influence an average. However, even when excluding the river and the denuded surfaces of the riverbed, the NDVI average stayed low. Therefore, other explanations needed to be considered. According to ethnographic data, the most important reason for the usage of Wenquan as a spring pasture was the early temperature increase which allows for a shortening of the winter camping time. (Jia et al., 2020:17)

The analysis of remote sensing data seems to be useful only in that it does not, for the most part, *contradict* ethnographical accounts. It does not seem to have independent predictive power or be helpful for filling a particular gap in the knowledge. From one perspective this may be deemed a useful outcome: it can be seen as a separate, confirmatory strand of

research which complements the ethnographical approach. The remote sensing approach may therefore add to the robustness of the claims of the ethnographical approach; but in light of their tentative claims for it, the closing statement, that:

> this study, using detailed ethnographic fieldwork and analysis of modern snow and grass cover, has shown that ***there is a strong correlation*** between modern patterns of seasonal movement and those apparently practiced in the Bronze Age (Jia et al., 2020:21, my emphasis)

is somewhat lacking in nuance. Nevertheless, with its descriptions of methods, the paper can be understood as a kind of handbook for future practitioners, who may, it is implicitly suggested, find the same techniques useful or perhaps more productive in other contexts.

6.2.3 Witnesses

In their study of Boyle's 17th century experimental work and contribution to the establishment of the 'scientific method', Shapin and Schaffer describe how he enrolled witnesses to his experimental demonstrations, as a means of lending authority to his knowledge claims about the nature of air:

> Boyle insisted that witnessing was to be a collective act. In natural philosophy, as in criminal law, the reliability of testimony depended on its multiplicity…
>
> The thrust of the legal analogy should not be missed. It was not merely that one was multiplying authority by multiplying witnesses (although this was part of the tactic); it was that the *right action* could be taken, and seen to be taken, on the basis of these collective testimonies. (2011:56-7)

This perspective on Boyle's scientific work has strong resonances with Actor Network Theory, and is a rearticulation of the notion of enrolling the interests of actors in the endeavour to *mobilise* knowledge, that is, construct facts which can be accepted and made use of in contexts other than those in which they were first derived. As Shapin and Schaffer point out, this programme of enrolment was not limited to Boyle's physical demonstrations: he was able to extend the act of witnessing beyond the assembly rooms of the Royal Society by way of producing written descriptions:

> Another important way of multiplying witnesses to experimentally produced phenomena was to facilitate their replication. Experimental protocols could be reported in such a way as to enable readers of the reports to perform the experiments for themselves, thus ensuring distant but direct witnesses. Boyle elected to publish several of his experimental series in the form of letters to other experimentalists or potential experimentalists. (Ibid, p59)

In the same way, the publication of the papers described here can be understood as a way of enrolling witnesses to the research projects undertaken by the respective authors. In sharing their work with a select, specialised audience, and hopefully gaining the approval or assent of their readers, the authors aim to add value and authority to their findings. A significant part of the process of securing approval is the achieved by the community's adoption of the methods and assumptions related in the papers. The description of methods in the papers can thus be seen not just as a basic requirement for accountability and assessment of validity, but also as formulae for constructing knowledge, a means for encouraging others to partake in the same programmes of research and of building consensus for assumptions about what can be known. This is stated explicitly in papers 1 and 6, in which the authors offer their research practices as blueprints for others pursuing similar projects.

This perspective might help to explain the tone of some of the claims in the papers. It seems that it is not enough for the authors to simply relate the results of the research: they must also make strong claims about the generalisability of the methods used, and this is done sometimes even in cases where the results of those methods are somewhat inconclusive. The language of paper 3, for example, is noticeably more dramatic and urgent than that of the other papers: the new data will 'shatter' old debates and theories; enhanced methodologies will 'forever change our views'. This kind of language seems overblown in the context of an academic article, a form usually characterised by the effort to persuade by the use of evidence and argument. Such dramatic flourishes also seem unnecessary when the efficacy of the use of satellite imagery for identification has been clearly demonstrated. However, identification is not the only goal of the paper; the use of persuasive language appears to be part of a general advocacy for the use of spatial analysis techniques. While introducing the disclaimer that they wish to 'discard a strict topographic determinism' (Menéndez Blanco et al., 2020:35) in their research, the authors nevertheless look to exploit

the affordances of the manipulation of data in GIS in support of their interpretative efforts. The results of their applications of spatial analysis are either ambiguous—in the attempt to link irregular camp shapes to landscape topography (ibid, p14)—or inconclusive, in the attempt to link camp locations to topography or water courses (ibid, p30). The uncertainty of these outcomes is not a consequence of the avoidance of determinism, for example by introducing qualifying evidence: rather, it seems that the application of the analytical techniques was unproductive in itself. This does not stop the authors from advocating strongly for these methods:

> The results presented in this paper are convincing proof of the critical importance of Geosciences and remote sensing approaches to expand the archaeological knowledge about the expansion of the Roman state in the Iberian Peninsula. […]
>
> Far from being mere accumulative information, these data support the development of more precise methodological guidelines, and even predictive models, for surveying other areas where similar historical phenomena took place. (Ibid, p36)

It is difficult to avoid the impression that the paper seeks here to act as a validatory resource for the geosciences research approach, as much as an enlightening narrative about the incursion of the Romans into north-west Spain. This functions to reinforce the credibility of the research programme the paper belongs to. The authors position themselves as witnesses to the 'critical importance' of geoscientific methods, and invite readers to corroborate their account. This can be read as an appeal to be recognised as worthy members of a specialised academic community which is centred on work of this kind, and/or as an attempt to contribute to the discourse of that community and establish authority within it. The legal analogy highlighted by Shapin and Schaffer, and the imperative to take the 'right action' (ibid, p56-7) based on witnesses' testimonies, emphasises the moral quality of research practice. An important part of being a worthy member of a research community is about being *ethical* or *moral*; advocacy for particular methods can therefore also be interpreted as an assertion of their integrity—that they represent actions which are 'correct', for example by embodying objectivity, even in those cases where the results they produce might appear to be inconclusive.

The articles site their technical images in privileged, non-performative spaces, so that they can function not just as enablers of the sharing of process, and enablers of witnessing, but also as expert witnesses themselves. One strand of their authority is derived from their apparent neutrality, their separateness, and is a consequence of 'the asymmetrical faith we place in our access to representations over things' (Barad, 2007:49). This separateness is physical as well as philosophical: the view from above is authoritative exactly because it is so 'unhuman' and thus has the appearance of being untainted by human fallibility. But there is also a sense that the technical achievements which the images represent, lend, in their sophistication, a further stamp of authority to the research processes and related claims described in the articles. The industry and mastery of complexity required to put a satellite in space and share photographs from it to the planet's surface below—or to develop a method to use laser sensing to create a 'point cloud' which is then rendered as a 3D computer graphic (LiDAR)—is highly impressive. Such achievements provide a platform of credibility on which to conduct research; they are representative of a series of knowledge 'steps', of accumulated wisdom, which with their impression of forward momentum, seem to offer the possibility of taking further steps forward from where they have left off. Arguably, a core part of their appeal (at least for some) is that they embody an underlying narrative of the *mastery* of nature, reinforcing the perception of distance between humans and human-made representations, and the world. These qualities can help researchers to narrativize their work, positioning it as part of a long march towards truth:

> In the years to come, the increasing access to new geospatial datasets and the enhancement of archaeological methodologies will forever change our views on the Roman conquest of northwestern Iberia. (Menéndez Blanco et al., 2020:36)

That technical images can encapsulate such qualities is an illustration of their richly performative and agential status, in spite of their neutral appearance. The technological platforms which produce these images open new avenues for research, but at the same time provide warrants of seriousness or legitimacy by virtue of their own impressive technical achievements, which are in themselves proofs of effective, transitive knowledge and the tangible qualities of human agency.

All of the papers make generous use of illustrative diagrams and photographs to demonstrate their working methods and share the evidence they have interpreted. Also common is the use of tables or matrices to communicate quantitative data. Less frequently (only in papers 2, 5 and 6), do the article authors use graphs in the shape of bar charts and the plotting of points on an XY grid, to visualise numerical data. This is a reflection of the methods used and the scale of the datasets under discussion. In most cases, the datasets are small enough that data points can be named individually in a table row. Much of the work takes place in the conceptual spaces of geophysical landscape and cartography, so the visualisations largely consist of photographs of landscapes, processed photographs or other landscape renderings—for example ones constructed using LiDAR data, as in paper 3.

The authors of paper 1 use references to justify the use of satellite imagery, as in this example:

> Satellite imagery can provide a quick and relatively low cost approach for monitoring natural and anthropogenic hazards over large and inaccessible areas (Youssef et al., 2015; Kaiser et al., 2014; Pradhan, 2010; Rahman, Shi & Chongf, 2009; Biswajeet & Saro, 2007). (Agapiou et al., 2015:230)

They go on to note that cloud-free coverage is required, but otherwise do not explain or interrogate the process of procuring or making use of imagery from hardware circling 500km above Earth. This might be interpreted as a presumption of an expert readership who are already well informed about the technicalities of satellite data use: it can also be seen as a black-boxing of this foundational part of the research. In this next example a series of complex transformative steps are presented in the briefest of summaries:

> The supervised classification, remote sensing Indices (built-up indices), and spatial statistical analysis methods have been used in this study. The Digital Elevation Model (two-dimensional (2D)) and SWAT model have been extracted from SRTM radar data. The layer stacking, dark subtract, geometric correction, unsupervised classification, supervised classification, and post supervised classifications techniques are carried out while using ARCGIS 10.4.1, SNAP 6.0, and ENVI 5.1 software. (Elfadaly et al., 2020:5)

This presentation is geared towards a notion of reproducibility. What matters here is that version 10.4.1 of ARCGIS is used to achieve the results, rather than the detail of the actual changes effected by the software. The correct black boxes must be used. Presenting these processes ('layer stacking', 'dark subtract', 'geometric correction', etc.) as an unproblematic basis of knowledge creates a solid platform for the subsequent analysis, but neglects some of the important contingencies involved in their use. Some of those involved in the use of satellite imagery are treated in detail by the authors of paper 6:

> The EAMENA workflow (Figure 2) comprises computing change between composite Sentinel-2 (Level 2A, surface-reflectance) satellite images using Google Earth Engine. Including other satellite data could improve the sophistication of future incarnations of our workflow, but at present, using just one sensor limits issues caused by radiometric and resolution differences [24]. […]
> The Sentinel-2 imagery is split into two collections according to the date function, one representing the later date the user defined and another representing the earlier date they wanted to compare it to. Each collection is then filtered according to the area of interest the user defined and to limit cloud percentage to less than 10%, based on the imagery metadata. Pixels which pass the requirements will be retained in the collection. A cloud mask is applied using a quality assessment band provided by ESA with the data and takes into account dense cloud (high reflectance in blue) and cirrus (high reflectance in a cloud screening band, low reflectance in the blue band, see Reference [25]). The composites are made by calculating the median values of each pixel (in all bands) in the collections. This uses the Google Earth Engine function 'reducer' to perform the compositing from the images in the collection, generating a single output image. (Rayne et al., 2020:4-5)

This kind of contextual information is crucial in light of the function of the imagery shared in the publications, which is to give transparency to the research process and allow for witnessing to take place. The images put the reader into the researcher's chair, allowing them to see what the researcher saw. The appeal to the visual senses is of course an extremely powerful one: 'seeing is believing'. Visual evidence, from Euclid's graphical geometric proofs, to Galileo's sighting of Jupiter's moons, through to modern-day photography, seems to offer a shortcut towards truth: it leaves nothing left to be said. The dangers of relying solely on empirical sense-data for sense *making* have been widely discussed, not least by the critical realists I referred to earlier (§3.1). Nevertheless, as the primary way of experiencing the world for most of us, visual interpretation can appear

direct and unmediated in a way which symbolic systems of communication cannot; and remote sensing imagery is further bolstered in its apparent trustworthiness by an aura of technological authority. In the legal analogy, these qualities allow researchers to call upon RS techniques and their outputs as credible, reputable sources for backing up their claims. It requires a rigorous researcher to properly cross-examine such witnesses in order to clarify the exact nature of their contribution. Detailed accounts like Rayne et al.'s above provide at least a partial corrective to the impression that such imagery is unmediated and beyond human agency. (A similar lesson in relation to the creation of 3D computer graphics for archaeological reconstructions, regarding the need to contextualise, or even problematise the image production process, is discussed in §3.4.1.)

6.2.4 Certainty

To what extent are authors certain in their claims, and do they leave space for contingencies? Doubtlessly there are factors external to the texts themselves which could have had a bearing on the expression of certainty: the stage of the authors' careers, or their level of expertise in the field of RS interpretation; the cultural capital they have access to; or the stylistic expectations of their community's research practice. These factors are unfortunately beyond the reach of this study, so while acknowledging their possible influence I will limit my comments to the structural and stylistic aspects of the texts themselves.

Looking solely at the conclusion sections in each paper, my interpretation of the degree of certainty expressed about each paper's claims was as follows:

1. High
2. Moderate
3. High, with qualifications
4. High, but based on modest claims
5. High
6. High, with many qualifications and detailed examples

The general trend is unsurprising: while in theory a journal might publish a paper which expresses low certainty about its conclusions, because it provides value in some other respect, in practice this is likely to happen only infrequently. The assessments, by necessity impressionistic, are a reflection of the vocabulary used, the clarity of the claims and the reported suitability of the research approaches taken for future projects, as in paper 1:

> The results of this study can be used as a road map for taking specific actions regarding the protection and/or consequent restoration of the archaeological monuments. (Agapiou et al., 2015:238)

It is important to recognise the different scopes of enquiry in these papers, and adjust any assessment of the handling of certainty accordingly. For those papers with archaeological goals, rather than the goal of cultural heritage management, the questions asked are massively more complex and open, and one would expect this difference to be marked by a distinctively different discourse. Paper 3 is a useful test case for this, as it combines the goals of *identification* of sites with an attempt to improve *understanding* of the movements of the Roman army in the Iberian Peninsula around the 1st century BC. The language in the discussion section which relates to hypotheses about the Roman military and their relationship to indigenous peoples is indeed cautious and measured in comparison with the sections related to the identification of sites; as discussed above, the authors are fairly clear that in relation to this, their efforts to find meaningful patterns in the location of sites proved inconclusive. In one exceptional instance the authors have no reservations about claiming, with the help of a reference, that a fact was archaeologically proven:

> Current data from both Cantabria and northern Castile reveals that this area was a hotspot in early Imperial times, and the diachronic presence of fortifications at the same spot has been proved archaeologically [186]. (Menéndez Blanco et al., 2020:34)

Otherwise they emphasize the need for further research, recommending a holistic approach which joins the field of Iron Age research with that specialising in the Roman military, in order to capture the diverse responses of the indigenous population to Roman incursions. This cautious tone finds a contrast in the preceding 'Results' section, where the identification of sites is documented. Aside from a few mentions of the need for more in-

depth archaeological investigation, the archaeological traces are regularly referred to without qualification as Roman camps (see also §6.3).

Papers 1 and 6 both include a 'ground-truthing' step in their methodological processes. This verificatory step is simplified by the availability of pre-identified sites alongside the remote sensing data. In this respect, the authors of these reports have a more straightforward job than those who look to use remote sensing for new site identification or to develop an enhanced understanding of the past. For the latter, 'ground-truthing' may be useful for confirming that patterns identified from above are indeed traces of archaeological remains, but this is a verification that further research can be undertaken—the sounding of a starting gun, rather than the crossing of a finishing line. No narrative accounts are given of the on-site surveys in paper 1, with the tacit implication being that the researchers had an omniscient view, experiencing no impediment to their survey of conditions on the ground. This seems highly unlikely, and by way of contrast, Rayne et al. acknowledge in paper 6 that:

> Sites were selected for validation based on their accessibility and the urgency of their condition; for example, several sites in the Aswan area were in the process of being destroyed, due to construction work, but this work also meant that we could not get close enough to some sites. (2020:7)

Perhaps unsurprisingly, the authors of all articles report that their results are of value in some way. In some cases, limitations are outlined:

> Although future enhancement is necessary to improve accuracy, this initial version of our change detection workflow highlights issues which are specific to each of the case study regions. (Ibid, p28)

> The different techniques and resources used here have repeatedly shown the potential of open-access and geospatial data, but also their limitations. In our case, LiDAR was not reliable in anthropised areas or extensively exploited agricultural lands [2,81]. Also, the data has low ground returns. High-resolution satellite coverage is rarely produced taking into account optimal conditions for revealing archaeology, and only a few series randomly end up on easily accessible engines and platforms such as Google Earth or Bing Maps. Even considering the full range of available datasets and the considerable potential of these landscapes for developing

> remote sensing approaches, the detection of archaeological features only came under particular conditions [66,68]. (Menéndez Blanco et al., 2020:35-36)

Looking in more detail at two papers which present their claims with unproblematic certainty in their conclusions, papers 1 and 5, it is interesting to see if this tone is matched in the language used elsewhere in these same reports. As noted above, the authors of paper 5 are careful to include a justificatory section for their partial use of an ethnographical approach, and they rely on the assumed historical consistency of environmental conditions in Xinjiang to lend their ethnographical results 'deductive security'. Their 'Methodology and Results' section is devoted solely to remote sensing research, and this is a reflection of the scientific positioning of the paper and its publication in a science journal. The vocabulary related to the remote sensing work is very moderate in its conclusiveness:

> Like every model this is a radical simplification of a complex human-environmental interaction, but we are trying to capture the essence of what makes a specific location preferable to others with regard to a localized subsistence economy. (Jia et al., 2020:13)

> The topographic location of the Wenquan spring/autumn pasture offered explanations beyond the simplistic assumptions of the model. (Ibid, p.17)

> The general model does provide insights into potential usage patterns of the landscape (Fig 11). The combination of snow cover and grazing quality seems to show relatively clearly what a particular area is best used for and the results match up with the ethnographically assigned usage. (Ibid, p.18)

These circumspect observations are not an obvious match for the ultimate conclusion which states that 'strong correlations' have been identified between the patterns of seasonal movement of modern and Bronze Age pastoralists, and cites the remote sensing approach as part of the basis of this conclusion. In the case of paper 1, the authors report their results without equivocation. In one specific scenario they use statistical means to handle uncertainty:

> The results (see Table 2) revealed the high importance of tectonic activity and urban sprawl phenomenon in the model development. After the calculation of the normalized weights, the consistency of the responses was checked by calculating the

> consistency ratio (CR). […] If the ratio exceeds 0.1, the set of judgments may be too inconsistent to be reliable. However, in practice, CRs of little more than 0.1 are accepted and the extracted weight values are considered as reliable (Alexakis et al., 2013). (Agapiou et al., 2015:237)

Otherwise, the results are presented in factual terms. Their conclusions are therefore in keeping with the overall tone of the paper, and there is no discussion of any possible limitations to the research approach adopted.

6.3 Translations

Papers 1, 2 and 6 look to construct a repeatable process for the monitoring and management of change, in relation to cultural heritage sites. One motivation for this is laid out in paper 6:

> …many archaeological sites are rapidly disappearing more quickly than human archaeologists can work to regularly monitor them manually. The change detection workflow described in this paper, therefore, offers a way of addressing this. (Rayne et al., 2020:27)

As far as is possible, they seek to make this process a black box in order to produce useful results efficiently and quickly. The production of a document which both encapsulates and contains the process is necessary for making the practice mobile, that is, performable by others in other contexts.

The authors of paper 1 use a predefined set of risk types, with two parent categories of 'natural' hazards and 'anthropogenic' hazards, which are divided into subcategories before being given a 'weighting' in the system, based on the AHP methodology. Importantly, the final step involves 'ground truthing' the research by physically checking the sites. Presumably this verification step was made only for those sites which were indicated by the previous steps to be high risk, as exhaustive checks would remove any efficiency advantage proposed by the authors. The authors of paper 6 also document the use of ground-based checks to validate their results and adjust their error matrices. In both cases, then, while the research process entails a complex series of technological transformations and translations between 'material forms' (Lucas, 2012:238), so that the sites being researched are replaced

as the focus of attention by digital assemblages of maps, photographs and numerical data, there is a reliance on first-hand empirical data from the ground to verify results. This is evidence of an epistemological hierarchy and implies a desire to root knowledge in personal, sensory, 'untranslated' experience, where it is available. The use of highly mediated technologically produced models does not necessarily represent a preferred alternative for accessing 'truth' in these contexts; its main perceived advantage is scope and efficiency.

I noted above that in their 'Results' section, the authors of paper 3 refer to the archaeological traces they have identified as Roman camps, mostly without qualification. The 'Results' section commences with the observation that:

> 66 ***possible*** Roman military sites were detected in the study areas during our survey. (Menéndez Blanco et al., 2020:10, my emphasis)

Inserted immediately afterwards is a table listing the same 66 sites, entitled 'Roman military sites in northern Castile (study area 1)': the 'possible' has been replaced, tacitly, with the 'actual'. The translation between representational forms, from a series of explorative site observations to a table of quantified results, acts as a filter for uncertainty. The interpretations are hardened into facts, and this new epistemological and ontological commitment is further cemented by the addition to the table of a column allocating hypothetical garrisons, along with troop numbers, to each of the newly discovered 'camps'. The move to a new representational form represents the move to a new state of knowledge, and the laying of a new base level upon which further claims might be built. This in turn enables the same operation to take place at the level of the article, which becomes a referenceable entity in its own right.

6.4 Writing as knowledge production

In his analysis of archaeological writing, Lucas draws on Hacking's proposal that 'different styles of reasoning have different ontological and epistemic commitments' (2019:99). The term 'reasoning' is used, rather than 'thinking', because:

> it takes place outside the mind as much as inside. More importantly, though, Hacking sees different styles of reasoning as accompanied by different kinds of objects, evidence and ways of constructing knowledge claims. (Ibid, p97-8)

The notion of reasoning 'outside the mind' can be understood as complementary to the ANT concept of agency in non-human actors as well as human ones; the idea that:

> In addition to 'determining' and serving as a 'backdrop for human action', things might authorize, allow, afford, encourage, permit, suggest, influence, lock, render possible, forbid, and so on. (Latour, 2005:72)

Lucas draws a connection between Hacking's 'reasoning styles' and Fleck's 'thought-styles' (Fleck, 1981) but suggests that while Fleck's concept of 'Denkstil' was 'largely very specific to disciplines', the divisions between reasoning styles:

> do not align with the divisions between sciences, but cut across them; a single discipline can and will display more than one style and indeed styles can be blended together. (Op. cit., p98)

Proceeding from this basis he seeks to apply Hacking's insight to four types of textual knowledge production in archaeology: description, narration, exposition and argument. His goal is to rethink these styles of reasoning in terms of their epistemological commitments. The following table summarises his detailed exposition:

Table 4. 'Text types and their epistemic features': reproduced from Lucas, 2005:133.

Text type	**Epistemic function**	**Epistemic strategy**	**Epistemic register**	**Epistemic virtue (Mode of Detachment)**
Narration	Story	Endings	Hindsight	Historical distance: being both 'after' and 'part of' the event
Description	Testimony	Conventions	Presence	Objectivity: being both 'there' and 'not there'
Argument	Advocacy	Warrants	Standpoint	Impartiality/neutrality: being the view from 'somewhere' and 'nowhere/everywhere'
Exposition	Enquiry	Distinctions	Ignorance	Clarity: being both 'familiar' and 'unfamiliar'

One of Lucas's main conclusions is that all of these types of text share a *common* epistemic virtue—that of *detachment*. In providing distance, objectivity, and so on, these qualities of detachment make the mobilisation of the knowledge claims possible—they make local truths transmutable into global ones, and admit the possibility of applying claims to other contexts:

> this broad distinction between text types which embed knowledge and conceptual vehicles which mobilize it is traversed by the fact that, in both, there is a tension between detachment and engagement. In all four text types, we saw how each embodied different epistemic virtues which expressed this tension in different ways: hindsight, impartiality, objectivity, unfamiliarity. Similarly, in the various conceptual vehicles, there was need for concepts or models to be contextualized with facts and yet not so attached that they could not move to work on other facts. (2005:159)

Applying these insights to the collection of papers under scrutiny, it is worth remembering that all are case studies, rather than general theoretical papers. All therefore exhibit traits of

narration and *description*. The depersonalised mode of narration is common, one which removes traces of authorial subjectivity:

> The Paphos district was chosen as a case study… […]
>
> Initially the potential hazards of the case study area were defined. Both natural and anthropogenic hazards were examined and evaluated. The hazards were divided into two main categories. (Agapiou et al., 2015:231)

As Hodder notes (§2.2.2), this mode can be used to help authorise the findings of interpretative work. By way of contrast, the subjective narration used in some articles was useful for cultivating authenticity and producing a narrative mode familiar from fiction, where the reader 'is' the first-person narrator but is also the all-seeing eye of hindsight. This lends the account an impression of analytical 'destiny' which would not necessarily be available in other formats, such as journal notes or documentary video clips, for example. In the papers, it is possible to see a range of positions between, to use Lucas's terms, detachment and engagement: from the highly detached mode of paper 1, through to the often more engaged approaches of papers 5 and 6, where 'we' refers to the research team rather than an abstract, ideal knower-researcher or an assumed consensus:

> We classified monthly snow cover in each pasture to thin, moderate, and thick based on the average snow cover. (Jia et al., 2020:16)
>
> We visited a sample of the sites in the field in both Egypt and Libya in 2020 to validate the automated approach and adjusted the error matrices accordingly. (Rayne et al., 2020:7)

In these latter papers, detachment is instead provided via technologically-mediated empirical data collection and analysis. This emphasis stems from the 'scientific' positioning of the articles, in their methods and choices of journal of publication. In addition to the application of the logic of mathematical and graphical transformations of the data for *argument* and at times *exposition*, the use of technologies for data collection and interpretation also functions as a warrant for claims, for argument: as posited above, these can be called upon as reliable witnesses, given authority by their substantial effectiveness

and synechdochic representation of sustained chains of reasoning. It is the interplay of these variations of engagement and detachment which makes these accounts convincing (or not): the engaged presence of the first-person account, and the simultaneous all-seeing hindsight; the painstakingly particular application of technical analysis, and the ostensible unthinking commitment of technology to impartiality.

Lucas's model of textual analysis prompts us to think about how archaeological writing can convey or produce 'knowability'. He is concerned with, and stops at the level of, the text as producer of meaning. But the text-oriented approach cannot easily account for the importance in knowledge-making texts of authorial authority; of *credibility*:

> The notion of credibility permits the linking of a string of concepts, such as accreditation, credentials and credit to beliefs ("credo," "credible") and to accounts ("being accountable," "counts," and "credit accounts"). This provides the observer with an homogeneous view of fact construction and blurs arbitrary divisions between economic, epistemological, and psychological factors. (Latour and Woolgar, 2013:239)

Credibility has the capacity for amplifying or undermining our impressions of engagement and detachment in research outputs. Almost every academic publication, in its layout, prioritises authorship above everything other than the title of the text; and there are strict conventions which ensure that contributors' affiliations with institutions are also clearly declared, thus allowing readers' assessments to be informed by the institutional as well as the personal status of authors. Each knowledge claim is a dynamic relationship between the evidence described and the knower who recovered or experienced it, and the security of the claim rests in part on the perceived trustworthiness and authority of the knower. (While the promise of *original* or *insightful* analysis is that it can generate authority, *independently* of the status of the researcher/knower.)

I suggest, based on my analysis of papers here, that the academics listed at the top of articles are not their only authors. The human authors act as amanuenses for the non-human actors, willingly handing over the pen or the keyboard, and the responsibility for observing and representing, to the satellite camera, the computer-generated landscape visualisation, the algorithm which detects changes in the individual pixels of hundreds of

images at superhuman speeds, and the software function which generates the plotted graph. The paper authors borrow the credibility of these actors, and lean heavily on the detachment they are perceived to possess by virtue of being non-human and apparently disinterested. The other recruits in the army of co-authors are the texts and the academics listed in the references of the articles. The papers are intertextual, a conversation with other texts and historical subject-specific thought. In Latour and Woolgar's formulation, laboratory work is effectively the process of converting speculative statements into accepted scientific knowledge ('Type 4' statements in their schema):

> Activity in the laboratory had the effect of transforming statements from one type to another. The aim of the game was to create as many statements as possible of type 4 in the face of a variety of pressures to submerge assertions in modalities such that they became artefacts. (2013:81)

The references in articles are useful for performing those operations which can transform the status of the authors' artefactual statements into accepted knowledge, for example by confirming a finding.

Sinclair suggests that archaeological writing can function something like an observational instrument. Drawing on the work of Ihde, he distinguishes between instruments which involve 'embodiment-relationships', that is, direct embodiments of the world (e.g. a magnifying glass), and those which radically transform the world via a hermeneutic relationship, often involving amplification and reduction (e.g. an electron microscope):

> We perceive the archaeological record through previous work which gives it structure. When analysing archaeological texts, we can explore the changes in the categories used as though they were a means of perception which is developing. They are evidence of the way in which the outside world has been structured. Through time changes in this structure have cumulative effects and these transform the perceived world. As time passes, our means of perception (continually learned through exposure to texts), becomes less like a magnifying glass and more like the electron microscope, giving particular emphasis to one feature out of a previously greater number. We, thus, perceive the archaeological record in a manner which resembles Ihde's hermeneutic relationship, amplifying certain aspects and reducing others. (2000:481)

He goes on to demonstrate how archaeological typologies can become the basis of understanding themselves, rather than their referents:

> I shall concentrate upon just one example; the way in which the profound variability (and hence ambiguity in an interpretive sense) present in all lithic collections is gradually controlled. Meaning is given and formalised to the extent that the stone tools themselves are no longer what is observed but rather the profiles of assemblages on graphs and the indices of particular families of stone tools types. (Ibid, p484)

This is the product of a sequence of historical writings, wherein 'each successive genre has inherited and amplified a particular perception of its material' (ibid, p485). (See also 'xeroxing', §2.3.3) Perhaps it is by the deployment of typologies as a 'material context' (Boozer 20015:94), that Menéndez Blanco et al. can so confidently refer to the sites they have identified as 'Roman camps'. In one sense, their identification of sites can be understood as correspondences to the limited criteria of typologies, rather than as relationships to historical sets of events and circumstances. Certainly Sinclair's notion of a 'hermeneutic relationship' is useful for understanding how bodies of knowledge evolve to be what he calls the 'building blocks of our genres', or in my terms, co-authors. His call to pay greater attention to the processes of amplification and reduction involved in the 'unconscious structuring which we inherit' (op. cit., p487) reinforces the need to look beyond the level of the text and 'text styles' and to try to understand knowledge claims in the wider context of 'thought communities'. These consist not just of textual reasoning modes, but of reasoning with objects and technologies, and the circulation of discipline-specific ideas in dynamic relationships with the authority and reputation of their authors.

6.5 Discussion

Hacıgüzeller underlines the 'scholarly constructed nature of the strong and historically stable relationship between GIS and positivism' (2012:246), and traces a history of critical thought which traverses human geographers' rejection of GIS in the 1990s as a 'Trojan horse that could overpower social-theoretical considerations in geography' (ibid, p249), through

to later, more nuanced accounts which show 'awareness of the complex relationship between technology and epistemology in general, and GIS and positivism in particular' (ibid). Amongst the earlier critiques, Taylor describes the conceptual move from Geographical *Knowledge* Systems of the 1960s to Geographical *Information* Systems, as a return of 'the very worst sort of positivism':

> What does it mean to retreat from knowledge to information? Knowledge is about ideas, about putting ideas together into integrated systems of thought we call disciplines. Information is about facts, about separating out a particular feature of a situation and recording it as an autonomous observation. Hence disciplines are defined by the knowledge they produce and not by facts: a 'geographical fact' that is not linked to geographical knowledge (e.g. 'Test matches are no longer played in Dacca') is merely vernacular ('trivial pursuit') geography. The positivists' revenge has been to retreat to information and leave their knowledge problems—and their opponents—stranded on a foreign shore. (1990:212)

Of particular interest in the later critiques is that of GIS as a manifestation of 'representationalist' thinking. Representationalism can be summarised as:

> the belief in the ontological distinction between representations and that which they purport to represent; in particular, that which is represented is held to be independent of all practices of representing. (Barad, 2007:46)

This notion of a bifurcated world, where there are 'representations, on the one hand, and ontologically separate entities awaiting representation, on the other' (ibid, p49) has received significant challenges from a range of theorists, yet is 'so deeply entrenched within Western culture that it has taken on a common-sense appeal' (ibid, p48) (see also post-processualist critiques, §2.2.4). There is an epistemological critique of GIS which views its representations as emblematic of this divided conception of reality: they 'just' represent, and have no agency. This is significant because:

> when representations are treated as purely representing, they are taken as a substitution for an independent reality, but when they are taken as performing, they are acknowledged as making a difference here and now, in various ways contributing to the production of realities. (Hacıgüzeller, 2012:253)

The fear is that GIS representations can give a misleading sense of an 'objective' reality, with their narrow focus on topography, vegetation and so on, and their apparent separation from the 'subjective' world of 'culture, the mind, meaning and the present' (ibid). This is reinforced by the distinction commonly made by GIS users between 'anthropogenic' and 'natural' phenomena (Agapiou et al., 2015, Elfadaly et al., 2020, RSDP observations) and the identification of archaeological features as 'anomalies' in natural landscapes (Menéndez Blanco et al., 2020, RSDP Observations). Table 3 describes just a few of the many relationships and influences which go to make up satellite imagery and GIS representations, highlighting the extent to which they are deeply woven into the fabric of the world they depict. In the papers I have discussed in this chapter, there is, at one level, consciousness of the performative nature of these technological representations. Details are given of compensation for cloud cover, the processes for constructing composite images or those for transforming imagery via software processing. These are useful for the reader, but they give the impression of being means of drawing back curtains which have partially obscured an otherwise privileged, ideal view, rather than a description of a window's active construction. Or as Garfinkel et al. put it, the process is like 'extracting an animal from the foliage' (1981:132). In the following example, data is 'extracted' from satellite imagery, as though removed whole and intact:

> Satellite imagery of medium and high spatial resolution such as Landsat TM/ETM + and QuickBird was acquired and pre-processed in order to extract the road network of the area, faults and land use/land cover. All these data were incorporated into the final landslide hazard model. Following, topographic characteristics such as relative relief, slope, aspect and surface hydrological information were extracted from the ASTER GDEM (30 m resolution) of the target area. (Agapiou et al., 2015:233).

Elfadaly et al. (2020:7) describe the 'extraction' of data from satellite imagery, but include details of the mathematical transformations involved, thereby implying the logical possibility of adopting other methods. The implication, nevertheless, is that the world (and, for archaeologists, the past) is a distinct entity which is separate from us and can be revealed—or 'extracted'—with the aid of representations in an epistemologically neutral space.

For Hacıgüzeller, concerns about GIS's potential to misrepresent are not meaningful as long as we remain 'outside' representational thinking and see representations as 'participating in knowledge creation' (op. cit., p253). However in Barad's understanding of representationalism's entrenched, 'common sense' nature, 'It takes a healthy skepticism toward Cartesian doubt to be able to begin to see an alternative' (op. cit., p49). In most of the papers discussed here there is little or no sign of such scepticism. This is a good example of the gap between archaeology's theoretical aspirations and its everyday practices. There appears to be little trace of the long-running historical debates about GIS discussed by Hacıgüzeller, or any fallout from them, in the way that these articles make use of such software systems. There are occasional hints in the articles at the possibility of using alternative methods or refining current ones, but no criticality of the limited perspective of the GIS-centred approach. On one hand, this gives the impression that the critical theoretical outputs of archaeology are just another set of specialisms in a highly multidisciplinary field—distinct channels of research running through a flat landscape along with many others, with no guarantee that they will ever feed into each other or otherwise run together. On the other hand it may be seen as an expression of the difficulty of incorporating reflexivity and criticality into research in a way which does not threaten the aura of objectivity or undermine the goal of 'moving knowledge forward'.

Daston and Galison (2010) describe the history of the idea of scientific objectivity and the changing conventions of visual imagery as evidence which accompany it. 18th century image atlases of specimens from nature featured etchings and mezzotints which were idealised exemplars, 'reasoned images' with anomalies smoothed out and 'imperfections' corrected (Daston and Galison, 2010:42). In the mid 19th century, automated means, including photography, were used to minimise intervention in image production, 'in hopes of achieving an image untainted by subjectivity' (ibid, p.43). This came at a high price however, as these images often included inconsistencies and artefacts, threatening to undermine the very purpose of scientific atlases, that is, 'to provide the working objects of a discipline' (ibid). These images were *individuals* rather than *types*. In response to the perceived limits of mechanical objectivity, 20th century scientists found two new paths to follow: one was a rejection of empirical images; the other a turn to 'trained judgement':

> trained experts sought to make use of a 'sophisticated' or 'trained' eye to put back together what a radical nominalism risked tearing apart. (Ibid, p.317)

Daston and Galison's theorisation of scientific imagery and its uses is closely tied to the idea of the development of an accompanying 'scientific self': indeed, 'Making a scientific image is part of making a scientific self' (ibid, p363). It is only through the bringing of 'trained judgement' that the scientist is able to discern meaningful patterns in mechanically or digitally recovered imagery, for example. The authors note that while the reliance on 'trained judgement' represents an evolution of the notion of the scientific self, and makes possible new practices regarding scientific imagery, this does not amount to a paradigmatic or epistemic 'rupture'. The 'old' practices of 'truth-to-nature' and mechanical objectivity are still commonly found. In the remote sensing papers discussed here, there is a strong dependence on automated image making; in fact this is their *sine qua none*, and also stamps the guarantee at the heart of their knowledge claims: that their findings are trustworthy, insightful and worthwhile. This cannot be seen as simple mechanical objectivity, however, due to the self-awareness regarding the complex processing and translation of the images used. 'Trained judgement' is brought to bear on the base images, in order to make them 'see-able' and readable in the 'correct' way, but interestingly, this application of expertise can be sub-contracted—to software functions, and to pipelines of processing which have clearly been pre-established by other authors or collectively by the academic community.[32] Recall Elfadaly et al.'s technical incantation: 'The layer stacking, dark subtract, geometric correction, unsupervised classification, supervised classification, and post supervised classifications techniques are carried out while using ARCGIS 10.4.1, SNAP 6.0, and ENVI 5.1 software' (2020:5). What is noteworthy is that language used around these processes is often still the language of the programme of mechanical, or in this case mechanical and digital, objectivity. When these 'expert' adjustments can be outsourced to camera manufacturers, computers or simply to processes which are accepted as conventional, it seems more possible to avoid discussing them in terms of their active effects. The net result is a kind of 'soft' positivism. Some of the articles discussed here seem, at face value, to

[32] This demonstrates the difficulty in differentiating between the acts of image capture on one hand, and image processing which is contingent on an act of interpretation, on the other. Digital cameras apply 'corrections' 'in the box' to images to compensate for asymmetries in lens shapes, for example.

embody the positivist approach of which the human geographers were so wary. What 'softens' their epistemological outlook is their distance from an approach which straightforwardly identifies the representation with the object of study, as Riley does here:

> The advantage of a photograph… seems to be self-evident. The photograph is the actual section. There is no artist's interpretation in the reproduction of the structures. (Riley quoted in Daston and Galison, 2010:319)

The remote sensing article authors actively intervene in the preparation of their images. Their interventions depend heavily on *outsourced* 'trained judgements', in the forms of software programme functions and black-boxed transformation techniques. In Latour's terms, these are taken to be 'intermediaries' rather than 'mediators' (Latour, 2005:37), and can thus recede from the foreground of consideration. In GIS-based research, this helps to build the platform, and clear the stage, for the ensuing discussions about the computable, 'datafied' aspects of the imagery or other data sources. In some cases these involve the explicit application of *human* 'trained judgement', as when landscape features are picked out, and hand-drawn vector graphics are created over satellite images or maps. In others they involve computational operations based on datasets comprised of, for example, indices of vegetation (NDVIs), or Digital Elevation Models (DEMs)[33]. Overall, the authors are able to use language which emphasises objectivity and underplays the subjective judgements they bring to bear on the evidence in order to produce the 'working objects' of their discipline.

Daston and Galison's concept of the cultivation of a 'scientific self' is a useful perspective from which to make sense of the paradox of the highly interventionist processes the researchers use and their unwillingness to frame these in language other than that which indicates objectivity. We might understand this analytical mode to be a characteristic feature of what it is to *be* a scientist, or an archaeologist using scientific methods. To introduce self-reflexivity about, for example the ontological limits of GIS, may simply be *incompatible* with this sense of selfhood. Garfinkel et al. describe the 'embodiedly situated

[33] The use of acronyms seems to be a reliable indicator of an established black-box procedure, which can be summed up by a symbolic label.

practices' of radio astronomers collecting and analysing sensor data, in processes of mutual witnessing of the type described by Shapin and Schaffer:

> These practices are occasioned; they are 'hidden' in and as their apt and familiar efficacy; they are only available to practitioners; and only to their vulgar competence, they are done unwittingly; they are developingly objective and 'account-able', *i.e.,* observable-and-discourse-able; they are unavailable to reasoned reflection, to introspection, to ethnographic reportage, to the analysis of ethnographic documentation, or to documented argument except, and at best, as documented conjectures; they are done in detail; they are real-worldly, and they consist of all that detail can be in technical, material contents; they are only discoverable and cannot be imagined; *and they are naturally accountable*. (Garfinkel et al., 1981:140)

What is deemed naturally accountable does not require analysis—that would be unnatural and counter intuitive. This raises the question of how working practices take on this status, and so develop resistance to introspection—and whether the 'scientific self' which these practices constitute can be remade in light of different ideals. One answer might lie in the rejection of a representationalist perspective on data collection and analysis. For Barad:

> a performative understanding of scientific practices […] takes account of the fact that knowing does not come from standing at a distance and representing but rather from a *direct material engagement with the world*. (2007:49)

The figurative concept of representing 'from a distance' is made concrete in a system which is designed around 'God's-eye view' maps and space satellite imagery. A challenge to this way of thinking would involve somehow reducing this distance, bringing additional context to its perspective, or otherwise problematising its hegemonical status. In terms of the practicalities of GIS use, Hacıgüzeller suggests the incorporation an awareness of the 'spatio-historical context' of its development and use as a technique, and proposes the enactment of 'fruitful relations between other forms of archaeological knowledge and those created through the technology' (Hacıgüzeller, 2012:257). Interestingly, the most nuanced papers of the 6 discussed, in terms of knowledge claims and understandings of facticity, are those which use either a hybrid approach (Jia et al.'s use of ethnography alongside remote sensing) or ground the use of GIS in a detailed account which provides, at least in part, the

context of a ‘direct material engagement with the world’ (Rayne et al., who discuss the historical and cultural background to their project and the strengths and weakness of specific applications of their methods). In both cases the effect is something akin to introducing a measuring rod into a photograph to give a sense of scale. The juxtaposition of research methods and the recontextualisation of the technological solutions help to show that meaning is actively produced rather than revealed. In the non-representationalist approach, the ‘scientific self’ evolves into a subject who can exercise ‘trained judgement’ not just in relation to technical data, but in relation to the application of a range of research methods, and of different ways of seeing and sensing.

* * *

The papers discussed are all case-studies, and share the theme of remote sensing. All seek to leverage the capabilities of GIS and the affordances which access to remote sensing technology provides, effectively fencing off a space in which research work can make progress without interference from ‘troublesome’ knowledge (see §7.5). A ‘techno-optimist’ perspective and technical focus on GIS and RS is central to the academic identity of the papers, their authors, and the thought communities they operate in, as exemplified by the journals they have been submitted to for publication. Those commonalities aside, they have quite varied aspirations. Some of the papers have the goal of developing a methodology for comparing temporally separate datasets related to sites of cultural significance, with a view to detecting change and mitigating risks to the sites’ conservation. This task, complex as it is, might be considered straightforward in comparison with the goal of developing new understandings of the past based on highly fragmentary evidence, as tackled in other papers. Some of the articles have multiple objectives, encompassing archaeological identification *and* interpretation, for example. Each paper needs to be considered on its own terms.

The papers demonstrate some mismatches in tone and certainty between the body of the texts and their conclusions. Measured language, ‘radically simplified’ models and hypothetical scenarios give way to ‘strong’ correlations (Jia et al., 2020:21); the possibility of ‘predictive models’ is lauded where the use of such models was apparently inconclusive (Menéndez Blanco et al., 2020:36). Agapiou et al.’s conclusion that their research results

clarify ‘the need to take certain actions for the protection and preservation of the monuments’ (2015:238) seems to beg the question of the research. These apparent inconsistencies might be put down to the pressure to report conclusive findings, justify the time and money expended, and ‘move knowledge forwards’. They could also be read as expressions of an epistemological commitment to the methodology and methods used—a desire to validate the work of the community and to be recognised as a valuable contributor to it. Or, they may simply be attempts to capture the *formal* essence of the genre of writing that is the academic article, with its conventions relating to narrative shape and the framing of conclusions; though such an approach would indicate a shallow perspective on the relationship between research form and purpose.

Alongside the articles mentioned so far, it should be noted, is one with very considered conclusions, replete with detailed qualifications and reports of limitations (Rayne et al., 2020); and another which is extremely modest in its claims (Luo et al., 2017). I do not attempt here to make inferences about general practice in the field based on an analysis of this small sample of articles.

In general, the articles exhibit a lack of reflection on epistemological assumptions: the epistemological perspectives of the authors were almost always tacit and by implication assumed to be shared with readers. The only exception to this is a passage justifying the use of ethnographical methods (Jia et al., 2020). The absence of similar discussions is most noticeable in those papers which have to deal with highly tenuous evidence in their attempts to make interpretations of long-past events: their ‘methodology and methods’ sections deal only with the technical aspects of remote sensing and GIS use. Instead of considerations of what can be known, there is an assumption of a shared, self-evident epistemological framework. The materiality and agency of the representations used goes unexamined, even in the cases where they clearly represent a ‘hermeneutic relationship’, to use Sinclair’s term, with the sites they relate to, for example with LiDAR-generated computer graphics (Menéndez Blanco et al., 2020:33) or ‘pansharpened’ satellite imagery (Luo et al., 2017:84), both of which exhibit conspicuous amplifications and reductions. As Latour and Woolgar put it:

> The result of the construction of a fact is that it appears unconstructed by anyone; the result of rhetorical persuasion in the agnostic field is that participants are convinced that they have not been convinced. (2013:240)

The notion of 'natural accountability' cannot easily acknowledge or accommodate the situated and subjective aspects of knowledge making, in spite of the requirement to cultivate a 'scientific self', which exercises expert judgement. At the same time, the ANT perspective is useful for understanding how the high degree of abstraction in representations such as idealised LiDAR-generated 3D models, and the narrow, selective focus of GIS research, make findings derived from them more mobilizable to other contexts. These technological representations present opportunities to harden interpretations into facts, by the facilitating processes of translation and filtering as they mediate information. I have also argued that technologies are called upon as credible witnesses for the warranting of findings, by dint of their demonstrable 'effectiveness', their tangible capabilities in mastering or ordering natural elements. Even where the conclusions derived from technological means are somewhat ambiguous, as in paper 3, the presence of such 'co-authors' is used to lend weight to the methodologies and methods which are at the heart of these papers and their claims.

Lucas's dichotomy of 'engagement' and 'detachment' is a useful one to bring to bear on the understanding of the production of knowledge in these articles. The engagement of narration and witnessing is in tension with the detachment of the technological collectors and translators, the translated data itself, and the apparent objectivity of the impersonal 'God's-eye view' observations. This tension can also be seen in the simultaneous and paradoxical minimisation of authorship (in order to produce facts which can transcend the local and particular) and maximisation of authorship (in order to produce a credible and authoritative 'knower', and in a few cases, credible localised knowledge). In the case of those papers which take something close to a positivist position, such as paper 1, the message is that anyone could and would produce the same results, if they executed the same method correctly: but that not everyone is to be trusted to do this. We are to have faith that the authors are virtuous enough to have developed and applied the correct methods while simultaneously removing any individual bias in their execution. Their

narrative account is definitive, the data 'speaks for itself' and authorial subjectivities are effaced. Contrastingly, the narrative in paper 6 emphasises the detail of local findings, highlights the active voices of the authors and points out their fallibilities, while still making claims for the potential universality of its methods. This, then, would seem to be the main challenge of transparency in research: to find the correct balance in the tension between engagement and detachment; and to in doing so to try to actively recognise the full range of actors which contribute to the work of making facts mobile between the local and the universal: the accumulated bodies of consensual knowledge, such as archaeological typologies; the text styles; and the technical data collections and representations which facilitate translation and reification as part of the production of knowledge.

7. An Epistemological Programme

7.1 Introduction

I started this thesis with the assumption, based in part on the literature, and in part on personal experience, that there is a mismatch between some of the theoretical aspirations of archaeological research, and the standard forms for documenting and sharing research in that field. I will now review my original proposal in the context of the findings I have shared in studies 1 and 2. Firstly, I will explore any commonalities or contradictions in the conclusions I have drawn from the different studies. I will then consider the implications of these in relation to the originally stated goals of increasing transparency and reflexivity in cultural heritage research. I will build on these findings and relevant themes from the literature to set out the case for the conscious adoption, in interpretative research, of a suitable epistemological programme which can help guide practice in striving towards those goals. I will then go on to consider the implications for practical research methods.

7.2 Commonalities and discontinuities in studies 1 and 2

The varied perspectives gained from the interviews, observations and article analyses were useful in illuminating different aspects of some common themes.

The strongest and perhaps least surprising common thread was the influence of the institutional patronage on research projects, and the extent to which projects are shaped by their institutional functions. This was visible to an extent in the conventions of archaeological articles, in the shape of author credits and acknowledgements; but only in the 'behind-the-scenes' views of observations and interviews were the practicalities related to budgets, time, research points, IT support and so on, properly available.

A strong theme throughout was the techno-optimism which pervades much, though not all, of the thinking and practice of archaeological work. I took this to be an indication that technological innovation is perceived to hold the promise of opportunities for novel

research approaches in archaeology, greater efficiencies, or even 'breakthroughs', and to facilitate the need for academic disciplines to continually reinvent themselves. Accompanying this focus was a data-centric approach, one in which the primary concerns were the computability of evidence and its compatibility with digital storage and retrieval technologies.

What emerged from the observations, and to some extent in the academic articles, was that a substantial part of the knowledge-making work discussed was either tacit or 'unofficial' and therefore under-represented in project outcomes. The tacit aspects of the work were linked to the parallel projects which accompanied the 'official' one: the construction and maintenance of authoritative 'knowers' and their thought communities. It was in these parallel spaces that the nature of the commitments to epistemological and ontological standpoints was shaped and refined. The separation of this work allowed the 'official' narratives to be uncomplicated by uncertainty or introspection, and for representational forms to appear 'natural'.

In the RSDP, the negotiations tended to be about how to make interpretations of evidence, and how to translate those interpretations into documentation, and in particular, database records. These translations were at the heart of the production of knowledge claims, and in both the observations and the academic articles, such transitions were used to harden interpretations into facts.

The *discontinuities* in the studies emerged mainly in the cracks which opened up between the descriptions of goals and ideals given in interviews, and the everyday practice of research work, as seen in observations and academic articles. The focus on the visions which informed and justified final outputs tended to obscure the extensive negotiations, messy processes, and sometimes highly subjective interpretative decisions which had been a part of their construction. Some of the research values which interviewees recognised as positive, such as the reuse of results, iterative collective development of projects, or considerations of uncertainty, were in practice found to take place only in limited ways.

7.3 The need for an epistemological programme

If the goals of research are to provide transparency, to allow results to be reused, and to be open to different voices and perspectives, then the findings from these studies are problematic. Difficulties for these aims arise in the gaps between the way research is conducted and the way it is presented.

Consider for example the issue of the reuse of research results. Evidence from the interviews suggests that reuse of archaeological research is limited, except in those cases where researchers are focused on quantitative data, often in the context of scientific specialisms in archaeology. It also came across clearly that difficulties had been encountered in fulfilling the ideal of creating digital archaeological archives which were comprehensive and searchable, thus allowing legacy research projects to be fully exploited for evidence retrieval, comparisons, pattern finding, and so on. An important part of these difficulties was related to practical issues such as storage, maintenance costs and challenges to the sustainability of technology initiatives; but there was also a more fundamental problem, which was the lack of commensurability between projects.

This is more understandable if we consider research in terms of its epistemological and ontological commitments. Reuse is most plausible where these commitments are well understood, and in particular where analysis can be reduced to black-box procedures. If I am reusing research results which incorporate DNA analysis from a certain laboratory, for example, I may be able to use the same laboratory to analyse my own material. Material handled or analysed with established scientific procedures is among the most likely to be reusable, precisely because there is a strongly shared understanding of the 'reasoning style' adopted by the researchers who work with it; and there appear to be, at face value at least, few unaccounted-for externalities in the production and presentation of the work. This is highly contested (e.g. Latour and Woolgar, 2013), with good reason, but it is the assumption of a shared commitment which is significant in the context. In the same way, materials which have not been directly subject to anthropogenic influence are particularly suitable subjects for research reuse, simply because they fit most straightforwardly with a positivist perspective of the world: data is 'out there' in the world, separate from us, and we can collect it and work with it in a neutral mode.

The picture is quite different in projects which more obviously involve interpretative aspects and have negotiated outcomes. The archaeologists interviewed in study 1 tended to make sense of the general lack of reuse of archaeological research results in terms of either a lack of time, or because of mismatches between documentary records. One pointed out that setting up a network to make international records comparable had turned out to be a largely useless initiative because the formats of the records were so different from country to country. Another discussed insuperable obstacles in mapping a range of project records semantically to the same schema of linked-data relations, because the projects were so diverse, represented specialist interests, and were simply 'not created with such a purpose in mind'. These examples are telling, because they reveal the lack of a common ontological base for knowledge-making work. I suggest that researchers refrain from reusing the results of others based on an intuitive understanding of this—an awareness that knowledge claims are derived from situated perspectives and assumptions which are not fully available to readers of project outcomes. They are, in effect, based on models of distinctively different realities. To try to make use of the outcomes of such processes, proceeding on the basis that they are universal, would be unreasonable, just as it would be unreasonable to attempt to incorporate another researcher's journal entries into your own. Even so, in spite of the interpretative nature of much archaeological work, and the apparent gaps between ideals and practice, the narrative of reusable project outcomes, stored in a common format, has been able to persist.

It might be argued that the complexities related to reuse and standardisation in archaeology are a direct consequence of its multidisciplinary nature. There have long been attempts in the discipline to reconcile the influences of the 'two cultures'. Indeed, it is archaeology's relationships to the 'scientific method' and interpretation which make it so interesting for a study of epistemology in research. Knapp goes so far as to make a clear distinction between 'archaeological' and 'scientific' perspectives on interpretation, identifying a 'fault-line' between them (Knapp, 2002:38). He asserts that archaeologists have 'created multiple pasts, reflecting the questions we ask or the answers we expect or wish to see' (ibid), and that the interpretation of human behaviour does not follow directly from scientific analyses. Knapp's understanding of the scientific perspective is of a positivist one which 'cracks the code' of a singular reality: for him, scientists outside of archaeology misunderstand the nature of the discipline because they 'have a different trope in mind, the trope of a 'crucial

element' (ibid, p42). Scientific analysis can be used to drive and inform archaeological research questions, but it is for archaeologists to decide whether it is producing 'worthwhile results', based on their scholarly 'ends' (ibid). The instrumental knowledge of science is deemed too narrow to address this broader interpretative context. In one sense this is strongly reminiscent of Daston and Galison's evolved objective 'scientific self', and the idea of using trained judgement to bring meaning to mechanically recovered evidence, but it goes further than this—it is an explicit acknowledgement of a kind that is not often found in natural science disciplines—that knowledge construction is, fundamentally, a situated practice.

Others have gone further than Knapp, lamenting the 'scientific illiteracy' of archaeologists and dismissing their tendency to want to 'analyse things' as an indicator of a 'naïve empiricism':

> For archaeological science is not a science as it would be recognised in the natural sciences. Rather, it is an assemblage of techniques lacking a coherent epistemology adequate to the tasks to which it might be expected to aspire. (Pollard quoting Dunnell, 1995:243)

The critique from Knapp and Pollard is that there is a lack of awareness of the importance of context for epistemological validity, and a naïve assumption that data on its own might constitute a sufficient basis for knowledge claims. The position is similar to that summed up here by Maxwell, in an outline of the CR perspective:

> a key property of evidence (as opposed to data) is that it does not exist in isolation, but only in relation to some claim (theory, hypothesis, interpretation, etc.). Evidence is thus in the same position as validity—it can't be assessed in context-independent ways, but only in relation to the particular question and purpose to which it is applied. (2012:145)

It would be unjustified to call the participants in studies 1 and 2 'naïve empiricists', but certainly there were examples in study 2 where the techno-optimist approach manifested itself in research which was heavily data-centric, and the broader context of the research was left unexamined. This was particularly noticeable in the cases of cultural heritage management projects, where there was no requirement to create a research question

related to an interpretation of the past. The mapping workshops in study 1 were closer to Knapp's ideal of archaeology, in particular the development of those 'case studies' which went beyond the identification of sites, and risks to their conservation, and included interpretative aspects which invoked a particular 'past' in order to ask questions related to it. In such cases, the 'objective' archaeologists exercised 'trained judgement' when considering the relevance of scientifically derived knowledge.

The relevance for commensurability between projects, and the techno-optimist ideal of unified repositories, where archive documentation from diverse projects is conformed to a common schema, is clear. A data-first approach to organising research fails to recognise that evidence cannot be assessed in a 'context-independent' way, as Maxwell puts it. It also fails to account for the interventions of experts who exercise their judgement in the management of data resources. These facts, I would argue, form the basis of the intuition which discourages archaeologists from reusing the outputs of previous projects. (I distinguish *reuse* here from *referencing*, which has distinctively different commitments.)

I suggest that positive assessments of published research take place not in the context of a universal ontology, but in the context of plausible, internally coherent and relatable accounts, in which the reader can comfortably find a position for themselves in their role as a 'scientific' (or academic) 'self'. This positivity and relatability does not necessarily equate to universality or reusability. There is a tacit understanding on the part of researchers that each project has its own epistemological and ontological commitments. In the interviews this found expression in discussions of trust, methods, institutional alignments, and so on; in the articles, black box procedures for collecting and processing RS data.
Knowledge making, or 'construction', is often understood as a process of building an edifice from the ground up, but the character of the research discussed here in observations and article analyses could be more usefully understood as a process of filtering data and experience, to reduce them to conceptually and physically manageable scales—a 'conquest of abundance' (Feyerabend, 1999). I suggest that it is this process of filtering which makes otherwise intractable evidence manageable and orderable, and thereby capable of producing meaning. Filtering is an appropriate conceptual means for understanding epistemology because knowledge 'construction' can, paradoxically, be understood as a

negative process, a process of exclusion. As Daston and Galison put it, 'epistemology is less about trailblazing than about path clearing':

> Epistemology seeks first and foremost to identify and remove sources of error, rather than to define the nature of truth. Errors notoriously proliferate; so do the strategies for blocking them. That epistemic virtues should be multiple and historical is the unsurprising consequence of the largely negative mission of epistemology: they were called into being to counter equally multiple and historical epistemic vices. (Daston and Galison, 2010:377)

Sometimes, in the face of overwhelming complexity, the best we can do is to rule out the most unreasonable and historically disproven scenarios for generating understanding. This impulse, and the fear of error, are significant co-authors in academic writing and in the negotiations which happen behind the scenes in projects like the RSDP.

The notion of knowledge production as a filtering process is also conceptually helpful for understanding how barriers to the reuse of work are introduced: subtractive procedures always 'start from somewhere': they have different starting points in the context of real-world research, so that what functions usefully as a filter in one case may simply not be applicable in another. This is exemplified by the archivist's account in study 1, of the attempt to conform specialist archaeology projects into the same relational schema. The projects 'were simply not created with such a purpose in mind'—they started from different views of the world, and the filters they imposed on those views chiselled out purposive ontologies which were ultimately incommensurable. To impose a relational documentation structure which was sufficiently broad to encompass both projects would have been meaningless, as it would not have accounted for the significance of the specialised filtering processes which made those projects distinctive.

In reality, the same problem of incommensurability exists for those conceptualising knowledge making as a brick-by-brick or jigsaw-puzzle-solving process. While it might be convenient to imagine different research processes as starting from the same place—'from nothing', as it were—their starting points can never truly be identical. Knowledge making is always a process which is already in motion, always arriving from somewhere else.

The rooted but restless nature of knowledge and the fundamental epistemological and ontological differences between projects are intuitively apparent to researchers and it is for this reason, arguably, that referencing is standard practice in academia, while reuse is not.

The RSDP can be taken as a useful example. The appropriate sets of evidence and models to use for analysis were at times unclear to participant researchers producing mappings. The data model used for recording sites evolved over the course of the project. At times it was acknowledged that different interpretations of the same data would be equally valid. Yet the final documentation was to be presented as a direct window onto the landscape being mapped, in a format which captured and shared only a fraction of this complex work, and few hints of its nuances, and appeared hardly mediated. There is a good prospect that the outputs of this project will be reused, because it is, among other things, a gazetteer, a sharing of 'dots on a map', although with much more sophistication than that phrase implies. It includes highly interpretative, hand-drawn outlines of archaeological features, and estimates of condition and risk. But these elements have been hardened into facts or as-good-as-facts through the selective presentation of the research process in a translated format. The filtering out of the discussions and negotiations behind their production significantly reduces the transparency of the project, but ironically, it is this level of detachment, this apparent factuality, which helps makes it a plausible candidate for reuse. Its 'naturalness' clears the path for readers to bring their own research assumptions to it: there is no epistemological or ontological gulf to be bridged. We therefore find two research ideals, transparency and reusability, in apparent conflict with one another.

Where claims go beyond putting 'dots on a map', where the dependencies and contingencies of knowledge claims start to multiply and interpretative aspects become more overt, the prospects of reuse become less likely, due to the awareness of the situated nature of research. Again, we might understand the suitability for reuse in terms of the reader's understanding of the original research and whether they, the reader, feel that they can proceed from the same *starting point*, an intuitive way of expressing whether they have the same understanding of what is knowable, and the same experience of being in the world. Some—perhaps most—research outputs will pay no heed to setting out their own starting point and will present their perspectives on knowing and being as 'natural',

inevitable or ‘common sensical’. This may be helpful for mobilizing knowledge claims, but is a hindrance to considered analyses of their findings.

The discussion so far has demonstrated that the goals of transparency and reflexivity are poorly served by research approaches which skirt over their own epistemological assumptions, or have a highly exclusive approach to epistemological validity. Of the 6 articles which I discussed in study 3, only one engaged with the question of the epistemological validity of its research methodology. I do not suggest that this is surprising—on the contrary it is to be expected where the norm is to avoid or suppress what I will call ‘troublesome knowledge’: that is, issues which cannot be easily accommodated in the current epistemological framework, or perspectives which threaten a ‘relatively stable, determinate, and therefore knowable and predictable’ metaphysics (Law, 2004:144). The failure to engage with the possibility of diverse understandings is an implicit assertion that the current ‘system of knowledge’ is ‘applicable, and evident to the knower’ (Fleck, 1981:22). It might be argued that to make this assumption, within a journal article for example, is a convenient shorthand within a community of thought which has shared priorities: on the other hand, what remains unstated is rendered potentially ambiguous for members of the collective, and potentially opaque for outsiders, newcomers or those reading research retrospectively.

I conclude that for interpretative research to aspire to approach the ideals of transparency and reflexivity, and the possibility of reuse and reinterpretation, researchers should adopt a more overt epistemological programme, one which is clear about its own affordances and limitations.

7.4 Epistemological Modesty

I therefore propose a programme, called ‘Epistemological Modesty’ (EM). The features of the programme of EM which I will discuss are as follows:

- Knowledge is situated
- Knowledge is made, not found
- The creation of knowledge claims also involves the creation of a credible and ethical 'knower'
- Knowledge does not 'start from nothing'—it is based on epistemological and ontological commitments
- Those epistemological and ontological commitments are a product of the genealogy of the discipline
- Agency is not the sole province of human actors
- The structures we impose on the world to make sense of it are created from the perspective of our embodied experience
- The structures we impose on the world to make sense of it are context-specific, and at times messy or inconsistent with each other
- Some aspects of our knowledge of the world are not susceptible to being reproduced in documentation
- Knowledge models should not be exclusive or claim privileged access to a redemptive truth
- Knowledge claims should be accountable to context

These are derived from themes I have identified in the literature related to archaeological epistemology, and related theories of knowledge more generally. They are characteristics of a requirement to live alongside the implications of 'troublesome knowledge', some of which I described in relation to the studies above. I will argue that the logical consequence of awareness and acknowledgement of the *limitations* on ways of knowing is the adoption of a more open, *pluralistic* research practice.

The examples I will highlight in the literature share commonalities in their goals for creating an epistemological approach—even where this is not explicitly stated—which I believe is conducive to the aims of research transparency, reflexivity, multi-vocality and, potentially, reinterpretation and reuse. In most cases the characteristics of this kind of practice are stated as part of another project: to produce a robust model of reasoning; to renew the

potential of GIS; to set out a feminist practice; to explicate a practice which emphasises the agency of things as well as people. I believe that it is useful to identify this epistemological programme and give it a name because this makes it possible, in turn, to set out its characteristics and consider how these might be manifested across a range of research practices.

The foundational aspect of EM is that it assumes that all knowledge is situated knowledge, and all meaning is meaning to a person. It assumes that knowledge always starts from somewhere; that context is vital and knowledge claims cannot start 'from nothing'. Context has significance for research claims in a multiplicity of ways. In esoteric research communities the appropriate deployments of specialist discourse and interpretative categories need to be learned by those seeking to acquire membership. The subsequent assumption of shared expertise means that the inherent ambiguities in language and understandings can be ignored, and some discipline-specific technical or historical details can be referred to without in-depth explanations. Such references still remain visible, but what is usually left out of sight are the assumptions about what *can* be known, and what the world is like, or consists of: the epistemological and ontological commitments of the research. There is often a tacit assumption that on these matters, the readers and the authors also 'start from the same place'. This is a problematic assumption, particularly for interpretative work which is working with incomplete evidence and must make 'leaps of faith' to formulate hypotheses, bringing in externalities which may not be fully accounted for. To make hypotheses about the past is doubly complex, because, depending on the authors' approach, this may involve tacit assumptions about the world as it *was* as well as the world as it *is*.

One important reason why the commitments behind research should not be taken for granted is that people are inconsistent in the ways they make sense of the world. This was in part demonstrated by the gaps which emerged between research ideals and practice, in studies 1 and 2 (see §7.2). A consequence of the situated nature of knowledge is that people produce meaning in relation to their sense of self, and their physiological experience of being in the world, and this is reflected in the language structures we use (Lakoff and Johnson, 1980). These structures and the sense we impose on the world, can be use-specific

and at times contradictory. The EM approach recognises that the categories we use to divide up the world are constructed, rather than revealed; they do not represent a 1:1 reflection of a world which is 'out there'. Over and above the 'messiness' of the way we experience the world, there are often more fundamental differences between the 'starting points' of readers, which should not be ignored—from cultural and linguistic differences which foster different understandings of our place in the world and our relationships to the past, to different levels of access to knowledge resources such as books and the web.

The lack of commensurability between project archives is easier to understand if we adopt this perspective. The filters which are used to produce and make sense of projects' datasets, to isolate the signal from the noise, are use-specific and mapped out in relation to a particular context—namely the priorities and historically accumulated consensual knowledge of an esoteric thought community. There is no guarantee of meaningful overlaps between them, or of mutual consistency. Our ontological understanding of the world may appear to be unified and complete, and we may assume that it is shared by others; in fact, aspects of it are amplified, reduced, prioritised or ignored according to circumstances. This presents significant challenges to the ideals of universal classification and data reuse (see §2.3.2, §2.3.3).

A natural consequence of these starting principles of EM is that the view of the past must be understood as seen through the eyes of the situated knower. A 'separate', 'objective' past would imply a state of things independent of knowers, a place which is somewhere whole, definitive and discoverable. In fact, as Knapp says, archaeologists create multiple pasts which reflect their particular interests (2002:38). These are projections from specific presents; and it is because the past 'lingers in and relates to our presence in various ways' (Hacıgüzeller, 2012:255) that we can make the past out of the things in the present. In this view, 'emergent causation takes over from that which was considered resultant' (Witmore, 2020:10). The implications of this understanding are profound for what researchers might believe they can know and share about the past; they imply a requirement, when seeking to make sense of traces of the past in the present, to also account for the traces of the *present* in the *past*.

Daston and Galison note that a prominent narrative of the Scientific Revolution is how:

> knower and knowledge came to be pried apart, so that, for example, the alchemist's failure to transmute base metals into gold could no longer be blamed on an impure soul. Key epistemological claims concerning the character of science […] depend on the schism between knower and knowledge. (2010:39)

They point out that a benefit of studying the history of scientific objectivity in terms of the history of the 'scientific self', and of reinstating the relationship between knower and knowledge, is that: 'Epistemic virtues would turn out to be literal, not just metaphorical, virtues' (ibid). In their account, objectivity has latterly been characterised by the researcher's application of 'trained judgement' to evidence and so the 'scientific self' must cultivate the appropriate virtues to exercise this duty competently. I interpret this to mean that in practice they must be a competent representative of a thought community and its collective expertise, shared tacit assumptions and sense of ethics. From the EM point of view, this reinforces the need to have a critical discursive relationship with the genealogy of a discipline, and to conduct 'source-side criticism', a reflexive consideration of the context of texts used (Richards-Rissetto, H. and Landau, K., 2019:122). Beyond this, it raises the broader question of what the nature of an epistemologically modest 'scientific self' might be. In Daston and Galison's view,

> As long as knowledge posits a knower, and the knower is seen as a potential help or hindrance to the acquisition of knowledge, the self of the knower will be at epistemological issue. The self, in turn, can be modified only with ethical warrant. (Op. cit., p40)

I have presented a certain type of modesty as an epistemic virtue, due to its positive associations with transparency, reflexivity and multi-vocality. I have also suggested that part of the project of research projects and articles is to construct authoritative knowers as well as knowledge claims. The work of Daston and Galison suggests that this should be understood in terms of *virtues* as well as authority.

The emphasis on knowers is a consequence of the framing of knowledge as always situated. From this perspective, knowledge cannot exist independently of a self or a community of knowers. The picture is complicated by the roles of authority and epistemic virtues such as objectivity, in making knowledge. A graphical representation of ideal knowledge might

consist of a triangle, with the knower on one side, the evidence on the other side, and the epistemological and ontological commitments forming the base (Figure 5). Building knowledge requires construction of the knower as well as construction of the claim: otherwise the triangle cannot stand because the claim cannot converge to a point of authoritative experience.

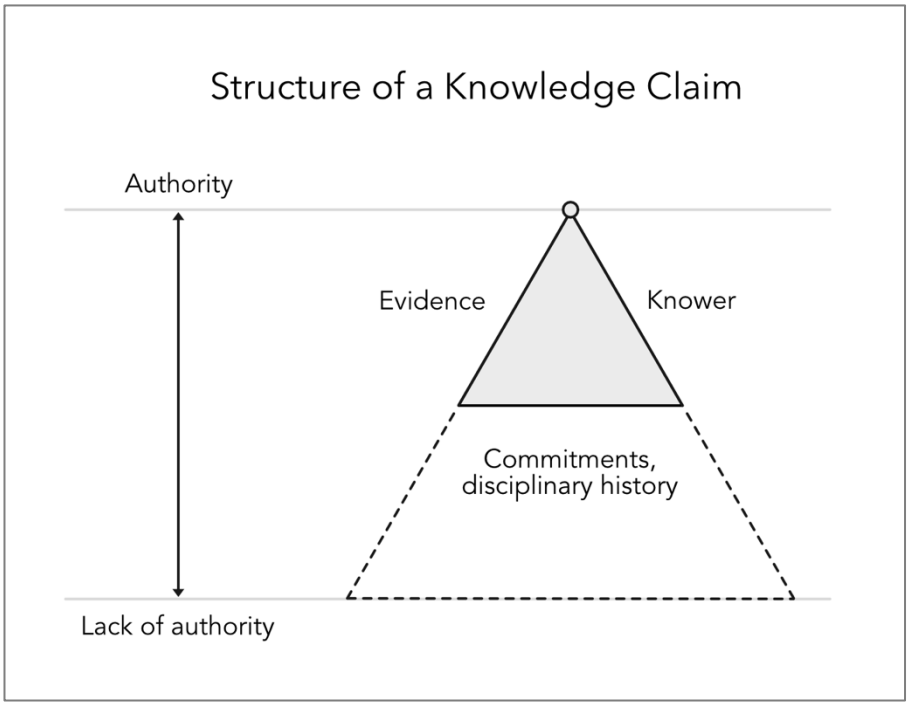


Figure 5. Tripartite graphic representation of a knowledge claim, with evidence, knower and epistemological and ontological commitments.

It might be suggested, however, that knowledge claims are often less than symmetrical—that the triangle is in some cases skewed. An expert's knowledge is more valued than a beginner's, for example, though all researchers can augment their authority by enrolling the authority of other witnesses. It is also possible to make knowledge, or consent for knowledge, based on minimal evidence, simply by multiplying the consent of witnesses by other means, for example by appealing to a convenient 'truth', or an entrenched idea, in the face of information to the contrary. (This is to be distinguished from propaganda made with false evidence.)

A modest research approach would develop a greater awareness of the relationship between the sides of the triangle, including the genealogy of the commitments which form the base. If it is accepted that in research we are building the knower as well as the claim, this can inform the effort to make research more transparent, more self-reflexive, or more robust. In the worst cases, academic endeavours become too weighted towards the creation of knower authority or the preservation of the knowledge community, and the result can be unethical behaviour, such as plagiarism, or the faking of evidence. A by-product of the *agonistic* nature of research (Latour and Woolgar, 2013:237) might be blocks on certain models of open collaboration. The desire to build expertise, or authority, either on an individual or collective basis, is likely to result in the competitive limiting of access to thought communities, or to works in progress. The status and authority of a thought community or a cultural institution like a museum (as discussed in interviews in study 1) is based at least in part on its exclusivity. The opening of the doors to the 'engine room' therefore typically only takes place in formalised circumstances, for example in one-off collaborative projects; or in the case of overwhelming external forces like a war effort, as Galison describes in his study of 20th century microphysics (1997).

Another important facet of the project of building an authoritative 'knower' is the active avoidance of error. In my view this 'negative epistemology' (Daston and Galison, 2010:377) is a core part of academic research and is one of the main reasons that the negotiated aspects of research projects remain undocumented. For the same reason there are strong impediments to the sharing of work in progress and to synchronous collaborations: aside from the concern of being pre-empted by other researchers, a safe space is required in which to test and refine proposals without 'looking stupid', and, which amounts to the same thing, to ensure that the 'knower' is operating within the bounds of the relevant thought community:

> The member's account, and its associated self-evident method, have great instinctive appeal; the social forces that protect and sustain them are powerful. The member who poses awkward questions about 'what everybody knows' in the shared culture runs a real risk of being dealt with as a troublemaker or an idiot. Indeed, there are few more reliable ways of being expelled from a culture than continuing seriously to query its taken-for-granted intellectual framework. (Shapin and Schaffer, 2011:6)

As a key part of operating successfully within the bounds of a research community is to act ethically, a negative epistemology can also be considered a guard against unethical behaviour.

The limitations on communicating tacit knowledge (see §7.5.2) constitute another strong justification for adopting a 'modest' epistemological approach. The 'social' and/or somatic sourcing of the production of knowledge, processes which in ANT terms might be described in terms of the relations brought about by the interactions between agencies, is another argument for understanding knowledge as situated and multivalent. EM has a symmetrical understanding of knowledge construction, which locates agency not just in humans, but in other species, tools, objects of various kinds, institutions, environments, and importantly, in the inertia of established intellectual traditions and consensual knowledge.

Finally, a knowledge which is situated, which cannot fully reproduce its own assumptions, and which makes use of representations with their own active agency in the production of meaning, cannot be exclusive. It cannot reject other possibilities outright or claim privileged access to a redemptive truth.

7.5 Troublesome knowledge and Epistemological Modesty

Having set out an overview of EM, I will now fill out some of the context of these ideas via a more detailed discussion of some key areas which have the potential to invite interactions with 'troublesome knowledge':

- Reflexivity
- Tacit knowledge
- Specialist knowledge and collaboration
- Categorising the world
- Uncertainty

7.5.1 Reflexivity

There are undoubted difficulties in sharing useful insights about one's own research assumptions when making knowledge claims. It can be very tricky to describe the campsite when you have no way of leaving the tent. In addition, the validity of self-reflective practice has arguably been undermined by entrenched associations between reflexivity and relativism, which see the recognition of uncertainty and the admission of the existence of alternative perspectives as unacceptable challenges to credibility. Lakoff and Johnson (1980) describe a cultural tendency to limit the available choices in knowledge making to a positivist understanding of the world on one hand, where there is an absolute truth about how the world is, independent of the person perceiving it, and which scientific objectivism is progressively revealing to us, in 'incremental, step-by-step terms' (Gribbin quoted in Erickson, 2010:78), and a relativist world view on the other, which leans heavily on the Romantic tradition, in which imagination triumphs over science and each individual makes their own reality, free of any constraints. (They reject this dichotomy and describe a third option, which I discuss below—see §7.5.4.) I suggest, based on my reports of its systematisations and preoccupations with techno-objectivism, that the mainstream of archaeological research, while not at the polar extremity of positivism, at least in terms of its tenets and aspirations, is nonetheless heavily preoccupied with the avoidance of relativism, and that this is strongly constitutive of its negative epistemology. On the issue of introspection in research, or the lack of it, Fleck, writing from his experience as a microbiologist, as well as a philosopher of knowledge, notes that:

> Whatever is known has always seemed systematic, proven, applicable, and evident to the knower. Every alien system of knowledge has likewise seemed contradictory, unproven, inapplicable, fanciful, or mystical. (1981:22)

He discusses the 'tenacity' of 'structurally complete and closed' systems of opinions, and the 'enduring resistance' that they offer to contradictory propositions:

> What we are faced with here is not so much simple passivity or mistrust of new ideas as an active approach which can be divided into several stages. (1) A contradiction to the system appears unthinkable. (2) What does not fit into the system remains unseen; (3) alternatively, if it is noticed, either it is kept secret, or (4) laborious

> efforts are made to explain an exception in terms that do not contradict the system. (5) Despite the legitimate claims of contradictory views, one tends to see, describe, or even illustrate those circumstances which corroborate current views and thereby give them substance. (Ibid, p27)

This system of entrenchment may sound wilful in Fleck's account, but the operative word is 'unthinkable'—it is the lack of a conscious epistemological programme which makes alternatives inaccessible. One might expect a more eclectic set of understandings in archaeology, which in the years since Fleck wrote these words has gone through extended phases of reflection and debate about its adoption of systems of knowledge (e.g., Trigger, 2006). While broader social science debates in the 80s had a particularly strong influence on the course of research in the discipline for a period (ibid, p446), the study 1 interviews suggested that the concerns which animated archaeology's 'crisis debates' (Chapman and Wylie, 2016:19) now appear to have receded from the foreground, except for those communities of archaeologists which have active interests in cultivating different ways of knowing.

It is worth taking note of Fleck's emphasis on 'systematic' knowledge. For some, archaeology is often not taken seriously (Shanks and McGuire, 2000:57), and has struggled at times to establish itself as a distinctive discipline, rather than just a borrower from other areas of academia (Lucas, 2015:15). An introspective archaeology which admits contradictions may be more resistant to the systematisation which is often taken as a mark of academic seriousness and authenticity of knowledge—and may make it more difficult to hold the spectre of relativism at bay. Certainly, Fleck's description of the resistance offered by incumbent knowledge systems to other perspectives is still a good fit for the types of archaeology discussed in studies 1, 2 and 3. The response it elicits from him is therefore highly relevant. By way of illustrating his understanding of the genesis of 'facts', he demonstrates that the invention of syphilis as a medical condition was a highly contested process:

> The development of the concept of syphilis as a specific disease is thus incomplete in principle, involved as it is in subsequent discoveries and new features of pathology, microbiology, and epidemiology. In the course of time, the character of the concept

> has changed from the mystical, through the empirical and generally pathogenetical, to the mainly etiological. (1981:19)

The conclusion that he takes from this is that the concept is 'unattainable except through a study of its history' (ibid, p21). The stark contrast between this conclusion and the observation that systematic knowledge nevertheless presents itself as self-evident, prompts him to ask:

> May not the time have come to assume a less egocentric, more general point of view and to speak of comparative epistemology? A rule of thought that allows one to make use of more details and more compulsory connections, as the history of science teaches us, deserves to be emphasized. (Ibid, p22)

His is a more vivid articulation of the notion that knowledge making never starts from nowhere; it is always arriving from another place. Such complex histories may resist systematisation, but they are vital to a proper understanding of knowledge claims and how we arrive at them.

7.5.2 Tacit knowledge

Closely related to the idea of making the *history* of knowledge more accessible are considerations of *tacit* knowledge and whether it can be shared. Collins describes tacit knowledge as 'knowledge that is not explicated' (2010:1), quoting Polanyi thus: 'We can know more that we can tell' (ibid). He distinguishes between different types of tacit knowledge: relational, somatic and collective. *Relational* tacit knowledge 'turns on *the way societies are organized*' (ibid, p86); this is knowledge which *is* explicable but remains tacit, due to choice, convention, or a failure to understand how to communicate it. Tacit *somatic* knowledge is that which is related to the limits and affordances of human and animal physiology. In archaeology it is of particular relevance to fieldwork, where there will be practices of skilled tool use which are learned 'on the job' and resist being described in terms of a list of instructions, like those for assembling flat-pack furniture. Similarly, the uses of 'reasoning tools' in archaeology for 'thinking outside the brain' might be resistant to description because of the unconscious aspects of their application, for example in human-computer interactions. Finally, *collective* tacit knowledge is at the centre of Collins' scheme,

'the irreducible heartland of the concept' (ibid, p119). This is knowledge which is located in the *collective* rather than the *individual*. In fact, Collins suggests that a special feature of human brains is that 'they afford parasitism in the matter of socially located knowledge' (ibid. p131). While being able to ride a bicycle—that is, being able to successfully balance on it while pedalling—is associated with *somatic* tacit knowledge, the ability to adapt to cycle riding in busy traffic depends on *collective* tacit knowledge, because a 'social sensibility' is required to make the appropriate adjustments to stay safe. For Collins, this collectively owned knowledge is, ironically, in spite of being the region 'we live in every day' (ibid, p158), the most difficult to access in terms of explication. He arrives at the conclusion that *all* collective tacit knowledge simply cannot be made explicit, because:

> We can describe the circumstances under which it is acquired, but we cannot describe or explain the mechanism nor build machines that can mimic it. (Ibid, p 138)

Based on Collins' insights, we should expect that tacit knowledge is always involved in the production of knowledge claims, precisely because 'we can know more than we can tell'. If some vital aspects of knowledge are not communicable in research documentation or the other artefacts which make up research outputs, the implications are profound for transparency, reusability and our overall understanding of research virtues and epistemic virtues specifically. There is an argument that *genre conventions* in academic reporting effectively limit access to the 'human side of the process' of producing scientific knowledge (Erickson, 2016:61), and have thus contributed to a crisis of reproducibility. Collins shows us that reproducibility must also deal with the inherent *limits* to sharing tacit knowledge. For example, we may make assessments that some aspects of knowledge are *relational tacit knowledge*, because they have been concealed (as when the master keeps secrets from the apprentice) or because they have been unrecognized as being significant. In such cases we could have reasonable expectations that these tacit aspects could be explicated and successfully communicated in the right circumstances (for example, by accessing the master's secret notebook; or via a research diary which communicates the conditions or habits in which the tacit elements of knowledge have been embedded). In other cases, with *somatic* or *collective* tacit knowledge, we might be able to communicate the conditions which have brought about the knowledge but be forced to accept that there are aspects

which cannot be fully communicated. The *full* context of a piece of knowledge can never be communicated, even if the gist of its contents is somehow articulable. By Collins' logic, collective knowledge simply cannot, in any case, be transformed into documentable form. This implies that a large part of research outcomes will always consist of tacit knowledge, which it is largely assumed the reader can access and retrieve with the same facility and consistency as the authors. This is the normal assumption of academics, who do not expect every paper to be an exhaustive recounting of 'life, the universe, and everything' in the lead up to a specialised research claim. Instead, they expect to be made aware of the *salient* background information. But the tacit aspects of research have often remained so because, as Fleck puts it, knowledge systems seem 'proven, applicable, and evident to the knower' (1981:22).

Fleck's 'history of a concept' may be reconsidered in this light: if such a history is the product, in part, of collective tacit knowledge, it may in important respects evade meaningful reproduction. Collins speaks of the 'social', and then in more materialist terms, of the 'collectivity of brains' as being the location of collective knowledge (op. cit., p132). This sets him apart from the ANT approach, which rejects the notion of the 'social' as a distinctive causal entity, seeing it rather as a symptom of the dynamic relations between actors (e.g., Latour, 2005). Collins, in a rather narrow reading of the 'symmetry' of ANT, emphasises the collective body which the affordances of human language make possible:

> In the case of both ANT and too enthusiastic a dissolution of the boundary between humans and animals, the mistake is the symmetry—it is humans alone who have this linguistic equivalent of DNA; we cannot understand the world without the asymmetry of Social Cartesianism. (Op. cit., 2010:169)[34]

[34] This might prompt the question: 'Who's world?' Is such an anthropocentric perspective adequate in a world in which the depth of human dependency on the environment and other species is made ever more starkly obvious? Or as Witmore more eloquently puts it:
'We need a different metaphysics to provide some purchase on our catastrophic times, when greater-than-gargantuan objects exceed the human ability to master them. Relations between permafrost and methane, between CO2 and sea-water, between atmosphere and sun require a very different empirical metaphysics than what we find in reductionist schemes that would situate all non-living objects as pawns in a game played exclusively by sentient beings.' (Witmore, 2020:11)

He does not claim that the 'collective body' is a universal one: indeed he gives many culturally specific examples of collective tacit knowledge. Even if we are unable to fully communicate the tacit aspects of knowledge, the choice to reduce our focus to a consideration of the mutual influences of the actors in that research community, including the non-human ones, seems to be the best available strategy for producing a space in which the nature of epistemological commitments can be considered. The 'untranslatable' nature of *all* collective tacit knowledge should not deflect us from explicating relational tacit knowledge where we *can*, and sharing reflections about our embodied and collectively derived experiences of knowledge construction.

7.5.3 Specialist knowledge and collaboration

But where do the boundaries of a research community lie? All of the archaeologists interviewed in study 1 agreed that they were highly dependent on the specialised knowledge of others. One pointed out the difficulty of assessing knowledge claims in unfamiliar specialisms. A consideration of archaeological practice as a multidisciplinary set of overlapping thought collectives can bring useful insights to the analysis of its epistemological and ontological commitments; both their nature and their compatibilities. Chapman and Wylie explore this in some detail, as part of an attempt to find a way towards a robust practice of reasoning from evidence in archaeology. As part of their enquiry they examine historical disputes about the uses of scientific techniques in archaeological practice. In both the cases of carbon-dating (^{14}C) and Lead Isotope Analysis (LIA), a technique for metallurgical analysis, an initial aspiration to find 'self-warranting' analytical techniques had to be moderated as the limited accuracy of the techniques when used in isolation became apparent. In the case of ^{14}C dating, parallel evidence had to be brought in to recalibrate atomic measures—evidence from sources such as dendrochronology and stratigraphic data, which the ^{14}C technique had been intended to displace. Chapman and Wylie celebrate the evolution of the ^{14}C technique in the face of initial difficulties in its application, seeing the relevant negotiations as a useful example of a successful 'trading zone', an interdisciplinary exchange.

The term 'trading zone' is taken from the work of Galison, who, in documenting the historical laboratory practices of microphysics, arrives at the conclusion that 'it is precisely the disunification of science that brings strength and stability' (1997:781). In his view,

breaks in theory do not necessarily lead to breaks in instrument use or experimental practice. Understandings and practices are recast and remade in light of new theoretical positions, and these happen at different rates in different specialisms, rather than synchronously, as in the 'paradigm' model (Kuhn, 2012). A consequence of this 'intercalated' model of overlapping theoretical and experimental phases is that it is possible to have zones of 'trade' between disciplines. Just as with economic trade zones between linguistically different communities, where a pidgin or creole language is spoken, it is argued that common ground can be found in interdisciplinary exchanges, even if there is no epistemological commensurability overall. The same terms can mean different things to different people, and this does not stand in the way of progress.

Chapman and Wylie apply Galison's insights to archaeological reasoning, in which they see the use of diverse bodies of evidence, or 'multiple determination', as a key activity. They draw on Longino's recommendations in developing a list of criteria for the success of such interdisciplinary work, including the following:

> (1) public venues for criticism which ensure that dissent can be voiced, and (2) norms of uptake for criticism; that (3) the standards that inform criticism are publicly recognized and themselves open to critical assessment, and that (4) practices of critical engagement should embody a norm of 'tempered equality of intellectual authority' (Longino, 1990:131)

By way of contrast with the iteratively negotiated nature of the application of ^{14}C dating, the de facto abandonment of LIA in archaeological research is seen by Chapman and Wylie in retrospect as a failure to construct a functioning trading zone. The disputes about LIA between specialists remained unresolved because there was a mutual misunderstanding of critical approaches, one of the key parties failed to openly publish all of their data, and in Longino's terms, there was no adequate shared space in which to develop a complementary understanding. In a successful example of 'multiple determination' however, such as the evolution of the ^{14}C dating technique, strength was able to emerge from diversity. We can understand this metaphorically in terms of Galison's 'intercalated' model of knowledge, in which he represents theories and experiments as overlapping bricks, offset for strength as in the wall of a house; or in the cable given strength by its many separate strands. Chapman and Wylie usefully invoke a parallel with Hacking's discussion of microscopy technologies

(Chapman and Wylie, 2016:159): he holds that what is revealed through different types of microscope is made more believable by the fact that different technologies (e.g. electron microscopy and fluorescent microscopy) agree in their observations (Hacking, 1983:201). The point is not just that triangulation is an epistemic virtue: it is, to use my own previous metaphor, that quite different filters, applied to the world in different ways in order to make knowledge, report the same results. This is taken to be an indicator of the robustness of a claim which can transcend a relativist understanding.

For Chapman and Wylie, the achievement of the development of the ^{14}C dating technique was not just a theoretical one but a practical one, which required 'sustained international effort to establish common standards and practices':

> The creation of this robust trading zone is a social and institutional as well as an empirical and theoretical accomplishment. (2016:156)

It is easy to imagine that 'many powerful forces work against the formation of a viable trading zone' (ibid, p183), and that the creation and sustenance of an eclectic research community is difficult. It may be that the outstanding promise of the carbon dating technique was what sustained related 'trading' over an extended period, and that it is therefore an exceptional example. In studies 1 and 2, where interviewees reported the significance of institutional patronage, and all the opportunities and limitations which that brings, it was clear that investments in interdisciplinary collaborations had to be made to fit alongside attempts to enrol other allies and interests to projects, not all of which had long-term trajectories.

For Knapp, the lesson to be derived from the bad-tempered debates around LIA is that for interdisciplinary work to be successful, greater understanding is required of how scientists and archaeologists *miscommunicate* with each other (2002:43). Arguably this can be stated more clearly and usefully as the need for an understanding of the respective *epistemological programmes* in disciplinary approaches, or at least a *respect* for alternative epistemological approaches. Galison's work is useful in this respect, as it shows that interdisciplinary work can proceed successfully without requiring fully-shared understandings between

specialisms. An important lesson to take from this is that knowledge models should not be exclusive and dismissive:

> data (…) are arguably open-ended, subject to multiple (social or analytical) interpretations, and require evaluation by close collaboration and interdisciplinary discourse rather than through contestation and defamation. (Ibid, p38)

Even in the absence of open hostility between specialisms, this conclusion is salutary for considerations of how knowledge is shared and stored. It helps to pave the way for alternatives to 'one-size fits all' solutions for describing data. In the same way, the insight from Wylie and Chapman, that scientific techniques showed their limitations when used *in isolation*, have relevance beyond the 'trading' required to foster productivity between specialisms. They help to demonstrate that a 'context-independent' approach to evidence interpretation has been a hindrance to 'trade' between disciplines—but we might extend this lesson to the case of the potential *reuse* of *project outputs*, which is another form of intellectual 'trade', albeit one which is likely to happen within a single discipline.

7.5.4 Categorising the world

In seeking to plot a course between the extremes of positivism and relativism, Lakoff and Johnson propose an understanding which they call 'experiential synthesis' (1980:192). Underlying their argument is the idea that all knowledge is related to individual experience, and 'meaning is always meaning *to* a person' (ibid, p228) (see §1.2).
In their account, based on linguistic analysis, the metaphors we use to make sense of complexity in the world are often mutually inconsistent, because it is rarely possible to find ones which cover all requirements:

> The use of many metaphors that are inconsistent with one another seems necessary for us if we are to comprehend the details of our daily existence. (Ibid, p221)

Metaphors can exclude and obscure at the same time as they include and enlighten:

> Each [metaphor] gives a certain comprehension of one aspect of the concept and hides others. To operate only in terms of a consistent set of metaphors is to hide many aspects of reality. (Ibid)

They argue that the same holds true for the construction of categories and sets, and that the set-theoretical concept of a category does not therefore necessarily map onto the sometimes messy ways people make sense of everyday things and experiences. This understanding undermines the practical possibility of putting faith in a stable and determinate metaphysics. It implies that efforts to construct comprehensive ontological models with logical internal consistency, as in the uses of linked data for research, may not result in accurate correspondences with lived 'reality'. There are obvious implications for academic research's preoccupation with the classification of its objects of study; but the 'masking' effect of categorisation is also visible in everyday research choices (e.g. see Footnote 31).

Lakoff and Johnson's model resonates with my experience of observing a research project in which negotiations were required to stabilise the epistemological and ontological bases for describing archaeological features. Individuals brought their own concerns and areas of expertise to the process of interpretation, and the 'masks' or 'filters' they used to build their concepts did not always neatly match up. This highlights the distinctive epistemological and ontological commitments which exist in both thought communities and individual projects, and the related difficulty of describing them in an archive using a standardised set of relations. The theory of experiential synthesis is an insightful expression of the concept of situated knowledge; but it also offers, like the pragmatic philosophy of Chapman and Wylie, signposts towards an epistemological programme which can avoid the double dead-ends of positivism and relativism. We can recognise the value of attempts to achieve rigorous consistency within a dataset in order to make entities comparable, while also acknowledging, when making models of the world, that not all of our categories will fit together congruently into a larger, complete picture without gaps, overlaps or contradictions.

7.5.5 Uncertainty

> Archaeology in essence then, is the discipline with the theory and practice for the recovery of unobservable hominid behaviour patterns from indirect traces in bad samples. (Clarke, 1973:17)

It has been suggested that archaeological evidence is 'inherently unstable' (Chapman and Wylie, 2016:19). Gaps in the evidential record and the unbridgeable temporal distance between researcher and the object of study force interpretation into a prominent role in archaeological scholarship, and raise the uncomfortable spectre of pervasive uncertainty. All of the areas of troublesome knowledge I have considered so far entail encounters with uncertainty of one kind or another, not least in their problematisation of definitive, determinate ways of knowing. I wish to differentiate here between two types of uncertainty typically found in the sphere of academic research. One is epistemological uncertainty, which is concerned with the limitations of a given knowledge framework, and of *what can be known*; the other, ontological, or 'model' uncertainty, as I will sometimes refer to it, relates to the integrity and sophistication of a model's structure and the completeness of its data. For Gero:

> a feminist practice aimed at more nuanced understandings of the past and open to more subtle, multivalenced notions of reality, must accept ambiguity as a central feature of archaeological interpretation (2007:311).

She makes a connection between the erasure of ambiguities in research and 'universalizing epistemic structures' which stem from shared 'rules and rule boundaries, values, priorities and specifications of social relations' (ibid, p313). Her problematisation of certitude, therefore, is an emancipatory project targeted at displacing prevalent epistemological assumptions and 'status quo rule-bound archaeological practice' (ibid); her proposal to *interrogate* the past rather than advance exhaustive conclusions about it requires new epistemological and ontological perspectives. What makes this approach remarkable is that unlike some other emancipatory programmes it does not aim to replace one authoritative system with another: the logical consequence of 'honoring ambiguity', as Gero puts it, is to

emphasise ‘complicating causation’ and the use of ‘multiple lines of evidence as constitutive of past realities’ (ibid, p312).

It is Gero’s critique of the handling of uncertainty within *model* contexts which leads her to reject a reductive epistemological approach, and associated ‘mechanisms of closure’ such as ‘cleaning the data’. In other areas of the literature the *nature* of the uncertainty under discussion can be unclear or remain unarticulated. I have discussed this in relation to the London Charter and Seville Principles (§2.3.3). The emphases on subjectivity and self-reflexivity in these documents give the impression of scholarship concerned with scrutinising its own epistemological assumptions and using them as measures for its own practices, just as Hodder (2000) did with his documentation of self-reflexive fieldwork techniques. However, while Hodder was to follow his principles through to the logical conclusion that a pluralistic interpretative approach was appropriate, and that:

> it becomes necessary to accept that our ‘mediation’ involves a particular perspective which has to be negotiated in relation to other perspectives (Hodder, 2000:43)

the London Charter and Seville principles ultimately function as calls for more completist but nevertheless *universal* knowledge models. What is problematised is the level of access to empirical evidence given to viewers of computer-based reconstructions, and transparency in general about the degree of data completeness in individual empirical models. They count, therefore, as new chapters in the manuals of scientific ‘objectivity’, speaking to the need for academic credibility, and connectedly, to the need to be able to distinguish ‘scientific’ or ‘academic’ visualisations from those produced for consumption by non-experts for non-academic reasons: but markedly less so as contributions to the effort to explore a range of epistemological approaches to research.

The devotion of attention to uncertainty has allowed archaeologists to refine their archaeological models. Bayliss and Whittle set out the advantages of applying Bayesian analysis to the construction of archaeological chronologies. Bayesian statistics is:

> fundamentally probabilistic and contextual. It simply means that we analyse the new data we have collected about a problem in the context of our existing experience and knowledge about that problem. This enables us to arrive at a new understanding

> of the problem that incorporates both our previously existing knowledge and our new data. (2016:217)

This affordance of assessing data in the context of existing data or 'prior beliefs' is not just useful for narrowing down possibilities: 'The fundamental point is that Bayesian statistics, being a formal methodology, force archaeologists to be explicit about their strands of reasoning' (ibid, p218). While the formal introduction of contextual information has the potential to be extremely valuable, the authors note the possible range of 'prior belief' types, and that some are more straightforward to assess and incorporate into Bayesian analyses than others. Stratigraphic sequences can be unequivocal or a matter of opinion, for example, depending on the case. In their opinion, 'the scale of belief is not readily quantifiable', and:

> We therefore do not currently incorporate degrees of 'prior belief' into our chronological models since we believe that the quantification of a qualitative belief is always on an arbitrary scale. (Ibid)

The method is therefore best suited to unequivocal quantitative data, though its outcomes may still require the application of expert judgement. While it addresses the issues of both ontological and epistemological uncertainty, its ultimate goal is to seek to create definitive models. The authors are highly reflexive about the goal of improving the scale of chronological resolution, noting that theoretical approaches may in the long run revert to broader chronological narratives, but state that in the meantime:

> we should do all we can, in the creation of chronologies, to maximise the detail of more precise histories, even if they unavoidably contain elements of both epistemological uncertainty and ontological indeterminacy. (Ibid, p239)

Beyond the cases of probabilistic analyses of uncertainty, I would argue that the cause of the exhausted impetus of initiatives to accommodate uncertainty in research outputs is the dissonance to be found not just between the aspirations of a universal epistemological/ontological model (which I would suggest is the spirit in which the London Charter and Seville Principles have generally been interpreted) and the inclusion of multiple

research *perspectives*, but even between a universal model and multiple plausible research *hypotheses*. These latter two, while philosophically compatible, pull in opposite directions. There is a potential problem, when admitting multiple hypotheses, of threatening the ideal of determinate knowledge required by a universal model; or of opening the door to the myriad other possibilities which the research has worked so hard to suppress in order to *stabilise* knowledge. Opening up interpretation to the full range of logical possibilities also has the potential to transform research into an unending column-sorting exercise, which does not fit well with a notion of distinctive scholarly authorship. Considered, evidence-based analysis is one marker of serious scholarship. However, the point at which evidence and statistics is transformed into *interpretation* is exactly where misalignments may emerge between set-theoretical concepts and experiential understandings of reality, and factoring in 'troublesome knowledge' has the uncomfortable potential to highlight such misalignments. The management of this process of translation can be identified with those distinctive aspects of scholarship which make it 'artful' and differentiate our interpretative outputs from those of other scholars and those of computing machines. It is for this reason, presumably, that the culmination of most archaeological research projects is a document in the form of a narrative, featuring characters, a problem to be overcome, and a journey of discovery, with a start, middle and end—rather than just a spreadsheet of data or statistical probabilities. Content formats which challenge the coherence and linearity of such narratives might also present a challenge to the development of a sense of academic and authorial identity.

From a perspective which sees the available epistemological positions in terms of Lakoff and Johnson's dichotomy of positivism and relativism, considerations of uncertainty are likely to be associated with the threat of relativism, and seen as representing additional work, in the shape of more extensive documentation, with few tangible benefits. What is required therefore, is a viable alternative to that dichotomy.

7.6 Beyond Relativism

A metaphysics which is 'relatively stable, determinate, and therefore knowable and predictable' (Law, 2004:144) is a poor fit for a self-aware approach to research which can account for the implications of troublesome knowledge. Presumably it is exactly for the purpose of avoiding trouble that such a metaphysics tends to be widely adopted. The challenge then, is to highlight the opportunities presented by reflecting on the limits of knowledge, while avoiding the perceived nihilism and ethical unaccountability of relativism.

Lakoff and Johnson's experiential synthesis is a convincing account of how we make sense of the world using metaphors which are strongly shaped by our embodied experiences. The closest thing we may have to a 'zero point' from which to start constructing knowledge is the shared physical experience of our engagements with the world. While there are profound differences in individuals' physical bodies and sensory capabilities, the embodied experience we have of 'being in the world' seems to be a reference point which we are able to use to start making shared understandings. Collins calls this the 'minimal embodiment thesis' (2010:135). This might help to explain the power of a teleological narrative of knowledge, where understanding is an ultimately solvable problem: we make sense of the world from what appears to be a fixed perspective, that of a unified 'self', a reference point against which changes happen and difficulties are overcome. This sense of unity persists in spite of the inconsistencies in the ways we divide the world up into manageable concepts. What those concepts have in common is the self which experiences them. A vision of a unified knowledge is symmetrical with the idea of a unified self which always experiences the world from what appears to be its centre.

Collins' perspective on collective tacit knowledge suggests that we extend this sense of unity to a community of knowers, as we use language or physical engagement to parasitically draw on the 'social' body of knowledge. It is not difficult to see, in that case, how we might have a cognitive tendency to assume that our outlook on the world is *the* outlook. This solipsism is a good fit for a positivist narrative of knowledge, because it separates out the self from the self's surroundings, while simultaneously putting it at the centre of the action. For the philosopher Richard Rorty, 'the desire for objectivity', to register human experience against 'natural', non-local relations is part of a fear of the *mortality* of our community, and

‘an attempt to avoid facing up to contingency, to escape from time and chance’ (2011:376). It may be that we have difficulties in accepting other perspectives when they come from *beyond* the embodied, personal, community-collective experience of knowledge, not just because we lack meaningful reference points for them, but because they represent challenges to a secure sense of unity.

Defenders of objective scientific observation might argue that our vantage point on reality can be extended beyond the senses of an individual. It can consist of many pairs of eyes, or of the outputs of instruments. There is some irony in Shapin and Schaffer’s description of Boyle’s efforts in the mid 1600s to characterise his scientific experimental work on air pressure, among other topics, as fundamentally *modest* in character. In order to enrol witnesses to his findings, he moved his sense of ‘self’ out of the foreground, minimising his personal standing and authority in order to come across as a disinterested man of good faith. His strategies for doing this included reporting on unsuccessful experiments; demonstrating modesty by reporting on individual experimental trials, rather than setting forth hypotheses of full systems; avoiding florid writing styles in his reports and publications; and above all by separating out observations from ‘facts’:

> There were to be appropriate moral postures, and appropriate modes of speech, for epistemological items on either side of the important boundary that separated matters of fact from the locutions used to account for them: theories, hypotheses, speculations, and the like. (Shapin and Shaffer, 2011:66)

In doing so, he was able to shore up the epistemological foundations of his empiricist programme:

> It was necessary to speak confidently of matters of fact because, as the foundations of proper philosophy, they required protection. And it was proper to speak confidently of matters of fact because they were not of one's own making: they were, in the empiricist language-game, discovered rather than invented. (Ibid, p67)

The irony of Boyle’s modesty comes with regards to his absolute resistance to epistemological perspectives which ‘denied the value of systematic and elaborate

experimentation' (ibid, p72), as in the case of that of his opponent Hobbes; and even more so in the fact that Boyle's strategies of modesty have become equated with scientific objectivity, while the tradition of experimental science associated with Boyle's practice has generally become less than modest in its claims to exclusivity in being able to access the truth.

Would it be possible then, to reclaim the notion of 'modesty' as a meaningful notion in research, from the ambitions of empirical science, and the 'neutral' witnessing of scientific experimentation?

For Rorty, it was in the seventeenth century, in Boyle's time, that philosophy as we know it today asserted itself. Intellectuals fixated on the metaphor of the mind as a mirror of nature, because, in Rorty's view, 'it offered the prospect of a new, more secular vision of redemptive truth, at a time when the power of religion was beginning to wane' (Tartaglia, 2020:93). Rorty's rejection of the quest to 'reveal' a redemptive truth led to him being subject to charges of being 'a relativist, or a subjectivist, or a linguistic idealist' (Levine, 2020:371). In response he set out the dichotomy of *solidarity* and *objectivity*:

> There are two principal ways in which reflective human beings try, by placing their lives in a larger context, to give sense to those lives. The first is by telling the story of their contribution to a community. […]
> The second way is to describe themselves as standing in immediate relation to a nonhuman reality. This relation is immediate in the sense that it does not derive from a relation between such a reality and their tribe, or their nation, or their imagined band of comrades. I shall say that stories of the former kind exemplify the desire for solidarity, and that stories of the latter kind exemplify the desire for objectivity. (Rorty, 2011, 367)

For Rorty, the common understandings of relativism, that 'every belief is as good as every other' or that 'true' is 'an equivocal term' with 'as many meanings as there are procedures of justification' (ibid), stem from the realist perspective which assumes that the idea of truth having an *intrinsic nature* cannot be seriously denied. They are not representative of the beliefs of pragmatism. Instead, pragmatists hold the view that truth is 'what is good for *us* to believe' (ibid, p368). Theirs is an *ethical* project, founded on a sense of community, and expressed through the concept of solidarity. It is necessary for the effective functioning of a

community 'that there should be consensus on a significant number of things, that "truth" can be distinguished from "falseness"' (Van Niekirk, 2020:397):

> So the pragmatist suggestion that we substitute a "merely" ethical foundation for our sense of community—or, better, that we think of our sense of community as having no foundation except shared hope and the trust created by such sharing—is put forward on practical grounds. It is not put forward as a corollary of a metaphysical claim that the objects in the world contain no intrinsically action-guiding properties, nor of an epistemological claim that we lack a faculty of moral sense, nor of a semantical claim that truth is reducible to justification. (Rorty, 2011:377)

From some perspectives, Rorty's pragmatism still has more in common with relativism than he might care to admit (Van Niekirk, 2020:397). Importantly for the EM approach however, he grounds pragmatism in the context of a community of knowers, and champions a practice which is ethical and accountable, both of which are vital for a research practice which can be seen to make progress and can support the development of 'academic selves'. For Rorty, the pragmatist can only be criticised for 'ethnocentrism', not relativism, and ethnocentrism, a focus on the 'people to whom one must justify one's beliefs' (ibid, p375), is inescapable. If this is a form of relativism, it is a practical one which can be worked with.

In archaeology, Chapman and Wylie also take a pragmatic philosophical approach to tackling the difficulty of finding a path between 'aggressive optimism' and 'epistemic despair' (2016:7), the equivalents of Rorty's objectivism and realist conception of relativism. Their strategy echoes the need to strive for accountability without the need for a redemptive, universal truth. They appeal to the notion of multiple determination and the cable metaphor of knowledge, to demonstrate that strength, and 'deductive security', emerge from diverse ways of knowing and appropriate critical engagements:

> we offer a reformulation of ideals of objectivity; we argue that the virtues of practice […] are best captured by a pragmatic and procedural conception of objectivity, along lines suggested by Helen Longino's account of norms of critical engagement that are instantiated in and required of well functioning scientific communities (2002). On this account, the goal of inquiry is not to produce knowledge claims that are true in all contexts of practice and transcendent of local interests, but rather to warrant

> knowledge claims as credible given available resources, and reliable for specific purposes. Objectivity is, then, characterized in terms of norms of practice that, together, secure the trustworthiness of specific knowledge claims as fit for purpose. In addition to the epistemic virtues we distil from analysis of archaeological best practice, these norms include requirements of rigour, integrity and transparency in the collective appraisal of knowledge claims that make them accountable to their contexts of production. (Chapman and Wylie, 2016:11)

Objectivity is reimagined: it becomes non-universal, more about accountability, credibility and trustworthiness in relation to an acute awareness of the contextual background against which its worth is assessed. Space is allowed for the emergence of new perspectives or new vocabularies in future. In Rorty's terms, inquiry becomes about 'the continual reweaving of a web of beliefs rather than […] the application of criteria to cases' (Rorty, 2011:371), and this can help to dispel the parochial associations with 'local cultural norms':

> For now to say that we must work by our own lights, that we must be ethnocentric, is merely to say that beliefs suggested by another culture must be tested by trying to weave them together with beliefs we already have. (Ibid.)

There are traces of what I would call a modest epistemology in other theoretical initiatives in archaeology, for example in Gero's advocacy for 'honoring ambiguity' and 'complicating causation' (2007:312) in research practice. The themes of openness to multiple determination and the rejection of a universal ontology are familiar; they should be interpreted as an invitation to pragmatic industry rather than as traits of a vague epistemology. Hacıgüzeller, in a critique of GIS research practice, makes a connection between post-positivism and the necessity of being open to the influence of multiple agencies in the production of knowledge:

> Specializing in 'archaeological GIS theory' (or other digital technologies) has never been more timely and relevant. In fact, we find ourselves in a social sciences environment where we (or at least some of us) are increasingly convinced that knowledge created through scientific practices is not the result of a detached process where human subjects 'crack the code of independent realities' (Haraway, 1991; Latour, 1993, 1999, 2005; Law, 2004, 2009; Pickering, 1995). We are provided with a new vocabulary to discuss the agency of technology in the process of creating scientific knowledge in a trend led by theoreticians such as Bruno Latour (1993,

> 1999, 2005) and Andrew Pickering (1995). From these non-representational perspectives, acquiring knowledge is seen as a process wherein human activity and passivity are mangled with other types of agencies (disciplinary, technological, etc.) and it is seen that there is much more to the world (and, hence, our research projects) than human intentions and conscious actions (Wegner, 2002). (Hacıgüzeller, 2012:247)

As touched on in §6.5, she proposes, in another example of multiple determination, though of a different type from Chapman and Wylie's with its emphasis on logical 'warrants' for reasoning (Chapman and Wylie, 2016:33), that a revised GIS practice which reflects these emphases might be one which adopts a hybrid approach:

> where we no longer feel that the birdsongs, for instance, have to stay apart from an archaeological narrative that involves cost-distance analysis. (Op. cit., p257)

Her advocacy for a *genealogical* approach to GIS practice, to show the links between current understandings and the historical paths taken by archaeological GIS practices 'across time and space' (ibid, p256) is an echo of Fleck's call for knowledge transparency via a study of disciplinary history (1981:21), and Sinclair's quest for an awareness of the 'building blocks' of academic genres (2000:481). This can be interpreted as another take on the pragmatist's need to be accountable to context: the 'community' is in large part a product of its history of thought and consensus–making.

The branch of archaeological theory called 'Symmetrical Achaeology' also looks to Latour's work on agency for inspiration in rebalancing the weight of scholarly attention away from humans alone:

> Rather than arbitrarily elevating humans among themselves to a privileged position in relation to nonhumans from the start, a symmetrical archaeology recomposes all beings as entities among other entities. The principle of symmetry does not imply that all entities exist equally—asymmetries are legion—rather, it humbly suggests that all entities equally be granted dignity as concrete and autonomous things at the outset of any analysis. (Witmore, 2020:2)

An important consequence of the symmetrical approach is the understanding of the past. Witmore states this in terms of an anti–'whig history' perspective, where:

> the past that was does not exist as an authentic and privileged bedrock situated in a resultant, causal relation to what is present; rather, the things present are what make the past, including processes, possible. Understanding how things give rise to pasts demands a different understanding of causation. Here, emergent causation takes over from that which was considered resultant. (Ibid, p10)

The past does not have an independent existence—it is not 'waiting to be unambiguously deciphered once and for all'—rather, 'it is something that lingers in and relates to our presence in various ways' (ibid). In my interpretation, this outlook is characteristic of a 'modest' epistemology—in acknowledging the situated basis of our understanding, it is capable of admitting multiple and nuanced possibilities; it is non-exclusive. For Witmore, Symmetrical Archaeology it is not a 'replacement for post-processual and contextual archaeology' (ibid), but a highly inclusive set of strategies for interpretation:

> Symmetrical archaeology eschews that critical gesture of dialectical upheaval where other practices, concepts, or questions are ousted to the realm of the outmoded (González- Ruibal 2007). It avoids that disciplinary amnesia associated with a paradigmatic image of progress. Rather than exaggerating differences with other subdisciplines, symmetrical archaeology excavates to what is common for archaeologists everywhere—things. (Ibid)

The interpretative work of symmetrical archaeology 'is always open to reassessment' and should remain 'open to surprise and uncertainty' (ibid, p2-3) because of the significance of *things* and what *they* might suggest. In other words, the practice is open to the implications of accounts from diverse sources, both human and non-human, just as the EM approach is characterised by openness to a range of epistemological and ontological commitments.

The Romantic poet Keats formulated the concept of 'negative capability', one which describes a propensity for living in the midst of mystery (Ou, 2009). The term refers to the *positive* potential of the *absence* of certain knowledge. This is the opposite side of the coin to Daston and Galison's understanding of epistemology as a mechanism for the removal of

error (2010:377). However, Romanticism brings its own historical associations. Lakoff and Johnson identify the Romanticism of Keats's period with relativism (Lakoff and Johnson, 1980:192). The impulse to encourage epistemological openness may be inhibited by traditional dichotomies which are deeply embedded in the history of disciplines, and still have an ongoing influence:

> tensions between Romanticism and Rationalism have been formative and remain definitive of the identity and perspective of disciplines such as archaeology, cultural studies, folklore, history, literary criticism, and anthropology. (Ibid, p19)

Romanticism in archaeology, in favouring 'the local and culturally specific over the general', and 'the past of individual peoples or countries, not of humanity as a whole' (Trigger, 2006:111-112) developed strong associations with nationalism (ibid). Interviewee 9 in study 1 noted a resurgence of this connection, and a related struggle for authority in archaeology, stating that, 'we are losing badly in pseudo-archaeology and nationalism'.

In spite of the negative associations between Romanticism and a certain type of anti-rationalist relativism, I suggest that there is no need, when opening up the epistemological landscape, to suggest that it is all flat and equal. This is the realist perspective of relativism, which seeks to take defensive refuge in the notion of a 'natural' order of things (as a nationalist perspective might). Rorty's pragmatic solidarity, in contrast, is based on an ethical approach which can confront unjustifiable knowledge claims. He points out the error of positing wholly different 'criteria of rationality':

> alternative cultures are not to be thought of on the model of alternative geometries. Alternative geometries are irreconcilable because they have axiomatic structures, and contradictory axioms. They are designed to be irreconcilable. Cultures are not so designed, and do not have axiomatic structures. To say that they have "institutionalized norms" is only to say, with Foucault, that knowledge is never separable from power—that one is likely to suffer if one does not hold certain beliefs at certain times and places. But such institutional backups for beliefs take the form of bureaucrats and policemen, not of "rules of language" and "criteria of rationality." To think otherwise is the Cartesian fallacy of seeing axioms where there are only shared habits, of viewing statements which summarize such practices as if they reported constraints enforcing such practices. (Rorty, 2011:371)

This is a reminder that epistemological openness in research need not and should not amount to an abandonment of the values cited by Chapman and Wylie, of rigour, transparency, credibility and accountability.

The areas of archaeological theory I have picked out have emphases which are a good fit with EM and with Rorty's pragmatism. They help to illustrate how a modest, pragmatist approach to research might look, one which avoids both the straightforward 'application of criteria to cases' (Rorty, 2011:371), *and* accusations of a relativism in which every belief is as good as every other. The shift of focus onto specific actors, their histories, their interactions and their agencies, is a restatement of the commitment to be highly accountable to context.

7.7 Epistemological Modesty in context

The existence of various strands of thought which share fundamental concerns with EM is an indication of a widely-felt need for challenges and alternatives to hegemonical ways of knowing. I set out some examples of these here before considering their relationship to the tenets of EM.

Post-colonial and feminist perspectives are significant for those setting out models for alternative archaeological practices. Escobar provides a useful summary of those diverse schools of thought which are relevant to his conception of a 'pluriverse, a world where many worlds fit' (2018:11), one where difference is nurtured rather than effaced and considerations of epistemology are inseparable from those of ontology. In his discussion of Political Ecology, Feminist Political Ecology, Political Ontology and Epistemologies of the South, he identifies a range of alternative visions to those based on the fundamental beliefs of 'the Modern Onto-epistemic Order' (ibid, p64). A strong common theme in these is the rejection of Cartesian dualism and the separation of mind and body from 'the world'; the notion of cognition as representation (ibid, p63). This concept is familiar from the aspiration to a singular 'redemptive truth' which Rorty identifies as a key product of Enlightenment

thinking (§7.6). For Escobar:

> all modes of knowledge based on reason get at only part of the human experience, the reflexive part, bracketing its immediate, lived aspects, that is, our essential historicity. (Ibid, p64)

The implication of this for knowledge is that we need to find 'a *via media*: to understand the regularity of the world we are experiencing at every moment, but without any point of reference independent of ourselves' (quoted from Maturana and Varela, ibid, p64):

> This injunction has been anathema to the Western rationalistic tradition, for which the world out there preexists our interactions. In the enactive approach, we are always immersed in a network of interactions that are at every instant the result of our biological and cultural histories. We necessarily cocreate the world with others (humans and nonhumans) with whom we live in coexistence. The ultimate conclusion drawn by Maturana and Varela is no less startling, and equally foreign to modern logocentrism: "We have only the world that we bring forth with others, and only love helps us bring it forth." (Ibid)

The significance of language for world-making is emphasised by Mika et al., in making a distinction between two ontological orientations:

> one that reduces being to discursive practices, which we call 'wording the world'; and another that manifests being as co-constitutive of a worlded world, where language is one amongst other inter-woven entities, which we call 'worlding the world' (Mika et al., 2020:17).

They speak explicitly from 'Indigenous and racialized loci of enunciation' (ibid) to 'highlight the co-constitution of things in the world by making an ontology that is currently invisible, visibly absent' (ibid, p19). This is explored by contrasting the values of a holistic Māori philosophy of language with the 'grammar of modernity', which they describe as 'wording the world'. While they acknowledge commonalities with Western scholars who have 'problematised rationality, language, representation and being, like Heidegger, Wittgenstein, Derrida and Foucault', they suggest that those scholars have retained a common focus on 'the relationship of the human subject with knowledge, truth and meaning' (ibid, p23). The Māori philosophy of language, on the other hand, allows all

entities ‘to be seen as co-constitutive of each other’ (ibid). This ontological understanding, which is made invisible in ‘wording the world’, has potential to be made conspicuous by:

> acknowledging the limits, partialities and inevitable complicities in harm of the pervasive wording of the world in both coloniality and in well intentioned attempts to overcome it. This acknowledgement de-universalizes the claims of wording the world, creating perhaps a disposition of onto-epistemic humility before the unknowable totality of the world that could interrupt self-congratulatory and innocence/virtue signaling tendencies in academic decolonial efforts. It could also have the potential to create a generative space of emptiness/nothingness where something is missing and is missed: a crack where the nothingness-fullness of the world can erupt, on its own terms. (Ibid, p29)

The authors highlight the difficulty of challenging the authority of the ‘grammar of modernity-coloniality’ from within conventional academic discourse. They also state that the problem of ‘relating wider’ is even more difficult… it calls for an ‘act of collective onto-genesis, something that exceeds what can be done through academic discourse’ (ibid, p30).

Haraway (1997) develops Shapin and Schaffer’s (2011) insights into the ‘modest witnessing’ of Robert Boyle’s experimental science, which they interpret as a means of producing authoritative knowledge claims (see §7.6). She emphasises the assumed ‘invisibility’ of the scientist making modest claims—that ‘such a man must inhabit the space perceived by its inhabitants to be the “culture of no culture”’ (Haraway quoting Traweek, 1997:23). In this case, what seems to be invisible is assumed to be authoritative: but this is performed by way of a sleight of hand, a ‘magic mirror’. The *person* is removed from centre stage in order to foreground the apparently independent authority of nature and the object (Boyle’s air-pump in this case). This definitive, ‘objective’ ontology eclipses or makes invisible those in the ‘domain of culture and of society’ (ibid, p25), as described by Mika et al.; the ‘invisible’ scientist assumes the role of its spokesperson or ventriloquist, while his narratives:

> lose all trace of their history as stories, as products of partisan projects, as contestable representations, or as constructed documents in their potent capacity to define the facts (ibid, p24).

For Haraway, the kind of modesty which Boyle strives to characterise is 'one of the founding virtues of what we call modernity' (ibid). In her perspective, it is the instrumentality of this scientific world in sustaining and creating *gendered* ways of life which is the key to its significance; but her critique can be extended to include other concerns, as she alludes to here:

> The self-invisibility and transparency of Boyle's version of the modest witness—that is, the "independence" based on power and on the invisibility of others who actually sustain one's life and knowledge—are precisely the focus of late-twentieth-century feminist and multicultural critique of the limited, biased forms of "objectivity" in technoscientific practice (ibid, p32)

The invisible 'others' encompasses the contemporary subjects of colonialism and members of the presumed subaltern classes. Haraway asks if it is possible to:

> queer the modest witness this time around so that s/he is constituted in the furnace of technoscientific practice as a selfaware, accountable, antiracist FemaleMan, one of the proliferating, uncivil, late-twentieth-century children of the early modern haec vir and hic mulier. (Ibid p35)

Her answer consists of a call for a 'mutated' modest witness to 'live in worlds of technoscience, to yearn for knowledge, freedom, and justice in the world of consequential facts' (ibid, p267). This takes inspiration from the work of Sandra Harding, and the notion of 'strong objectivity':

> A stronger, more adequate notion of objectivity would require methods for systematically examining all of the social values shaping a particular research process, not just those that happen to differ between members of a scientific community. Social communities, not either individuals, or 'no one at all,' should be conceptualized as the 'knowers' of scientific knowledge claims. Culture wide beliefs that are not critically examined within scientific processes end up functioning as evidence for or against hypotheses. (Haraway quoting Harding, 1997:36).

For Haraway, the notion of affective action, *diffraction* rather than reflection, is important, and she sees Harding's understanding of reflexivity to be complementary to this:

> The point is to make a difference in the world, to cast our lot for some ways of life and not others. To do that, one must be in the action, be finite and dirty, not transcendent and clean. Knowledge-making technologies, including crafting subject positions and ways of inhabiting such positions, must be made *relentlessly visible and open to critical intervention*. (Ibid, p36. Emphasis added.)

The examples I have cited here share an imperative to reject transcendent ontologies and attend to the experiences of being-in-the-world as part of the processes of developing progressive epistemologies. This is a reflection of their emancipatory programmes. In my account of EM I arrive in similar territory, though often via different routes. For example, I explore the theme of pluralistic knowledge models through the work of Lakoff and Johnson, and their setting out of the embodied nature of language and its fundamental basis in our physiological experience of the world. Their work on linguistics and the use of metaphor, like that of Mika et al., demonstrates that language can be an expression of the cultural values which imbue our interactions and inform our perceptions of difference. It is useful, as is Star's work, for showing how knowledge models can be internally inconsistent or contradictory. The conclusion I derive from it is the same as that of Mika et al.'s, of a need for 'onto-epistemic humility' (Mika et al., 2020:29).

I also explore the notion of decentring the human subject, the idea that 'we have only the world that we bring forth with others' (Escobar quoting Maturana and Varela, 2018:64), but via the lens of Actor Network Theory, which provides a framework for understanding the roles of tools and media as well as human networks for producing shared knowledge models. Harding's emphasis on communities as knowers, rather than individuals, finds parallels in my discussions of 'thought communities', tacit knowledge and the genealogy of disciplinary specialisms.

The honest admission by Mika et al. that their suggestions are 'probably impractical in academic contexts' (op. cit., p30) indirectly highlights the potential usefulness of the concept of Epistemological Modesty. Underlying the examples from the literature cited here is a desire for or a working towards different models of praxis and different ways of framing the world, acts of 'collective onto-genesis' (Mika et al., 2020:30). These are mostly articulated in high-level terms—even in the case of Escobar, who's stated concern is design. It is noted that such acts cannot take place solely within the scope of university settings:

> There is no way we are going to intellectually reason our way out of coloniality, in any conventional academic sense. There is no way we are going to publish our way out of modernity. There is no way we are going to read our way out of epistemological hegemony. (Mika et al. quoting Burman, 2020:30)

My research shows an apparent gap between 'everyday' archaeological practice and some of the theoretical aspirations of the discipline. While Haraway writes with inspiring elan about the need to 'be in the action, be finite and dirty' (1997:36), there is always a danger of academic writing becoming a self-sufficient, inward-facing domain of expertise, which fails to make lasting bridges to other aspects of research praxis, particularly in interdisciplinary fields such as archaeology (though as I discuss in §8.5, there is no shortage of archaeology projects striving to put theory into practice). A key goal of Epistemological Modesty is to undertake the more prosaic work of making use of what material is available to lay the foundations for such bridges. In proposing axioms for research design, it looks to contribute to those efforts to manifest theoretical principles in practical projects which might ultimately enable different framings of knowledge production. The Orson project (§8) is a prototype for this.

The characteristics of EM have much common ground with feminist and post-colonial ideals. While I have taken much inspiration from feminist writing in particular (e.g. Gero, Wylie), and multi-cultural critiques of knowledge construction (e.g. Turnbull, Boast, Srinivasan), I have framed EM's premises mostly in terms of theoretical strands from Science and Technology Studies. I count myself as a feminist and anti-colonialist, but as a British man working from within the UK academic establishment, I choose not to use those perspectives as the principal avenues for approaching my field of research: I wish to avoid 'complicities in harm of the pervasive wording of the world in both coloniality and in well intentioned attempts to overcome it' (Mika et al., 2020:29). I have found the adoption of theoretical approaches from STS a useful means of gaining close access to the issues of data collection, organisation and interpretation which are so central to archaeological work. There is, naturally, a danger of following Boyle's example in the event that I present my point of view as *apolitical*, uninflected or disinterested: that I might appoint myself the invisible spokesperson for a definitive ontology. EM is therefore framed in terms of recognising *the limits* of a given knowledge framework and de-universalising its claims. As a result, EM itself is presented as a work in-the-making: it is not exempt from its own logic. The centring of the

concept on matters of epistemology means that it cannot so easily be perceived (and perhaps dismissed) as the specialised domain of a particular interest group: it is applicable to all knowledge-making practices. EM does not posit a singular, 'better' alternative model for relating to knowledge. In one regard this is an acknowledgement of its development from within a privileged discourse, one which is unable to 'read [its] way out of epistemological hegemony' (ibid, p30); but more simply, it has no redemptive alternative to set out. Rather it is a suggested *mode* for relating to knowledge-making processes. The concern with 'what can be known' and for establishing concomitant principles which can find manifestation in research practices (practices which may have opportunities to go beyond the limitations of conventional academic outputs), gives Epistemological Modesty broad relevance. It differentiates EM from other accounts which may have similar concerns, but which discuss them against the background of more specific agendas. It is important to give this mode a name which is self-explanatory, and not confusingly or obscurely defined in opposition to something else: it is not a 'mutated' version of Boyle's modest witnessing, for example. The notion of 'witnessing' connotes an uninvolved separateness, which is inappropriate for an approach which tries to account for its own agency as well as that of other actors in the production of meaning. In my reading of Shapin and Schaffer I concern myself with reconstituting the notion of 'modesty' rather than that of witnessing (§7.6). Neither is EM a 'modest intervention' (Haraway quoting Heath, 1997:36), which implies 'stepping in' to a pre-existent process after the fact, from a place in the margins. It is an inclusive mode for the practical enablement of Haraway's call to make subjective research positions 'relentlessly visible and open to critical intervention' (op. cit., p36).

7.8 Discussion

Rorty points out that there are some discomforts involved in adopting the pragmatist position. One is abandoning the idea 'that membership in our biological species carries with it certain "rights,"' and the supporting notion that we have links to a nonhuman reality which gives the species 'moral dignity' (2011:375). The EM perspective decentres humans in the production of knowledge, and highlights the tendency to misrecognise context-determined knowledge as access to a universal truth. Borrowing from ANT, an emphasis on

the mapping of assemblages and distributed action results in a focus on 'constant becoming':

> For ANT, the world is full of verbs and a continual process of morphogenesis in the sense of subjects and objects in the making (highlighting the need to attend to 'things in the making' rather than things merely existing). (McLean and Aroles, 2016:62)

The implications for research practice are the same as those described by Latour in his goal to be present at the construction of 'black boxes' of knowledge (1987:4) in order to understand what goes into them, and why, and how.

Chapman and Wylie adopt the metaphor of the cable of knowledge, to illustrate the concept of 'multiple determination'. Separate strands, when combined, provide strength. This represents agreement through the application of diverse perspectives in the community of knowers, 'the people to whom one must justify one's beliefs' (Rorty, 2011:375); credibility and accountability by the reduction of objectivity to solidarity. It succeeds brilliantly in showing a rigorous alternative to positivism. What is prioritised is:

> rigour, integrity and transparency in the collective appraisal of knowledge claims that make them accountable to their contexts of production. (Chapman and Wylie, 2016:11)

At the same time, the metaphor invokes a final, complete form of knowledge, one resistant to change as well as challenges; and it does not successfully demonstrate the principle that 'knowledge does not start from nowhere' or illustrate the need to live with 'troublesome knowledge', or accommodate a focus on 'constant becoming'.

A more apposite visual metaphor for an epistemologically modest model of knowledge might be that of a river course or delta with a tangled mass of channels, as in the map of the Mississippi in Figure 6, where both the origin and ultimate destination of the river remain unseen. Knowledge cannot emerge from nowhere, and it cannot flow arbitrarily—it must make use of existing channels, in the form of its disciplinary genealogy. Channels may merge and combine, or feed into each other, but often they remain separate (as when practice

does not correspond with theory). Channels may become deeper or silt over, as environmental conditions change, and as the flow itself makes its impact felt. As new channels are opened up, knowledge flows into them rapidly until it finds an obstruction.

Figure 6. Map of the Ancient Courses of the Mississippi River Meander Belt. Source: Lower Mississippi map series, Library of Congress.

The EM approach then, represents a shift away from the search for universal truths, towards a more carefully considered assessment of the rich contexts in which knowledge claims are made. This is reminiscent of Haraway's notion of 'staying with the trouble', which:

> requires learning to be truly present, not as a vanishing pivot between awful or edenic pasts and apocalyptic or salvific futures, but as mortal critters entwined in myriad unfinished configurations of places, times, matters, meanings. (2016:1)

'Staying with the trouble' means learning to live with troublesome knowledge. It might be characterised as the aim of knowing more about less. Arguably, the rejection of exclusive models of knowledge, and the willingness to 'live with mystery' can also restore a sense of *enchantment* with the world, an escape from the sense that:

> the world is embarked on a path at the end of which there will be no more mysteries. All things are taken to be potentially capable of explanation in terms that are acceptable to the rationality of science, and susceptible to intervention the outcomes of which are predictable. (Jenkins, 2000:16)

This might offer a path to breaking down the divisions between 'hated reality and welcome fantasies, science and art' (Feyerabend, 2010:32).

* * *

In this chapter I have discussed the requirement for a conscious epistemological programme in research, and its features, namely: recognition of the situated nature of knowledge; the need for a discursive relationship with the history of research disciplines; acknowledgement of the importance of communities of knowers for constructing claims; and a willingness to accommodate 'troublesome knowledge' which has the capacity to highlight the *limits* of knowledge models. I have suggested that the mainstream of archaeological research is defined in significant part *negatively*, in opposition to a realist formulation of relativism, in which all values are equal. The requirement for an epistemological programme arises from

the need to challenge the traditional positivist/relativist dichotomy and show that non-exclusive approaches are possible; approaches which can be ethical, rigorous and accountable.

The principles set out here for EM are not intended to be definitive or exhaustive: they are themselves ideas 'in the making', and are intended to function as prompts for the reconsideration of what epistemic virtues should consist of. In the next chapter I will consider what the practical characteristics of an epistemologically modest research practice might be, in particular with regards to digital tools for research documentation.

8. Study 4—Software Development Project

8.1 Introduction

As part of my research I wanted to investigate whether the ideals of transparent and self-reflexive research could be supported or encouraged by the affordances of research tools. In pursuit of this aim I undertook a software development project which sought to manifest some of the ideals of the 'epistemologically modest' approach in terms of its features and associated workflow. While I have argued the case for the conscious adoption of an epistemological programme in research, I realise that for many researchers, considerations of this sort will be interpreted as a specialist concern. For this reason I wanted to explore the possibility of building expectations of practice into a research tool, where the natural affordances of the tool might encourage epistemic virtues such as transparency and reflexivity without the need for the discussion of such themes using a specialised vocabulary. This was also intended, as a part of an ANT-oriented study, to be a means of exploring the relationship between research tools and other agencies which shape the research process.

In this chapter I will start by discussing the development of the software and the ideas which informed it, then go on to describe the feedback I received from students who tested the software in hands-on sessions.

8.2 Evolution of the software project

At the outset of the project, my plan was to build a visualisation tool, or set of tools, which would be useful for the representation of uncertainty, or for communicating the situated nature of knowledge, as a means of supporting transparency in research. As my research progressed, it became increasingly difficult to envisage how such tools might tally with my changing research priorities. Looking at the London Charter initiative and the work it had inspired on the representation of cultural heritage reconstructions using 3D visualisations, it became apparent that whatever the charter's authors' intentions were, the inclusion of

measures of uncertainty in research outputs was typically used as a means of expressing more explicit information about the completeness of a model, rather than about any degree of epistemological certainty. In fact it became difficult to imagine individual visualisation techniques which might be capable of explicating something more than this ontological level of uncertainty. One inspiring example was provided by an archaeological project (Bonde et al., 2009), which sought to actively problematise the authority of visualisations by showing a range of possible interpretations in visual form. The virtue of this approach, while motivated by the need to challenge the sense of inherent epistemological authority communicated by images, was not peculiar to visual media, however; it was a consequence of an openness to alternative research narratives. The emphasis in my project therefore changed from the visualisation of knowledge, specifically, to a wider consideration of the representation of knowledge. This had a strong bearing on the development of the notion of 'epistemological modesty' and on possible directions to take with software development.

8.3 Orson software—goals, structure and features

I opted to develop a research notebook software package, a flexible digital tool for the self-reflexive documentation of research. This seemed to be an ideal fit for considerations of the capture and sharing of research context. I saw potential for such a tool in three main areas:

1. Data collection and documentation of process
2. Active reflection and development of research theories
3. Writing up and sharing of projects

I imagined an ideal scenario in which there was synergy between these phases: the tool would provide a space for the collection of data, information or ideas, and for the documentation of research assumptions and processes; it would then enable reflection and the development of hypotheses by allowing the collected resources to be combined in different configurations and annotated; and finally, it would provide features for narrative summaries of the research. This paradigm provided the potential for contributors to

reconfigure the collected resources, or add to them, and to share comments or narratives which represented their own unique perspectives and understandings.

The name I gave to my software project was 'Orson', an approximate acronym of the phrase 'Open Research Notebook'. A suitable web domain was purchased to host the project online, https://orsn.io. The source code for the project is available, at the time of writing, at https://codeberg.org/orson/orson. The version of the code which is representative of the feature set at the time of writing is v0.3.7.[35]

The structure of the Orson software is as follows:

- An Orson instance hosts multiple Projects.
- Each Project has Cards, Contexts and Pages.
- Cards can have Tags.
- Cards can be added to Contexts. (A Context is a container for browsing or interacting with a group of Cards and other data. This is a non-exclusive relation. The same Card can be added to multiple Contexts.)
- The *content* of Cards can be added to Pages.
- Links to Cards, Contexts and Pages can be embedded in Pages, with live previews of these available alongside the main Page text.

[35] See https://codeberg.org/orson/orson/tags

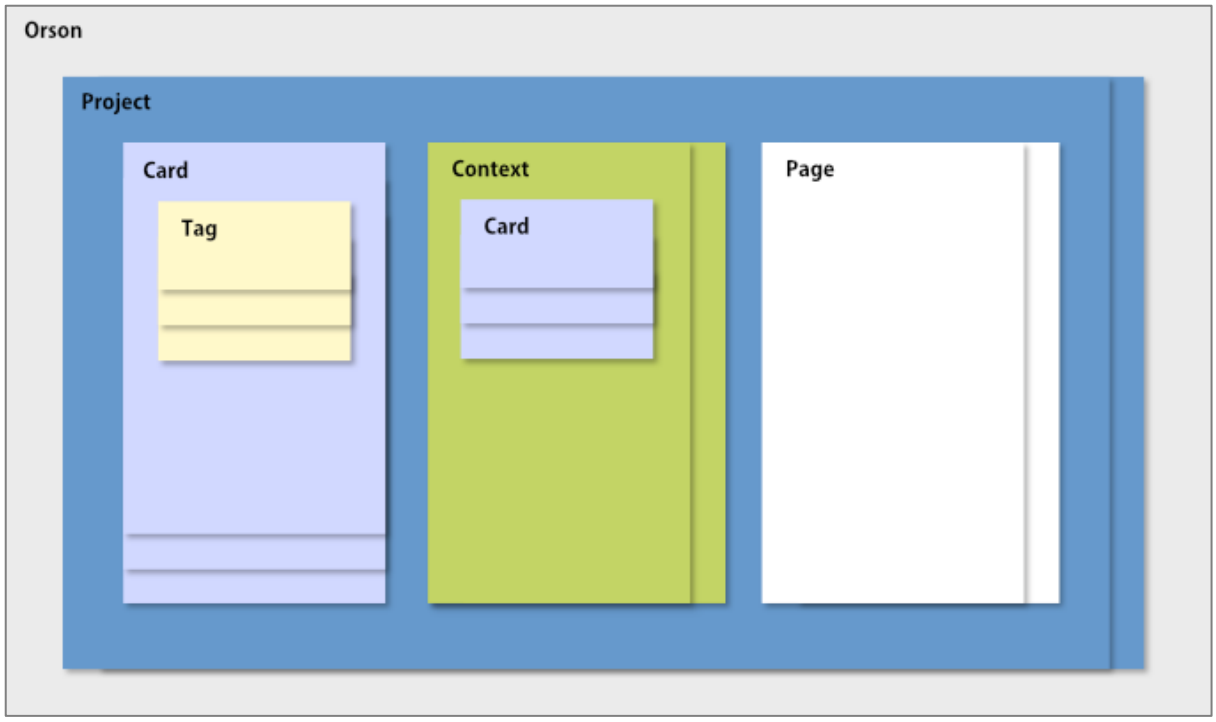


Figure 7. Orson software structure.

The basic structure is visualised in Figure 7. Non-hierarchical structures and non-fixed architectures were selected where possible, so there is a ‘flat’ relationship between all Cards, for example. Contexts provide a structural framework for organising Cards in relation to each other, one which need not be hierarchical in nature.

The main elements of the Orson interface are shown in Figure 8.

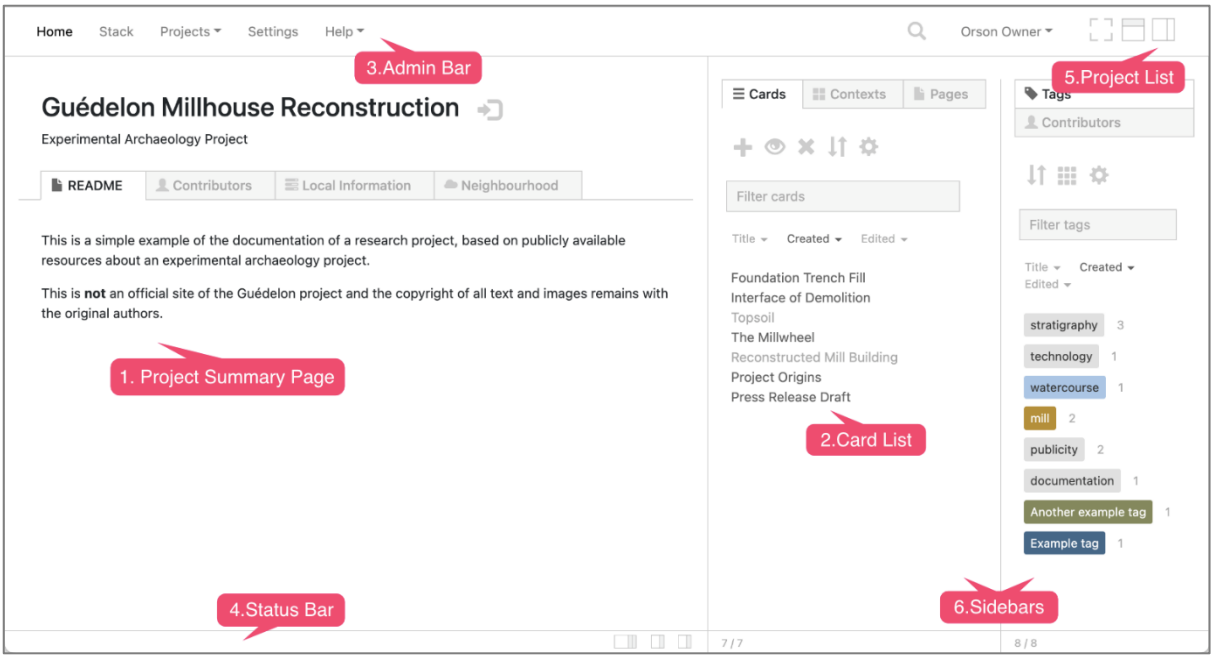


Figure 8. Orson main interface elements.

The starting point for Orson was a basic prototype of a knowledge-management software project which I had developed for my own use, inspired primarily by the historical use of commonplace books (e.g., Hess, 2022) and modern digital variations of them. One aspect of adapting the software to its new purpose involved reimagining it as a notebook for capturing the ongoing work of a research project as it unfolded. The principle that Orson should provide research journal or notebook functionality was inspired by calls in the London Charter and the Seville Principles to capture and share the detail of research processes, so that otherwise opaque research techniques should be opened up fully to scrutiny (Bentkowska-Kafel et. al, 2012). This approach is also a useful means of encouraging reflexivity (Mickel, 2015) and of capturing any changes to a research project's goals over the course of its life. (Similar concerns are addressed by initiatives in open access publishing in which a 'preprint' of the research project output is published before it is reviewed and officially published (e.g. Knöchelmann, 2019:7). This is seen in part as a safeguard against the retrospective rewriting of a research agenda to fit the findings.)

The use of Electronic Lab Notebooks (ELNs) has become widespread in the natural sciences (Sayre et al., 2018:2), mainly as a digital manifestation of the well-established laboratory notebook format which is designed to make experimental practice reproducible. While the

notion of reproducibility is somewhat problematic for disciplines using non-experimental practices (Knöchelmann, 2019), the motivation for transparency and the capture of 'paradata', and the desire to make tacit knowledge explicit where possible, remains highly relevant for cultural heritage scholarship and other disciplines. Another motivation for Orson, then, was to explore what shape an academic digital notebook for cultural heritage and the humanities might take.

8.3.1 Software paradigm

The basic metaphor for structuring content in Orson is that of a file system using index cards. The Card[36] is therefore the basic unit of information in the Orson system. Each Card has an editable text field and also supports the upload and display of images. To facilitate the use of the software as a research journal, each Card is given a default title of the date and time when it is created (see Figure 9.) Titles can be edited as desired if a Card is not being used as a journal entry.

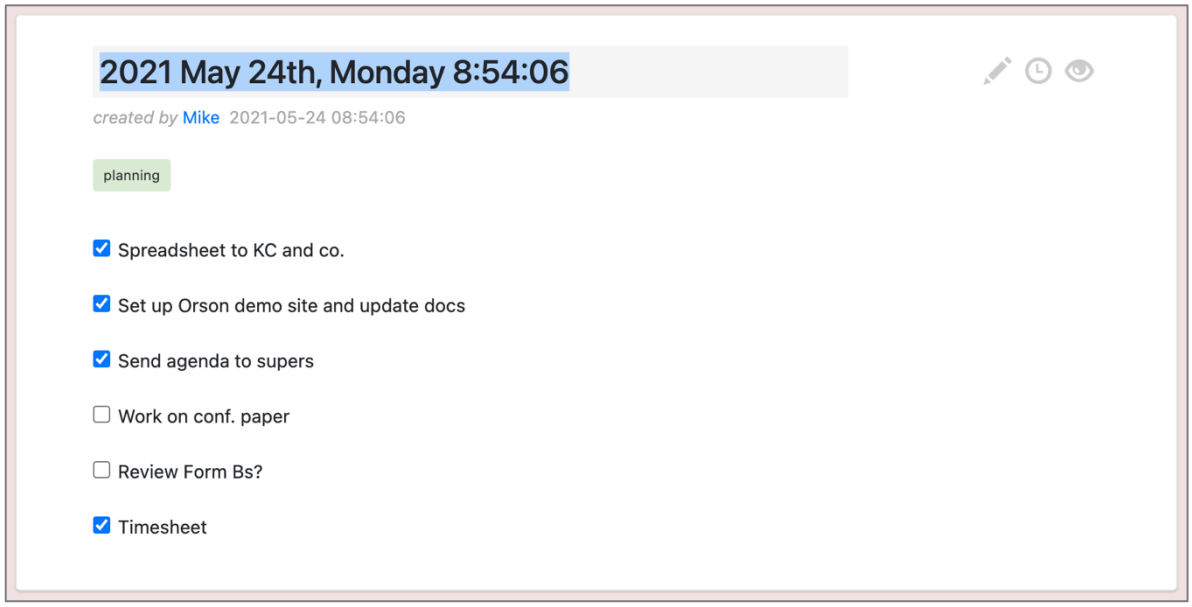


Figure 9. Date and time as default Card title in Orson.

[36] I will capitalise Orson content types, i.e. Card, Context, Page, Tag, in order to distinguish them from their homonyms.

Tags can be assigned to Cards, to facilitate categorisation, search and discoverability. The main interface page in Orson displays a full list of Card titles, allowing them to be ordered and filtered in various ways, including through the use of Tags. Each Card is opened for viewing when its title is clicked, on a temporary 'stack' of Cards[37]. The default arrangement of the list of Card titles is by order of creation, meaning entries are displayed chronologically like diary entries (see Figure 10).

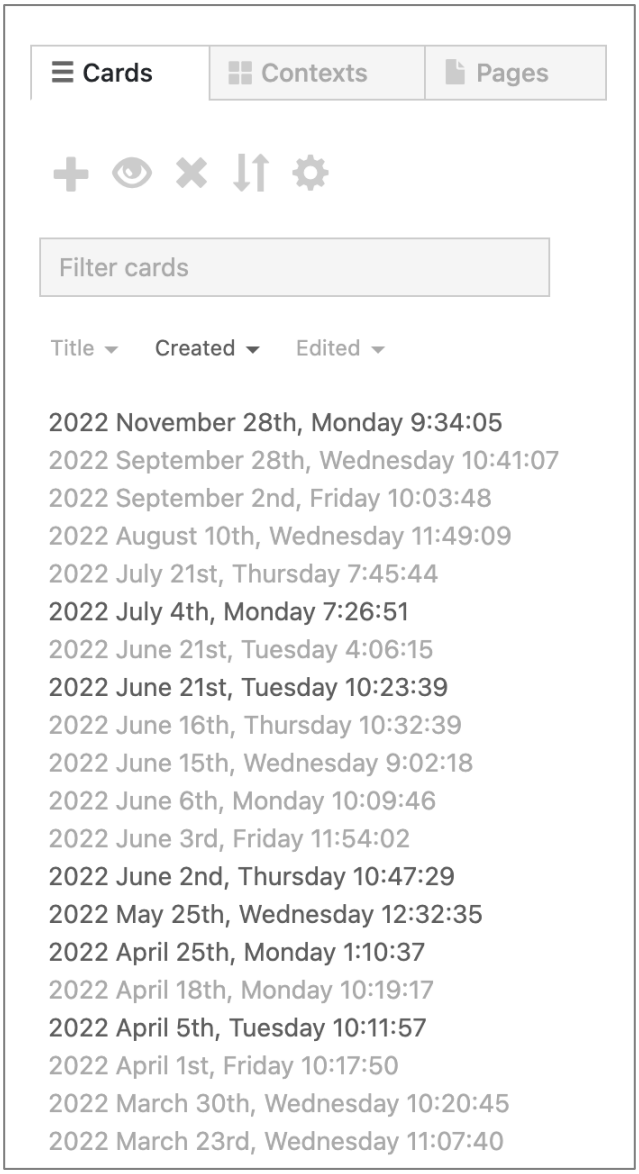


Figure 10. Orson Cards as journal entries.

[37] The term 'stack' alludes to both a physical stack of index cards and to the part of a computer CPU which stores working data resources before performing operations upon them.

The paradigm of interaction is one where a collection of items of evidence, ideas, proposals, images and pieces of data can be arranged and interacted with in many different ways. This is a core concept in the software: that there is no single fixed mode of presentation, and even where these individual resources are collected together to mark commonalities, make sets or contribute to the construction of narratives, it should always be apparent that they have the potential to be organised and presented in *alternative* ways.

To facilitate this affordance, an important feature of the development of Orson was to reconceptualise what had originally been imagined as visualisations or 'boards' (from 'pinboard' or 'whiteboard') as 'Contexts'. The notion of sharing information, or data, in different Contexts, is a manifestation of the idea that knowledge is situated; and that, by the same logic, information, data or research evidence can be viewed or understood from a range of different perspectives. Revisiting Drucker's example of John Snow's famous cholera map (see §1.5), the flexibility of having multiple Context 'types' in Orson allows for the possibility of data to be represented both on a map with a conventional perspective, or on a map with a first-person perspective. Both types of Context are theoretically possible, as the structural paradigm of Orson allows for the same units of content to be represented in different Contexts. This is achieved by allowing Cards to be *added* to Contexts, thereby giving concrete expression to the notion that knowledge is constructed and contingent, rather than universal and revealable.

Orson Contexts therefore have the potential to be more than just visualisations: they are opportunities for organising information in different ways, while bringing in other relevant information, but they need not involve conventional 'graphic' elements. A Context could be a straightforward grouping of elements (Cards) in the form of a list, for example. Alternatively, it might present the opportunity to arrange content elements into a hierarchical structure, or as nodes in a network. Contexts also offer opportunities for distinctive types of *interaction*, as in the case of dragging cards around on virtual pinboard. A core goal of this flexible approach is to allow for both *multi-perspectival* understandings and for *reinterpretation*, through the rearrangement or reorganisation of the original elements, and through the *comparison* of different Contexts. A standardised set of visualisations would not have been sufficient to support this ideal. A 'plugin' architecture was developed instead, which allows programmers to contribute custom Context types,

thereby extending the software to suit their particular research requirements, and bringing to bear the contingencies and agencies which are specific to their research situations.

The built-in Context types in the prototype of Orson which was tested with users were as follows:

- Annotation—For highlighting and adding commentary to text in Cards.
- Comparison—For comparing the content of Cards, Contexts or Pages.
- Map—For showing the location of Cards on a map, or adding notations to a map
- Node Graph—For organising Cards or other data as nodes in a graph of linked data
- Pinboard—For displaying Cards in a pinboard layout
- Slideshow—For displaying Cards as a slideshow
- Stack—For saving a simple collection or grouping of Cards
- Timeline—For displaying Cards and other items chronologically

The built-in Context types in the Orson prototype were primarily designed to support qualitative data, with the inclusion of maps and timelines being a reflection of the preoccupations of cultural heritage research. (A Table Context was also developed for the representation of numerical data, but this was not activated during test sessions.) This was assumed to be the best option for conveying the underlying principle of Contexts to users, and avoiding the impression that Orson was primarily a tool for the computational manipulation or visualisation of stand-alone numerical data, which tended to be the default expectation from those expressing interest in the project.
The Node Graph Context (see Figure 11) was developed specifically for its potential relevance to archaeologists creating digital Harris Matrix (Harris, 1989) representations of stratigraphy.

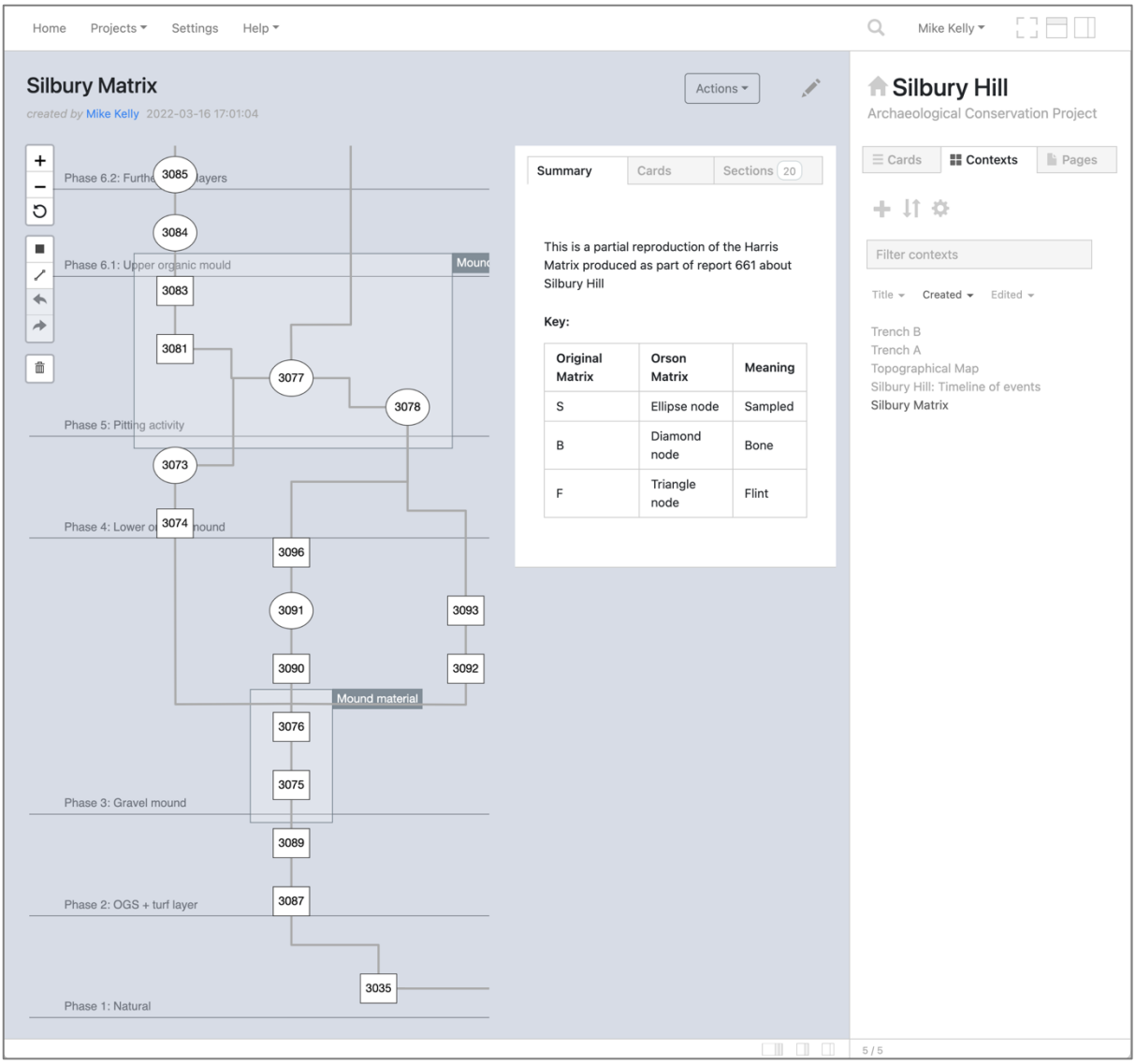


Figure 11. Orson Node Graph Context showing representation of a Harris Matrix.

The Comparison Context was developed as a means of supporting reflection and reinterpretation, and after Bonde et al. (2009), potentially *problematising* the impression of authority conveyed by certain types of representation, by virtue of showing alternatives. It allows for a split screen view of two different Cards, Contexts or Pages, with the option in certain cases to highlight differences between them. See Figure 12.

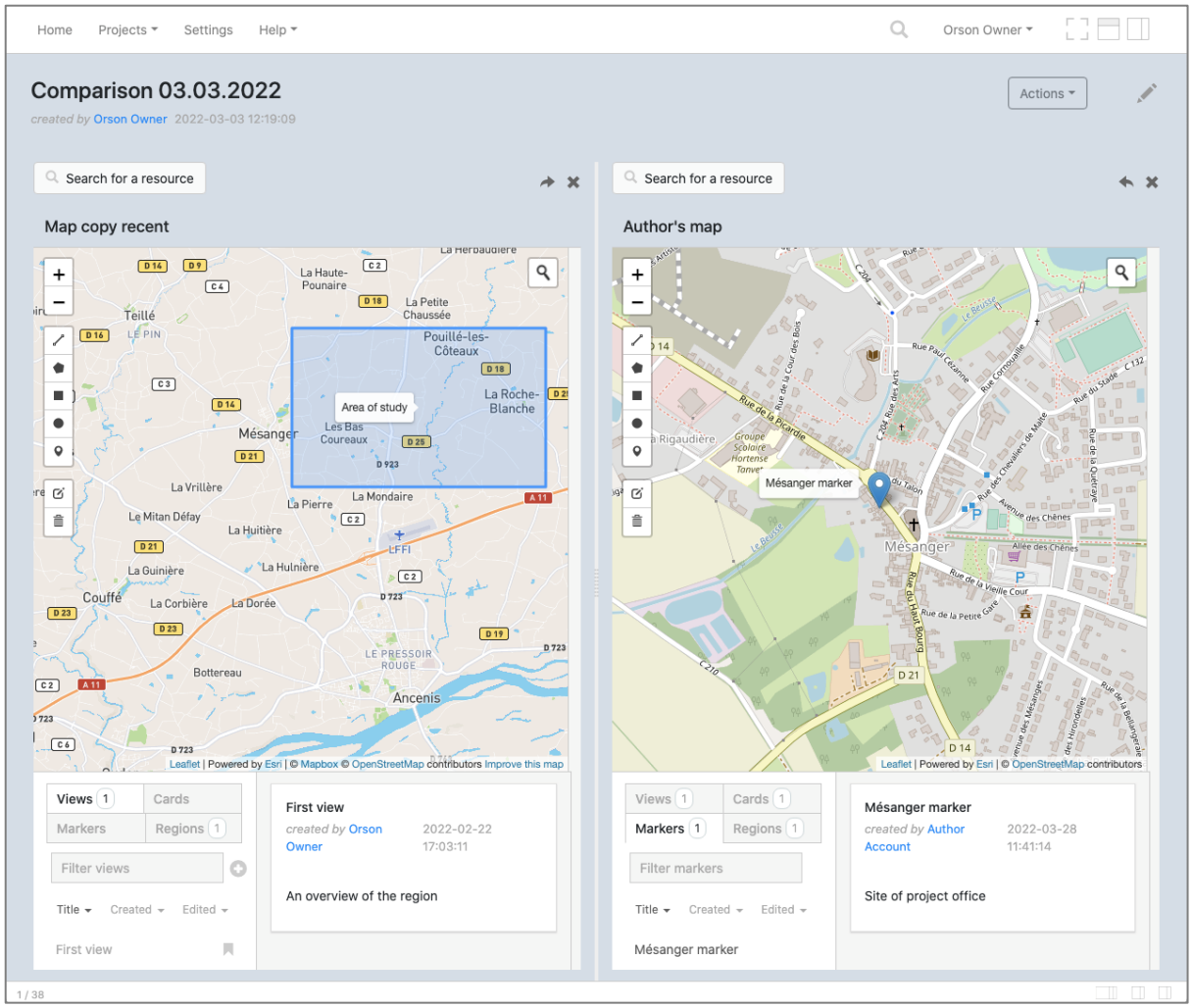


Figure 12. Orson Comparison Context showing split view of two maps.

In support of the same goals, a feature was added to allow for the capture of the historical revisions of units of Orson content, so that the evolution of that piece of content could be reviewed in its various revisions, if desired. See §8.3.2.

The other main content type in Orson, after Cards and Contexts, is ‘Pages’. Pages are also text and image-based pieces of content, designed for long-form text. They are intended to support the writing-up phase of a project—the creation of research narratives. They differ from Cards in that they are created, like Contexts, via a ‘plugin’ architecture, so it is possible to have different Page 'types’. This again allows for diverse presentation forms, reflecting the specialised needs of different research disciplines. In the Orson prototype, there is only one type of Page available, ‘SimplePage’. It makes concessions to the process of producing

long-form pieces of text by adding UI support for the formatting of text, in the shape of a toolbar. Importantly, and in-keeping with the ideals of reuse and reinterpretation, the 'SimplePage' content type allow content creators to embed references to other resources in the Orson system, namely Cards, Contexts and other Pages. These appear as normal hyperlinks in the text, but the system recognises these internal links and automatically adds a preview button beside them (highlighted in Figure 13). Previews are displayed alongside the body of the Page text. As well as adding interactivity to research narratives, this provides a means of linking the documentation of *process* (if this has been done using Orson Cards for example) with the summarised *outcome* of a project, emphasising the constructed nature of research narratives and that other accounts are possible.

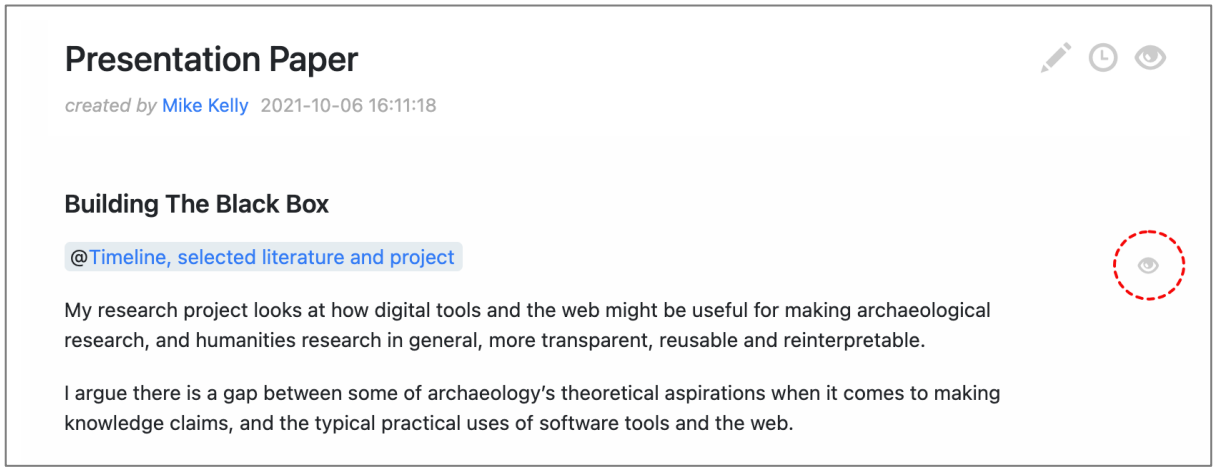


Figure 13. Detail of an Orson Page with an embedded link to an internal resource. A preview link is automatically generated (eye icon highlighted red).

8.3.2 Revisions

The Revisions feature was developed as a part of the goal to give research projects a 'memory', both to allow authors to reflect honestly and accurately on the genesis of knowledge claims, and to allow those looking to reuse or reinterpret project materials to study the decisions which were made in creating the content, given the appropriate permissions. When this feature is enabled, a snapshot of the current Card or Page is saved with a timestamp whenever an edit is made and more than 5 minutes (or another pre-

specified time) have elapsed since the last save. It is then possible to browse through these snapshots retrospectively and view the differences between versions, using a 'diff' algorithm[38] with colour-coded visualisation. Individual snapshots can be named for reference, and revisions can be filtered by name or date. See Figure 14.

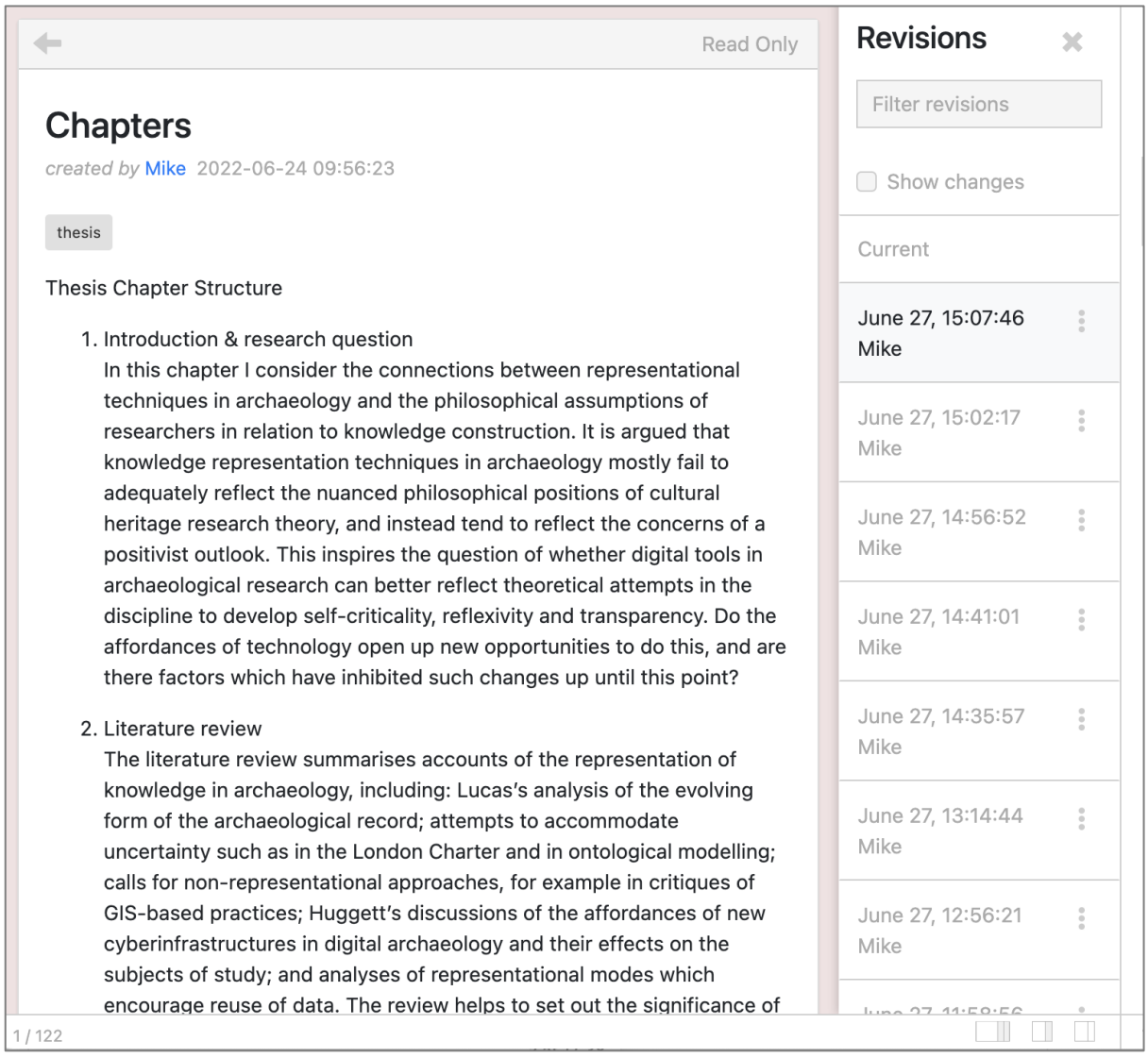


Figure 14. Browsing the revision history of an Orson Card.

[38] An algorithm which checks for differences between two versions of a text, A and B, and lists a set of 'patches' for modifying the state from A to B. The algorithm used in Orson was the following: https://github.com/google/diff-match-patch

8.3.3 Authentication and permissions

I spent a significant period of time on developing an authentication system for Orson in order to make it suitable for collaborative work and selective publishing. I implemented a minimum feature-set to allow for authentication at login and selective access to different pages and page components, based on a set of user roles. These roles, and their related permissions, were adapted from those in the well-known blog software Wordpress[39]. The development of a functional authentication system was necessary in order to be able to share a demo site of Orson with potential users, and to allow them to create content without fear of it being edited or deleted by others. Relatedly, the demo site needed to have a 'read-only' mode if it was to be shared to a wider group of potential users without editing rights. These were important factors for enrolling the interests of potential future users and conveying the long-term potential of the software. Moreover, it was clear that open access, collaboration, privacy and selective publishing would be core themes to discuss with users when getting feedback about the software. I therefore deemed it important to have some of these features in place before running feedback sessions, in order to avoid holding these discussions wholly in the hypothetical realm.

8.3.4 Contributor features

Based on the same considerations, I added in functionality to create and manage user accounts, added filtering of content by users, and created user profile pages. I also added the option to display authorship credits on all pieces of content. Thus the software was updated from being an anonymous content management system to a documentation space with basic collaborative authorship functionality. Users were designated as 'Contributors' in the software interface to accentuate the opportunities for co-creation.

8.3.5 Research virtues

The relationship between Orson features and the themes of Epistemological Modesty is summarised in Table 5.

[39] https://en-gb.wordpress.org/

Table 5. Orson prototype features in relation to Epistemological Modesty.

Orson Feature	**Relationship to EM / epistemic virtues**
Journal-based record of research process	Recognise situated and contingent nature of knowledge. Support reflexivity and transparency, capture of tacit knowledge, paradata, uncertainty. Paradigm of emergent knowledge rather than fixed solutions
Modular, plugin-based structure	Acknowledge distinctive epistemological and ontological commitments of different disciplines; support alternative interpretations and perspectives
Interoperable plain-text content	Facilitate reinterpretation and reuse
Revision histories	Support reflexivity and transparency. Paradigm of emergent knowledge rather than fixed solutions
Emphasis on context-based presentation	Recognise situated and contingent nature of knowledge. Support alternative interpretations
Comparison of resources	Support alternative interpretations, 'complication of causality'
Annotation of content	Support reflexivity, alternative interpretations and perspectives, documentation of uncertainty or ambiguity
Reconfigurable content (via assignation to different Contexts)	Support reinterpretation and reuse, 'complication of causality'
Research narratives with dynamic links to other internal content	Transparency—embed research processes and practices directly into research narratives

Table 5 cont'd. Orson prototype features in relation to Epistemological Modesty.

Orson Feature	**Relationship to EM / epistemic virtues**
Multiple users and user roles, user profiles	Provide context about contributors. Enable multiple perspectives, multi-vocality, acknowledge making of 'knowers'
Contributor credits	Acknowledge making of 'knowers', acknowledge significance of perceived authority, significance of credit for contributions
Selective publishing	Acknowledge barriers to full open access in real-world research. Recognise need for privacy and confidentiality in some fields of research

8.4 Software design considerations

The proposed software project had a wide scope, and it was clear that I would not be able to fully realise all my ambitions for its features in the time available. I therefore tried to prioritise the creation of a minimum feature-set which would allow me to test the software with users in a meaningful way. I sought to have a prototype which was sufficiently realised to prompt in-depth discussions about the ideas underlying the software's design. In addition to using Orson as a vehicle to test the ideas which informed the EM approach, I wanted to treat the project as one which might have the opportunity, at a later point, to develop into a real-world resource of genuine utility for research. With this in mind, I adopted certain principles in its development, based on previous personal professional experience, my research findings in this project, and reference points in the literature.

Latour's Aramis (1996) is an insightful explication of the many agencies involved in the development and deployment of a technological initiative, and of the requirement to enrol the interests of relevant parties if such an initiative is to succeed. It describes a planned novel public transport system which failed to become a 'real-world' project, an 'institution',

because its authors refused to deviate from a technical theoretical ideal which did not overlap with the interests of the relevant agencies:

> Aramis had not incorporated any of the transformations of its environment. It had remained purely an object, a pure object. Remote from the social arena, remote from history; intact. (Latour, 1996:280)

It was therefore 'fated to remain a Utopia, a UFO' (Latour 1993:391). Latour notes that:

> An object cannot come into existence if the range of interests gathered around the project do not intersect. Of course, interests may be modified and so may projects. But, if the two-way movement translating interests and modifying the project is interrupted, then the object cannot become real. […] The locus of enquiry is to be found in the exchanges between the translated interests of humans and the delegated competences of non-humans. As long as this exchange goes on, the project is alive and may become real. As soon as it is interrupted, the project dies, and we obtain, on the one hand, a social assembly of quarrelling human actors and, on the other, a stack of documents and a pile of idle and rapidly decaying technical parts. (Ibid)

These insights led to the conception of Orson as more than just a technical software-based solution to a problem of documentation, but as a potential site of evolving exchanges and translations between human, technological and other actors. The features of Orson were therefore designed to facilitate exchange and participation, and to allow for the embodiment of a range of interests.

8.4.1 Implementation of features

A significant barrier to participation in using open-source software is the learning curve associated with the installation of complex packages and dependencies. I therefore avoided designing a technological Portuguese man o' war, made up of multiple services, such as a separate search engine, image server, etc. Instead, Orson consists of one main service and a single database, and is all coded in the same programming language, Javascript. At a technical level, the project tries to be database agnostic, by using a code library which creates database transactions based on data object models. Programmers are thus able to write code which addresses those data objects, while the database-specific operations are

taken care of in the background. This means that the same code base can be used to communicate with various well-known relational database types: PostgreSQL[40], MySQL[41], MariaDB[42] and SQLite[43], opening installation to a range of people with varied database administration experience. For the same reason I avoided using a specialised graph database, which would have posed a barrier to participation for many users.

This database agnosticism was also implemented to allow Orson to be built in different manifestations, both as a *web* application (accessible via a web browser, using a web server-based database) and as a *desktop* application (installed on a local computer, using an embedded SQLite database), thus spreading its potential reach considerably. I anticipate that the future release of a simple-to-install desktop version of Orson will be the point at which most potential users will take the opportunity to test it.

My own professional experience, and the findings from the observations described in study 1, taught me that software research projects with many moving parts require a significant piece of external 'social' work to set up and maintain. Usually this is only feasible in the context of funded projects owned and hosted by institutions. One could not easily set up and maintain an instance of Arches in order to simply explore a research hypothesis, for example. Imagine, if every time you wanted to write an academic paper you first had to build a writing desk by hand from planks of wood, or manufacture sheets of paper from wood pulp. The goal for Orson was to make project documentation open to all scales of project, to allow for projects to be easily thrown up *and* torn down, without significant consequences in terms of the investment of time and money. Expensive projects with preconceived outcomes and strong links to institutional reputation cannot afford to fail, and are therefore strongly constrained by a negative epistemology. A useful analogy is to be found in Turnbull's account of the construction of Gothic cathedrals. He claims that these most impressive of European buildings were not conceived of using detailed and complete plans drawn up in advance. Instead, skilled masons used standard templates for constructing the constituent parts, ensuring continuity of geometry and scale; and the final forms of buildings emerged only as the construction work proceeded:

[40] https://www.postgresql.org/
[41] https://www.mysql.com/
[42] https://mariadb.org/
[43] https://www.sqlite.org/index.html

> They were in effect large-scale laboratories where knowledge spaces were coproduced through a process of contingent assemblage. Cathedral raisings were sites of experimental practice where the collective work of skilled specialists was aggregated, producing a manipulable system and a working experiment—the cathedral itself. This was possible in the absence of fully articulated structural theory, specified design or plans, or even of common measure, because the builders developed ways in which their local and tacit knowledge and their disparate practices could be combined and transmitted to other sites in the form of skills, geometric method and templates. This constituted a tradition of shared solutions and skills in which theory and practice were integrated and no strong distinctions were made between science and technology. (Turnbull, 2003:81)

Arguably, small-scale research tools can be used in a similar way to create large-scale outputs, without the need for a preconceived analytical destiny. An appropriate set of templates creates the conditions for accumulation and innovation. Turnbull notes the resonance of the cathedral building model with a characterisation of modern science, quoting Holton:

> ...the scientists' chief duty [is] not the production of the flawlessly carved block, one more in the construction of the final Temple of Science. Rather it is more like participating in a building project that has no central planning authority, where no proposal is guaranteed to last very long before being modified or overtaken, and where one's best contribution may be one that furnishes a plausible base and useful materials for the next stage of development. (Ibid, p77)

Such a model is also resonant with an epistemologically modest practice, and its emphasis on 'continuous becoming'. I seek to explore how this ideal can be practically manifested in software, in the discussion below.

The licensing model of Orson is designed to encourage community use and engagement. The source code is open for anyone to edit and customise, and the GPL3[44] licence ensures that any derivative versions have to be shared openly in the same way.

[44] https://www.gnu.org/licenses/gpl-3.0.en.html

The code style of the software is intended to be readable, consistent, and replicable, to encourage other developers to contribute custom features. At some points of the development, I favoured moderate and manageable levels of code repetition and verbosity over deep levels of abstraction and brevity, in order to make the code easier to navigate and understand. Even so, the project is not as accessible in some respects as I would have liked, because there is a learning curve involved in becoming familiar with the modern component-library-oriented 'technology stack' which I used to build Orson. Even so, I am confident that it should be possible for developers to make programming contributions to the software, particularly at the level of 'plugin' development, without the need for an in-depth understanding of its overall architecture.

Orson has a modular, plugin-based structure, which is a fundamental part of the goal of enrolling diverse interests in its use. It is anticipated that different plugins will be required for different research specialisms. The opportunity to renew the software by extending its functionality is also an important feature for retaining the interest of users in a landscape of ever-evolving software applications and specialist tools. As interviewee 5 in study 1 put it, research is 'an economy of time and attention.' Technological novelty is a strong draw for researchers looking for more efficient and engaging ways of working.

A plain-text format was selected for content creation in Orson, to ensure that the content remained future-proof and supported interoperability. This was also an important consideration for the support of a 'document history' feature, to enable comparisons between different versions of text without the need to account for obscure and/or proprietary formatting conventions. To satisfy the requirement for attractive content formatting and a good level of readability, the use of Markdown[45] text formatting was incorporated. This allowed for the inclusion of some simple optional formatting conventions in plain-text files without significantly affecting readability. The use of Markdown was a compromise, because the concept of 'inline' visible formatting codes is an unfamiliar one to non-technical users, who are more accustomed to the WYSIWYG[46] formatting conventions

[45] https://daringfireball.net/projects/markdown/
[46] 'What you see is what you get.'

of word processors like Microsoft Word[47]. However, I deemed it to be the best option for maintaining content in a plain-text format which would be both readable, future proof and compatible with versioning systems like GIT[48]. It was also apparent from my research that the use of Markdown was becoming increasingly common in other research contexts, as in software repository README files on sites like GitHub[49], and software packages like the data analysis tool Posit (formerly RStudio)[50]; and that Orson users could, in any case, successfully create text content without any need to be aware of Markdown conventions.

8.4.2 Reusability

One of the key themes I have discussed is the ideal of the reusability of research. I conducted a mini literature review specifically on factors which have a bearing on the reusability of research results (see Appendix 3), the findings of which were useful for the development of Orson's design and features, and for the design of the questions for the related feedback sessions. The design responses to the main findings on reusability are outlined here:

1. *Share information about authorial reputation and quality of project curation.*

My early efforts in developing the Orson software tool considered authorship mainly in relation to the creation of individual pieces of content, the documentation of authorship responsibility and chronology. The literature suggested that especially at the point of assessing suitability for reuse, an overview should be available of the creators of a resource, with reputational indicators, and any institutional affiliations they have. As a result, creators were given more presence in the Orson, effectively making them an additional 'content type'. Spaces were provided to fill out creator profiles and add links to external sources of information about them. Information about all contributors to a project was made discoverable from a single 'tab' in the project's home page. (Institutions or their departments could also theoretically be introduced as content types in a future update. This

[47] https://www.microsoft.com/en-gb/microsoft-365/word
[48] https://git-scm.com/
[49] https://github.com/
[50] https://posit.co/

would allow for the inclusion of information about, for example, standard local practice in archaeological site excavations.)

2. *Capture metadata and paradata.*

Detailed contextual information is required if research results are to be reused or reassessed. Some aspects of metadata can be generated automatically: for example, chains of authorship and dates of edits. In Orson this is achieved by the inclusion of revision histories and the option to browse through previous iterations of documents.
Support is provided for the journalistic recording of research processes.

3. *Cater for different disciplinary requirements in sharing contextual information.*

Faniel et al. (2019) explain in detail how researchers in different disciplines have different needs when it comes to assessing the reusability of datasets. Social scientists, for example, have a particular interest in missing data. In Orson, a modular structure with plugins allows the research software to cater for specialised requirements in the representation of and interaction with research. For example, a content-type was developed to represent Harris Matrix visualisations of archaeological stratigraphy.

4. *Provide information and advice about reuse.*

Each Orson project has a README file which also functions as a start page ('home' page in a web context) for viewers engaging with the project. READMEs can contain information about relevant institutions as well as authors, and could include instructions for reuse. This draws on the familiar tradition of software repositories and helps to cement the *expectation* of reuse. Conventionally, a software project README will contain information about how to install and get started with using the software, and will also link to any associated documentation such as API guides. This model could translate well to the context of research documentation.

5. *Standardisation may encourage reusability.*

The prototype of Orson does not support standardisation. One option to support a degree of standardisation in research software, while maintaining sensitivity to context, would be to allow for the user creation of content templates. In the case of Orson, this might consist

of having an option to save a Card as a template, or to set it as the default template for a Project. The contents of that Card would then be used as the starting point for any new Card added to the system. A skeleton document with standardised section headers could thus be basis of all new items of content. Leaving the creation of templates open to users might help promote the ideal of the discussion of metadata standards recommended by Kim (2021).
To fit with the philosophy of the Orson software, any future use of standards in the software (e.g. the implementation of semantic ontologies) should allow for comparisons between possible standards, or support commentary on them, or facilitate the addition of other contextualising information.

6. *Community norms have a significant influence on the extent to which reuse is practiced*
7. *Evidence of the efficacy of reuse has an influence on the practice of reuse*
8. *Repositories should support the measurement of reuse*
9. *There may be challenges to reuse in the context of decentralised non-collaboratory research efforts, where measures of quality and reputation are more difficult to ascertain.*

Measurements of reuse and community norms were not relevant at this prototypical stage of the Orson project. However, the possibility to publish research projects easily and allow interactions with them is supported in Orson through the use of web technologies for the publication of projects as web sites, facilitated by the provision of control of access and authoring rights for different user roles.
A future update should make project *contents* easily exportable, importable, and distributable, so that they can be published to established software repositories.

10. *Provide support for the navigation of uncertainty in order to clarify the utility of project data for reuse.*
Orson offers the means to compare revisions of a single piece of content or the differences between two separate pieces of content. The features inspired by the theme of reconfigurability which runs through the software are useful for considerations of alternative hypotheses. Such comparisons can also be annotated or discussed in pages which link to them.

8.5 Orson in context

It is instructive to explore how digital archaeology research projects with shared or similar goals to those of Orson have tackled the task of embodying theoretical ideals in their designs.

Opitz and Johnson (2016) describe their 'Interpretation at the Controller's Edge' project for the digital publication of archaeological excavations as a 'sociotechnical' one, after Dallas's suggestion that:

> archaeology appears to be once again at a crossroads, shaped by the interaction between its evolving theoretical and epistemological horizons, and the sociotechnical infrastructures informing its increasingly digital practices. (Dallas, 2015:77)

Their goal is to develop an archaeological interface which provides 'an accurate model of the dataset in question, but also interpretations generated throughout the excavation process' (op. cit., p4).[51] It should involve users:

> in the process of critiquing and creating meaning out of field data, helping to achieve a "slower" post-excavation experience by providing reflective, embodied engagement with the archaeological record via 3D representation. (Ibid)

They employ a 3D first-person graphical interface depicting the archaeological site for its facilitation of embodiment and because 'game-like interfaces can elicit a distinct form of proprioception and self-awareness among users', which is in turn 'beneficial to a slower approach during post-excavation' (ibid, p6). The authors also suggest that the interface can foster reflexivity, because 'increased self-awareness and non-linear exploration can encourage the "de-centering of the author" and involve users in the process of interpretation and critical examination' (ibid); but it is not clear that this should be the case, given the 'embodied' first-person approach.

[51] The web resource they discuss is available at: https://www-fulcrum-org.libproxy.ucl.ac.uk/epubs/bz60cw28v#/6/2[a01_Cover]!/4/4/1:0 (Accessed 08/09/2023)

As with Orson, there is a commitment to sharing the data resources used for interpretation alongside narratives about the research process and findings. Both platforms make use of the division of screen estate to show the connections between interactive visualisations, narratives and data resources. The Opitz and Johnson project uses the open-source software platform Fulcrum[52] to provide a high level of integration between its various resources. The project is highly engaging and is exemplary in its exposition of the relationship between research resources and interpretation. It differs in some key respects from the Orson project and its aspirations. First of all it is primarily a publication platform, rather than an authoring or co-creation resource. The means of interaction and visualisation are singular and fixed. Opitz and Johnson claim that 3D representations encourage 'more participation in the archaeological process […] allowing for a multiplicity of interpretations, explicitly operating on the analytical and abstracted level' (ibid, p7), but it seems that this work must take place offline, or off-site; there is no space for the publication of parallel interpretations on the site itself, and no opportunity to iterate on the findings presented in the interface itself. Orson, contrastingly, seeks to enable facilitation of the co-production of knowledge in shared online spaces.

The fixed, one-to-many publishing model of Opitz and Johnson's project, and the relatively high-tech nature of the presentation put limits on the kinds of interaction and engagement which are invited. In particular, in spite of its virtues, the 3D interface represents the surface level of a nested series of technological black boxes, which project technical authority. The in-depth understanding of these and their genesis is beyond the reach of non-specialised users, and the range of responses available to them is therefore proscribed[53]. Ironically, the goal of making the data reusable adds to the air of technological authority, abstraction and immutability: a notice at the start of the project narrative states the following:

> Electronic files
> This electronic text file was first created via XML encoding through a series of automated and manual processes, approximating the recommendations for Level 4

[52] https://www-fulcrum-org.libproxy.ucl.ac.uk/about/

[53] The project also depends on a specialised 3D rendering plugin component, which may be a negative factor for the longevity of the resource, due to the constant and rapid evolution of 3D web standards. More significantly, the volume of the 3D data used means that it is slow to load, even on a high-end system, and is likely to be inaccessible to many users with older equipment.

> of TEI Text Encoding in Libraries: Guidelines for Best Encoding Practices. It was later converted to EPUB 3.0.1 through a series of automated and manual processes.[54]

This statement provides confirmation that there is a general assumption of an expert audience. This is by no means necessarily a negative feature. But even for those specialist readers who are familiar with XML, TEI and EPUB, the dual 'series of processes' remain opaque—and there is a degree of tension between the previous emphasis on 'slow' archaeology and reflexivity, and the use of computer-mediated processes which strongly guide the interpretative approaches available for interaction with the source material.
In the Orson model, some perspective is brought to bear on the limits of any given mode of representation by allowing for alternative models, via the 'Context' plugin system. Orson also makes provision for fully or partially open co-authorship and iteration, though site creators are not obliged to implement these features. Like Fulcrum, the Orson system represents the iceberg peak of a sophisticated technical assemblage, but in its most basic level of authorship, the creation of Markdown text, there is less going on 'under the hood' than with sophisticated 3D visualisations (or even with standard HTML text editors, which typically interpose structured tags into authored text for the purpose of content formatting). On the other hand, Opitz and Johnson's discussion of the potential virtues of embodied representations raises questions about the centrality of textual and 2D visual content in Orson, and it should not be assumed that the use of 'plain' text and 2D imagery necessarily represents a more direct, unmediated path to essential truths. However, Orson does have the virtue of being open to different forms of expression, and allowing for reflection on form as well as content.

Another project which looks to foster innovative interactions with archaeological data is Dynamic Collections (Ekengren et al., 2021). This project is again centred on 3D representations of artefacts, but in this case the emphasis is on allowing those interacting with the platform to add their own annotations and curate their own collections from the shared body of records. This is contrasted by Ekengren et al. with online 3D archives which have a single-object browsing paradigm, such as Sketchfab (ibid, p.35). Project

[54] https://www-fulcrum-org.libproxy.ucl.ac.uk/epubs/bz60cw28v#/6/8[a04_Seriespage]!/4/2/2/1:0 (Accessed 08/09/2023)

administrators can publish their collections on the Dynamic Collections site itself, for teaching purposes, for example; but it is also possible for other users to make their own collections in a format which can be saved independently of the site, and later loaded into it for personalised visualisation and interaction as required. The users' collections thus exist as a preservable and exchangeable resource in an open and human-readable data format (JSON[55]). This is an innovative approach which like the Orson model, challenges the paradigm of a singular, authoritative interpretation of evidence. It would be closer to the goals of Orson if a range of users were able to publish their collections on the site itself, rather than circulating them independently to selected recipients. The creators of Dynamic Collections have aspirations which would bring the platform even closer to the Orson project:

> The plan for the near future is to expand this collection management to include more ways to enrich the structure and to work with the collected objects. We want to add the possibility of using the collected objects to create things like schemes, spatial arrangements, and graphs. These representations would truly transform a selection of objects into a reasoned collection. (Ibid, p349)

This is strongly reminiscent of the multi-Context paradigm in Orson, though Ekengren et al. seem content to retain 3D models as their 'base unit' of content. As with Opitz and Johnson's project, this makes the platform most suitable for discussions of well-formed, pre-existing research outcomes, rather than for setting out reflective material on the research process itself.

Cook gives an account of archaeological research projects which 'craft hybrid heritage', (§2.3.1 p53), based on the recognition that:

> the need to serve much more diverse audiences and act responsibly and ethically toward descendant communities often means that there is no one-size fits all solution for the production, presentation, or preservation of narratives of the past, but rather that many different media and formats, not to mention people, are required to produce accessible and inclusive archaeologies. (Ibid, p146)

[55] Javascript Object Notation

Her use of the notion of 'knowledge braiding' in relation to these processes has commonalities with Wylie's use of the 'cable' metaphor (see §7.6), as she acknowledges, but she uses it specifically to champion the interweaving of truths from 'diverse people and perspectives' (ibid, p145). She describes how the projects ran in-person co-creation events which made use of technology-based activities, including hybrid digital–analogue mapping and the curation of personalised 'playlists' from existing digital museum resources, in a process reminiscent of the custom 'collections' of the Dynamic Collections project. Technical detail about these resources and activities is sparse in Cook's account, but the implication of the report is that existing resources can be deployed in innovative ways, even when there is very limited funding involved (ibid, p149). In spite of working within limits of time and budget, positive outcomes were reported in which great value was put on 'the way in which the process of doing collaborative digital archaeology was as meaningful as the digital products themselves' (ibid, p154). The collaborative practices were useful for prompting reflection on the motivations for digitisation and the ethics of such practices in museums and beyond:

> The process of cocreating hybrid platforms and applications for participatory and meaningful interaction with the past not only shifts authority in decision-making processes to give descendant communities in particular a voice in determining appropriate digital strategies, but it also creates a series of checks and balances to ensure respectful and responsible practice. (Ibid, p157)

The difficult question of how to make such practices sustainable is addressed by Cook in her suggestion that the 'ongoing and targeted development of technology and platforms will open up new avenues and opportunities that will shape future directions in public archaeology' (ibid, p160); and by her emphatic declaration that a truly collaborative practice cannot be an 'exercise in political correctness or placation in digital and public archaeologies'—it must represent a 'complete upheaval' of existing frameworks which were designed to 'restrict, exclude and maintain control' (ibid).

I take this conclusion as a validation of the approach adopted with the Orson project, in which there is a non-hierarchical system of varied digital representations. Highly technical models of representation such as 3D graphics and game engines can be engaging and

facilitate insight; but there is the danger that they present work which is already so well-formed that it cannot easily be challenged. As Latour and Woolgar point out, the results produced in a technologically sophisticated laboratory can only be properly challenged by those with access to the same or better equipment (Latour and Woolgar, 2013). This raises the stakes for making challenges to knowledge claims significantly. Ideally, a platform striving for inclusivity should offer low-stakes opportunities both for *alternative* perspectives and modes of representation, and the means of gaining access to research narratives early in the process, before opaque technical interventions take effect. As in the projects described by Cook, there should be scope for reflection on and questioning of motivations and ethics as well as outcomes. For this reason, the curation of personalised collections of existing digital museum resources or 3D objects seems to be only a partial step towards a more collaborative style of knowledge-making, one which might, in the wrong context, flirt with the danger of being placatory. The Orson project attempts to address this potential problem by encouraging the documentation of process and facilitating comparison, reinterpretation and iteration.

Cook identifies a 'growing community of archaeologists at the intersections of digital and public practice' (see §2.3.1, p53). The shared imperatives of some of the projects cited here show that the Orson project has much in common with other Digital Archaeology initiatives. The examples also demonstrate that that there will be no 'one size fits all' solution to satisfy the requirements of all research projects. Orson should be seen as one contributor among various others which are pushing together to open doors to alternative research practices. The future development of the Orson project can take inspiration from Opitz and Johnson's discussion of embodiment in representations, from Ekengren et al.'s model of decentralised collection curation, and from Cook's description of hybrid in-person uses of technology, for example. The principles underlying the Orson project imply that there can be no definitive set of tools for making representations, but that those which provide opportunities for reflexivity and alternative perspectives are more likely to fulfil Cook's hope for the opening up of 'new avenues and opportunities that will shape future directions in public archaeology' (ibid, p160) via targeted technological development. Arguably, the application of the notion of Epistemological Modesty as a design principle for software and research can allow for the implications of the prioritisation of transparency, reflexivity and multivocality to be taken to their logical practical conclusions.

8.6 Interview and feedback sessions

Over the course of a month, I interviewed a series of students and researchers to solicit feedback and discussion about Orson and its potential relationship to their research processes (see §3.2.1, §3.2.2). These were in the form of one-to-one video-conference interview sessions which lasted approximately 60 minutes each. The interviews included screen time spent exploring tasks using the Orson application.

To break the ice, and gain some insight into their academic specialisms, I invited interviewees to start off by outlining their area of study and previous experience. They were then asked if they saw themselves as being part of a particular research community, and if their subject area required any particular technical skills or knowledge. These questions were useful for filling out the context of their research domain and level of experience.

I then shared my screen and gave an overview of the main features of the Orson software, its basic architecture, and its possible uses. This was done using an example project I had previously created—a conference paper with supporting materials—in a private web-based instance of Orson.

After this introduction I asked the interviewee to log into the same instance of Orson and to share their computer screen with me, thus allowing me to capture their interactions with the software in my recordings.

Using an example Orson project based on an existing Historic England archaeological archive (English Heritage, 2014), participants were asked to try to work out how to perform some basic tasks in the software: finding resources, copying resources and making simple edits to these copies. In one respect these tests were extremely useful for understanding the software's qualities of usability and discoverability. However these activities were also designed to help frame later discussions about scenarios for documenting and publishing research, by providing concrete examples of how digital resources might be annotated or copied and edited by different contributors. This discussion of hypothetical scenarios,

bolstered by the use of tangible examples, was the best option available in lieu of collecting data from an actual software trial.

8.6.1 Research questions

Orson's design goals are to manifest the ideals of epistemological modesty, to support transparency, the exploration of uncertainty and the reuse and reinterpretation of research results. The interview questions were designed to ascertain whether the software had clear affordances in these areas. They were devoted to the discussion of research practices and scenarios for collective work or the reuse of research results, including conditions for sharing, publishing and allowing contributions by other researchers; and how the software features listed above related to these.

Certain research questions were included with the aim of testing existing findings in the literature related to the reuse of data, to consider their significance for the design of software tools. Faniel et al. (2019) discuss the contextual information which is important for researchers looking to reuse others' research outputs. Their study suggests that the most significant of these is the detail of how the data was collected. Beyond the data collection process and information about the evidence itself, the next most significant factor for potential reusers is an assessment of the original researcher and the institution carrying out the research. Based on this finding, I included questions about the project summary page and the researcher profile pages in Orson. What kind of information would be useful on these pages to help viewers judge the quality and trustworthiness of projects, their results and their research team? A full list of the questions is available in Appendix 4.

8.6.2 Evolution of the interview questions

During the first three interviews, some usability issues were consistently apparent with Orson. In particular the main Project Page, which summarises the documented project, was not easy to find, and the wording of menu options to create duplicates of individual resources was unclear. The software was updated to rectify these issues. The main Project Page was set as the 'home page' in the system, removing the need for a question about navigating to the Project Page, and the question was deleted from the interview script. As a

result, the software's usability was improved and it became possible to devote more time in the interviews to the discussion of research practice.

8.6.3 Coding and analysis

In the coding process, I used Maxwell's distinction between 3 category types (2012:112) as a structuring principle:

1. Organisational categories. Broad areas or issues that are often established prior to data collection.
2. Substantive categories. Descriptive categories which include descriptions of participants' concepts and beliefs; they 'stay close to the data categorized and don't inherently imply a more abstract theory' (ibid).
3. Theoretical categories. These place the coded data into a 'more general or abstract framework' (ibid, p113). They often represent the researcher's concepts.

596 'substantive' and 599 'theoretical' codes were assigned to sections of text in the interview transcriptions, and these in turn were arranged into 5 main 'organisational' groups (non-exclusively):

- Innovation and technology (153 codes)
- Research procedures (356 codes)
- Knowledge and knowing (291 codes)
- Interdisciplinary work and collaboration (459 codes)
- Presentation and knowledge sharing (416 codes)

The creation of codes allowed for the detection of patterns of similarity throughout the material, and code co-occurrences were studied using code matrix visualisations (see Figure 15). 'Network' views in Atlas.ti were created using 'substantive' codes to explore connectedness and causality in single narrative accounts. These views were also labelled with 'theoretical' codes, to set the researcher's themes alongside the emic perspectives of participants. A discussion of the main themes identified in the interview material follows.

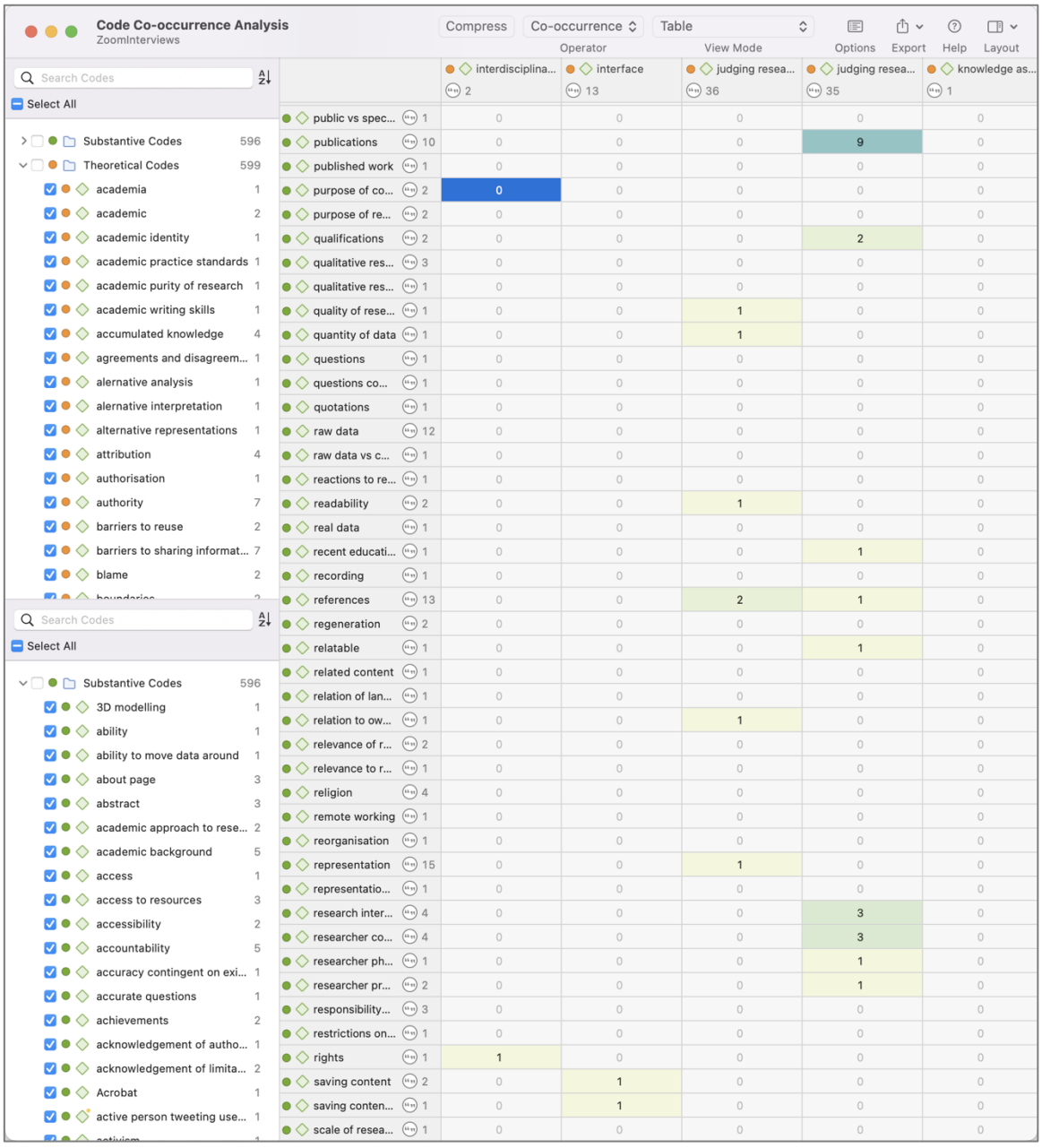


Figure 15. An example of code co-occurrence analysis in Atlas.ti.

8.6.4 Themes in the interviews

1. Overall impressions of software

The general responses to the software were extremely positive. Interviewees had to be prompted for negative aspects and few of these were forthcoming. This balance might be partly explained by the assumption, made by most participants, that the software was authored by the interviewer, though this was not explicitly stated unless asked. As a

consequence of this participants may have felt a diplomatic need to be positive, or have felt other pressures related to the inequity of the interviewer/interviewee relationship (Robson, 2016:172). Another potential reason for the popularity of the software was the time-limited nature of the tasks undertaken in the session. In some cases participants reacted positively to features in the software which they did not have time to explore in detail, for example the 'Comparison' Context type. It was apparent in such cases that they tended to assume that the feature would fully meet their own personal requirements. By way of contrast, one participant had access to a test instance of the software before the interview and had gone exhaustively through the features in her own time. She was therefore able to point out various bugs and deficits in the system, though this did not diminish her enthusiasm for the platform. Her experience suggests that those working with the software in earnest would be more likely to identify problems with usability, feature set or bugs.

Aside from these caveats, the positive responses to the software and its potential implied that participants could easily imagine themselves using Orson, and that it had relevance to their research needs. The potential value of the software's feature set and its 'look and feel' were highly appreciated by participants. This was an endorsement of the basic design decisions taken in developing the software, and implied that the architectural paradigm of Orson, in particular the Cards-Contexts-Pages structure and the support for multi-authorship and multi-perspectivalism, were quite straightforward for users to grasp, and that the software held some appeal for use in their own work.

2. Attribution and credit

A useful outcome of the interviews was insight into the multifaceted nature of research work and knowledge construction. In the literature about research reuse, the main preoccupation is data-oriented practices and data commensurability. These interviews, however, helped to illustrate that the development of the professional academic self should not be considered as an activity separate from the management of research resources. This insight emerged through the prominence in discussions of the issues of attribution and credit for research, ownership of research, and the conventions of collaboration.

Interviewees stressed the importance to them of getting credit for their work and the need for proper attribution when citing other's work, as a way of 'showing them honour', as one interviewee put it. Because there is a high degree of intertextuality in academic writing, it is

necessary to establish and observe clear boundaries of authorship. Publication of appropriate credits is both a way of allaying concerns about just and unjust rewards and of sustaining the ideal of research credibility. In my claims elsewhere that the construction of knowledge claims is as much about the creation of a coherent and authoritative knower as it is about the establishment of facts, my intention was to highlight and emphasise the situated nature of knowledge; but the interviews informed my formulation of 'epistemological modesty' by helping to show that 'knower construction' can equally be understood in terms of academic credibility and expertise in a domain. Importantly, the credibility of witnesses, as Shapin and Schaffer note in their study of Reformation period science, is a matter not just of authority, but of moral standing:

> The natural philosopher had no option but to rely for a substantial part of his knowledge on the testimony of witnesses; and, in assessing that testimony, he (no less than judge or jury) had to determine their credibility. This necessarily involved their moral constitution as well as their knowledgeability, 'for the two grand requisites, of a witness [are] the knowledge he has of the things he delivers, and his faithfulness in truly delivering what he knows.' (2011:58)

This helps to explain the concerns interviewees had about distinguishing between contributions in Orson or other projects: these were not related solely to a desire to accrue credit and authority; they were also about being seen to be academically *ethical*, a quality as important for academic credibility as expertise is.[56]

3. Ownership of research

In the interviews participants were asked whether they would be comfortable with directly editing the content of an Orson project created by another researcher, in the event that they had access and permissions to do so. This question was asked after a hands-on session in which they were shown how to duplicate a resource (an interactive map) and make an edit to it; and after a demonstration of how author credits are displayed in every piece of

[56] Arguably, the researcher who speaks from a recognised position of authority has more freedom to propose novel hypotheses or challenge orthodoxy—they are freer from the concerns of avoiding error and providing guarantees of their ethical qualities. This was summed up by interviewee 6 in study 1, who noted that she 'was not senior enough to publish her musings' on a topic.

Orson content. Overall, there was a strong reluctance to contribute to this hypothetical project, primarily because of the perception that it 'belonged to' someone else, it was 'their' research (the original author's) and by implication an expression of their 'voice'. This was not only an expression of concern for the original creator's authorship rights: interviewees were also worried about being responsible for introducing mistakes or errors into another author's work, a prospect which seemed potentially more damaging to them even than presenting a flawed piece of work under their own name. This served, again, to highlight the important *ethical* dimension of research work and academic writing. The code 'responsibility' was one which was applied frequently in relation to this topic. While pressures on collaborative work no doubt exist widely, they may have been particularly conspicuous here because the majority of participants were students, and they were often worried both about their competence in producing research to a high standard, and proving that they were ethical actors.

Interviewees were then reminded of the capability in Orson to copy resources and edit those copies, and were invited to consider scenarios where the authorship of resources and any edits to resources were clearly recorded in the software platform. There was general agreement that this was a more acceptable scenario for making contributions to the project. Some participants stated a preference for what I would call a 'closed-open' model of collaboration, that is, allowing contributions from an identified set of authors, but retaining editorial control over the publication of individual resources.

The strength of feeling about the ownership of research seemed to be a likely barrier to diverse or spontaneous contributions to a public Orson project; but some types of contribution did seem to be welcomed more than others. Various interviewees spoke positively about the potential of comments or annotations to either help them catch errors in their work or expose them to other perspectives. There was a sense that such contributions could be useful, without detracting from the integrity of the authorship of the original piece.

4. Collaborative work

When analysing the interviews there was a significant co-occurrence in the code group 'positive aspects of software' with codes related to collaboration, collective work or knowledge sharing. The adjectives 'useful' and 'helpful' were the ones most often used in

this context. This demonstrated a consensus around the notion that collaboration and collective work are positive ideals. At the same time the need for the imposition of conditions on such work, such as the preservation of clear marks of authorship, and limits to access, was also stated. I suggest that the notes of dissonance which appear here (collectiveness, openness and sharing vs a bias towards individualism and limited access) are a result of a dual research ethics which aligns with the two functions of research work that I have previously discussed: knowledge construction and knower construction. The former is founded on the ideal of objectivity in the processing of evidence, and finds legitimation in the enrolment of witnesses in the community and the establishment of consensus. The latter is usually only tacitly acknowledged as a goal, because its priorities and often agonistic nature can at times contradict the ideal of fully disinterested objectivity. Having access to make contributions is generally only granted to other qualified 'knowers' who are recognised members of a thought community. In an ideal world of unbiased or neutral research, a specialist from a scientific field may have a valid contribution to make to a social science study, and vice versa. In practice, outside of formally constituted interdisciplinary projects, such contributions are unlikely to be considered, as they do not come from established members of the thought community; and the thought community's boundaries are guarded, because it makes its *members* as much as it makes *knowledge*.

On a practical level, the feedback sessions helped bring context to the theoretical goal of inviting user interventions on existing content, for reinterpretation and annotation. They revealed a contradiction with some previous design decisions in the software, where the priorities were readability, discoverability, and simplicity. This had resulted in a clean interface, but the interviewees' interactions showed that the opportunities to transform content were not always front and centre, and their use was thus not invited. This prompted the question of how to find a balance between readability and the malleability required for the iteration of content, and resulted in updates to the software interface after the interviews were completed, to make opportunities for interactions more obvious.

5. Established research practices

It became noticeable in the coding of the interview material that researchers gravitated towards established research practices and publication models, and tried to make sense of

Orson in terms of these. This 'apprenticeship model' was probably prominent because almost all of the participants were students or recent graduates, and the prioritisation of the development of academic competence was potentially as important to them as the development of original knowledge claims. The common perception of Orson as a tool primarily for *organising data* suggested that the imagined research ends were already well established, and so the software was seen mainly as a means of smoothing the path towards producing a piece of academic writing, for example. Novel models of collaboration or publishing were discussed with interest and often enthusiasm, but discussions of these themes were wholly driven by the interviewer and were not a priority for interviewees. There was openness towards the ideals of EM and multi-perspectival knowledge, and acknowledgement of the positive benefits which might become available through their practice. On the other hand, there was a reluctance to contribute to 'other people's' projects, and a tendency to want to follow convention in order to demonstrate competence. The implication was that for alternative research practices to gain a foothold, there should be clarity about the terms of publishing and contributing, and that the rewards for new modes of working should be tangible.

6. Organisation of data

While Orson is designed to be a software application which supports not just the collection and organisation of evidence, but the testing of different hypothetical scenarios and the writing up and publishing of projects, one of the most common responses in the interactive sessions with the software was to note its potential as an organisational tool. Various interviewees talked about their difficulties in managing resources and about the ideal of 'having everything in one place'. Orson was seen by them as a possible candidate for fulfilling this ideal. This highlighted the point that for the academic, the pool of knowledge they are required to access to contextualise their own work is typically spread widely across the research community, in a diverse range of formats, and the act of curating or filtering it can be an unwieldy process. The ideal of collecting all resources into one place is probably approached most closely by a dedicated referencing tool such as Zotero[57], though such an application has limited provision for custom representations or interactions.

[57] https://www.zotero.org/

When a software package has diverse features and supports different phases of a process, as Orson does, this often puts it into competition with 'best of breed' specialised software packages which are more narrowly focused. This is partly addressed in the software by the use of a plugin architecture. The Timeline Context uses a mature third-party code library for drawing and interacting with timelines, for example. In other cases it may be more pragmatic to build 'connectedness' into the research software, allowing for the frictionless passage of content interactions between it and other network nodes, such as a web-based library of references accessible via the Zotero API. This, more so than content provision alone, would make it a good fit for a 'pooled knowledge' process which exists in a world of continuous innovation with regards to technological representations, networking technologies and research paradigms. It would also recognize, as previously mentioned, that a piece of research such as a journal article is more than the text and illustrations provided: it is given full meaning by epistemological assumptions and expectations which underlie it and surround its interpretation: and each researcher sees this 'neighbourhood' and its boundaries from a different vantage point. Interviewee feedback was therefore a useful pointer for future development in the area of interconnectivity with other resources.
A broader lesson from this strand of feedback was that the most obvious affordances of Orson were for collecting and managing resources. The possibilities for sharing a research journal, publishing interactive research narratives or reinterpreting research resources were less apparent.

7. Interaction and 'README' pages

I wanted to explore which software features or other inherent qualities might encourage Orson users to feel empowered to contribute to a published Orson project, presuming that they had access and editing rights to do so. The goal was to present published projects as entities which are to be actively *iterated upon* by the person interacting with them, rather than just consumed. This principle was extended throughout the platform, with the highly visible and browsable nature of all resources, and the attempt to make the potential for user-driven transformations of existing content an obvious and appealing prospect. As part of this goal, I borrowed terminology from the world of software development, and the archives of software source-code. These projects often invite active interventions from users, as in the case of open-source software packages, for example. This might consist of a

procedure to install the software, to configure or customize it, or to contribute code to it. As such projects often consist of a bundle of disparate files, the entry point for interactions is usually signalled by having a text file named ‘README’, which gives project information and sets out possibilities for engagement (see Figure 16).

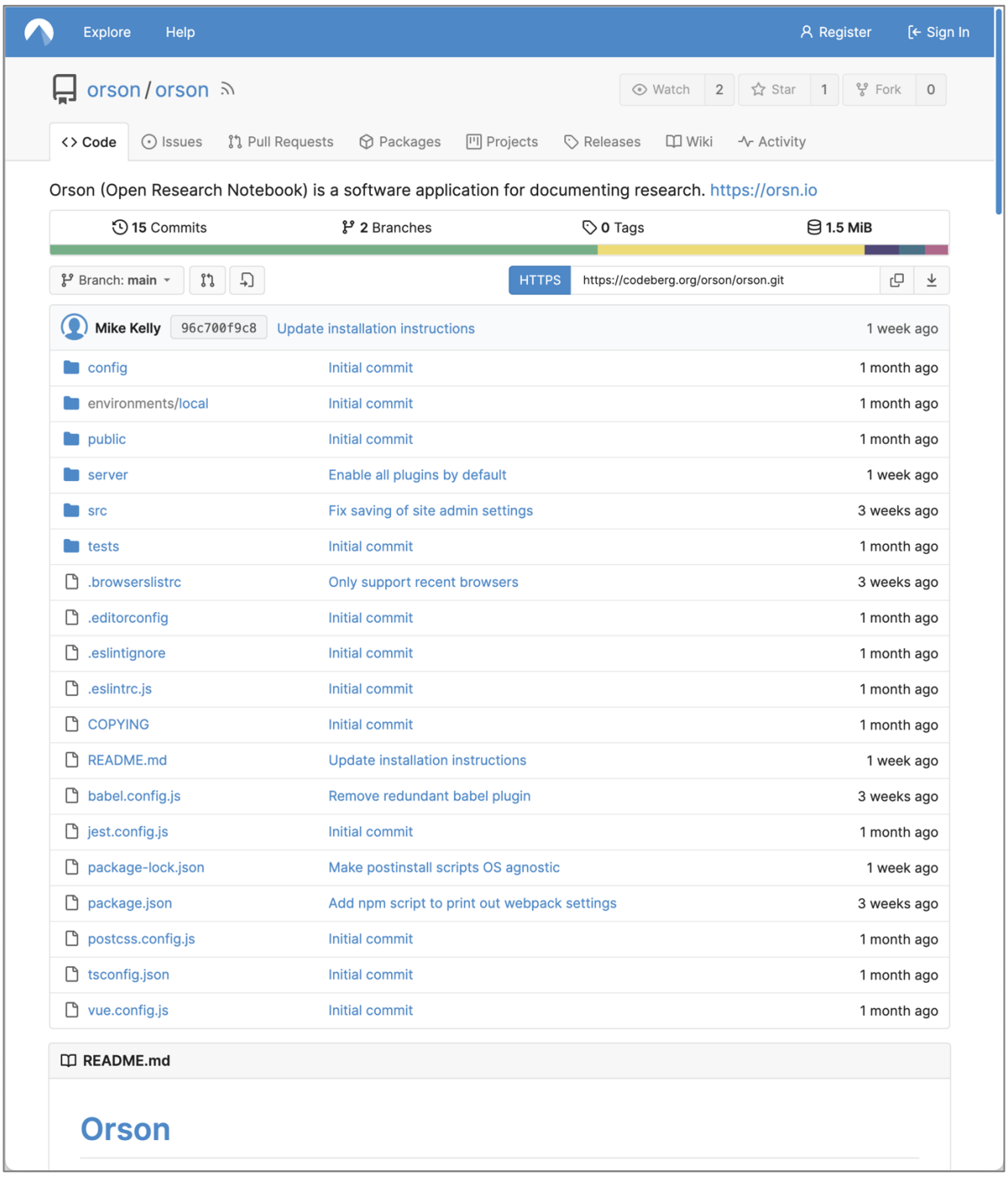


Figure 16. Orson project. An example of the presentation of an open-source software package in a repository, with a README file.

This imperative terminology implies the existence of processes waiting to be carried out, of potentials waiting to be fulfilled. There is something of the delicious allure of the cake in *Alice's Adventures In Wonderland*, inscribed with the instruction 'EAT ME'—the promise of a transformation or reward, or more prosaically, but still compellingly, an unfinished thread waiting to be tied off. This framing actively invites interaction, completion and in some cases further iteration. I used the README term for the title of the main project page (see Figure 17) and asked interviewees if they had any associations with it, and what they would expect to see in a README file.

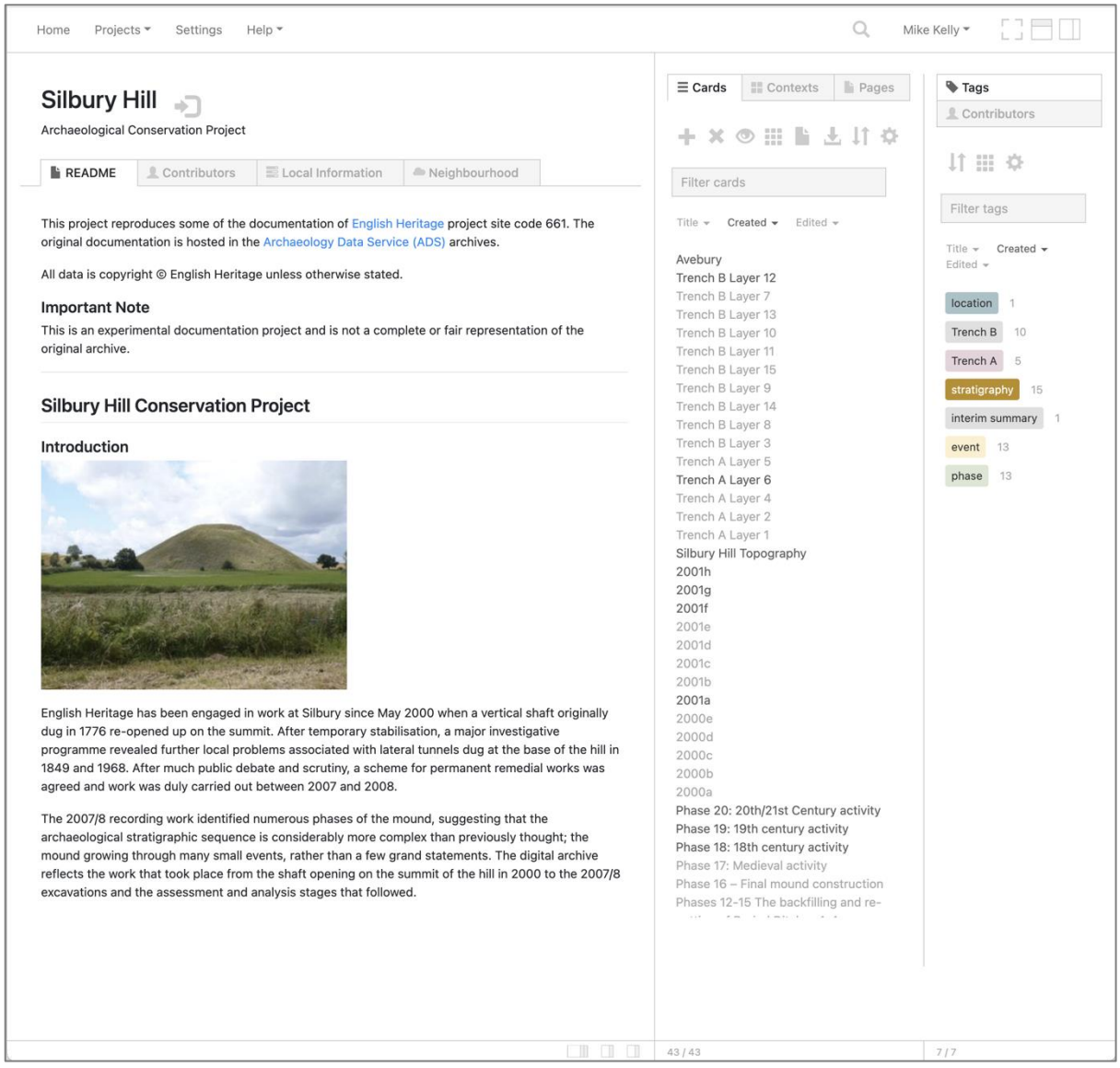


Figure 17. Test project in Orson, featuring README file home page.

Many interviewees simply reflected back the content of the example README page they were viewing, commenting that the term brought expectations of a project summary or overview. However, three of the participants noted the transitive intention of the word, that it was an 'instruction' and that, as one put it, it was 'intriguing' to find out what was required of them. Another pointed out the sense of urgency conveyed. None of the participants were familiar with the convention of the term's use in software archives, so this framing was novel to them. The feedback suggested that there was some potential in further exploring the encouragement of interactive modes of engagement.

The README page was also the ‘home page’ for an Orson project, and thus the site at which first impressions were formed of a piece of documented research. For the interviewees, keywords, and clarity in statement of research goals and results were the most important factors for helping them to judge projects by their README pages. References to the literature were cited by some interviewees as being important, not just to site the current project in the context of other work, but because citations were seen as useful sources for additional reading. This is a useful insight. It shows how pieces of research are not always read purely on their own merits, and are not necessarily read from start to finish. They can be seen as part of a network of ideas, a body of knowledge which gains authority from the weight of its accumulated parts. The list of citations can function as an overview of that body, and a roadmap for exploring some of its territories. Judgement of the project may therefore relate to the company it chooses to keep, and its allegiances to other pieces of work. This in turn raised interesting questions about the ‘glue’ which binds different pieces of research work together. Which identifiers are used to mark out projects or articles which use a particular research approach? In these interviews, it was the use of analytical approaches indicating a certain theoretical background, and these were identified by keywords or phrases—hence the frequent mentions by participants that they would look for a list of keywords on the README page, an established feature in the publishing of academic articles, researcher profiles, etc. As an example, one interviewee identified the defining characteristics of her chosen theoretical research approach as the concepts of ‘affect and embodiment’.

It was noticeable that the interviewees often cited the date of the research as an important criterion for assessing the relevance of the project, with the implication, explicitly stated in

some cases, that the more recent the research was, the more relevant it was. This may have been beyond dispute in some cases where for example the object of study was related to recent events, such as a government's policy on heritage conservation. Aside from such cases, the frequent references to research date, together with the reliance on theory-related keywords, reinforced the impression that those interviewed had a perception of research movements, or moments, which they looked to site their own work in the context of; and that awareness of, and membership of, current 'movements', was of importance to them.

In most cases, in discussions of an ideal README page, there was no mention of institution, or research *methods*, until prompted by the interviewer. This emphasis could be understood as an echo of Faniel et al.'s findings, which show that:

> data reuse is a process that includes several other stages, including discovery, access, selection, preparation and analysis […]. Different types of context may be important at different points in the process. (2019:1293)

Once a commitment to reuse has been made, details of research methods, metadata and institutional provenance are likely to become more important.

8. Profile pages

When looking at a researcher's profile page in Orson with a view to judging the researcher and making an assessment of their authority and trustworthiness, interviewees were most interested in seeing content related to the researcher's research interests, research theme keywords, and links to previous projects or publications. There was an interest in academic background, achievements and awards, and some mentions of institutional affinities. Many interviewees mentioned that they would like to have access to the researcher's email address or other contact details. Some also mentioned the possibility of including a profile picture, though they were careful to state ambivalence about this option.

The main point of interest was in getting a sense of the field that the researcher was working in, and the research themes which they were preoccupied with. This was invariably mentioned before considerations of previous publications or other achievements. There

was a strong sense that interviewees wanted to assess whether the researcher shared their preoccupations, was in their 'tribe', and spoke the same research 'language' as them. This again may be a consequence of the phased nature of commitment to reuse, which one might expect to begin with a 'ballpark' estimation of relevance. However, it was interesting to note that as with the project page, the information on the profile page was not always considered on its own merits, but was instead scanned for a sense of its *instrumentality*. To what extent was it useful to the interviewee? Was the author working in 'their' area, and could the findings be added as an ingredient to their own research 'cooking pot'? In the same way that a list of citations was mentioned as a useful roadmap for accessing other resources, having contact details or social media addresses on the profile page was seen as useful because 'they might be an active person tweeting useful things.' Examples like this again demonstrate how interview participants tended to have a conception of a broader research community and considered individuals and individual projects against this backdrop. Those researchers and their outputs were viewed as pathways into that community (to knowing it and potentially becoming part of it) as well as independent sources of knowledge. This was a peer-to-peer relationship: to pursue the cooking metaphor, the participants saw themselves not as customers in a restaurant, having a dish put before them for consumption; but as chefs looking over the shoulders of colleagues to learn from their recipes and culinary techniques.

The priorities in assessing projects and researchers can be seen as reflections of interviewees' aspirations—they were looking to orient themselves in their respective fields, as much as to contribute original work to it—but even for established professionals, orientation of the self in the broader research community and canon of literature is an important, ongoing concern. I suggest that the instrumentalisation of published project accounts and researcher profiles should therefore be facilitated in a tool like Orson, in order to help with the construction of knowledge claims, but also to help with the construction of knowledge *communities*, and the academic identities of individuals within them. The interviews illustrated that the individual research paper does not begin with the abstract and end with the conclusion. Inevitably it exists intertextually in a wider network of ideas, publications and people. The interest expressed by interviewees in having researcher profile pictures can be more easily understood in the context of knowledge communities. Perhaps

the idea of a research 'family' or community becomes a more tangible concept when there are images of participants to engage with. Viewers might ask themselves, even if subconsciously, ‘do they look like me? Do I fit into this group?’ or perhaps, ‘can I imagine seeing myself there?’ Those who mentioned the possibility of having researcher pictures on the Orson profile page immediately qualified this by saying they were ‘unsure how useful it would be’, apparently self-conscious that the notion of a researcher photograph might appear trivial or unserious. This was an indicator of how the ideal of academic research is to efface subjectivity, as a suspected enemy of impartiality and unclouded judgement. The convention of the profile picture—most commonly a head and shoulders picture of the author, making eye-contact with the viewer—is an indirect reminder that the accompanying data and narratives are situated, and that the sharing of knowledge is a social act.

8.7 Discussion

As a consequence of its inclusion of traces of authorship and accountability, and profiles of contributors with research interests and allegiances, engagement with a tool like Orson has the potential to be markedly different from that with a tool in which authorship is not foregrounded. The interviewees in this study analysed project summaries and researcher profiles partly from the perspective of instrumentalising these for their own work, suggesting that Orson could be a tool for contributing to the making or sustenance of a thought community, as well as a tool for documenting single projects. In one respect this was a positive outcome for the Orson project—the prospect that it might function as a node in a network of researchers, enabling exchange. However it was also clear that there were barriers to some of Orson’s other ambitions. When assessing content in Orson, interviewees were primarily interested in finding work which was a good fit for their academic interests and preferred research methodologies. This focus seemed likely to override any consideration of projects which were *not* in all respects an obvious fit. The implication was that the *reinterpretation* of project results was more likely to take place, if at all, when driven by formal external processes, rather than happening ‘naturally’ as a consequence of the affordances of the software and the availability of editable research outputs. The test

sessions were not the ideal scenario in which to test this hypothesis: ideally it would be revisited at a point when it was easy to publish and exchange Orson projects online, and public projects were available to be edited or repurposed. It also remains to be seen if additional development of the software's affordances for reinterpretation would have a further bearing on this, or for example, the foregrounding of the narrative-making functionality of the software.

Based on the criteria listed in the themes from the literature review on reusability of research, Orson is well suited to support reuse: it provides opportunities to establish trust in contributors, process and data by allowing for detailed documentation of these aspects. But interviewees were mostly non-committal about reusing the results of other projects, for example to augment data which they had recovered as part of their own research (a clear distinction was made between *reuse* and the more common act of *referencing*). This was not a possibility most of them had considered, mainly because their fields of study were not well suited to the use of standardised data descriptors. An exception to this was the case of those specialising in biochemistry. One biochemist interviewee was familiar with the use of data repositories for protein types, while another noted that there were issues with inconsistent terminologies for some protein molecules being used interchangeably in the field, and with inconsistent experimental conditions, and that these were barriers to reuse. The interviews were therefore inconclusive on this issue.

One theme from the literature review on reusability suggests that reuse may be less likely to occur where research outputs are not published in a centralised repository, because measures of quality and reputation may be more difficult to ascertain (see Appendix 3). This is potentially problematic for a project like Orson, which as part of the goal of multivocality, is intended to enable decentralised publishing models and the 'agile' creation of project documentation instances. On the other hand, there would be nothing to stop *exported* Orson projects being published to a central repository. This consideration, along with the concerns expressed by interviewees about the 'ownership' of research and their inhibitions in contributing to other people's work, implies that it would be beneficial to create a clear separation between Orson *instances*, for example an Orson-based web site with a domain name, and Orson *projects*, data bundles which could be copied between Orson instances (web site, desktop software, mobile phone app), or stored in a repository like GitHub. In theory, importing a project created by other authors into a personally

'owned' instance could lessen inhibitions in reusing or reinterpreting that content. To test this, the inclusion in Orson's interface of a clear invitation to 'fork'[58] a project would also be required. A straightforward mechanism for importing and exporting projects does not exist at the time of writing, so this is a topic for future research. It may also be possible in future for independent instances of Orson to act as satellites to institutions or other repositories, if a reliable identifier for contributors such as an ORCID id[59] or institutional email is used.

The interview and feedback sessions related to Orson were a strong illustration of the relevance of the ANT approach to understanding technological initiatives and the creation of knowledge claims. The most striking aspect of the interactions was that the academics did not consider Orson projects solely on their own merits: they wanted to know where those projects and their authors fitted into their own spheres of research, and whether they could be used as entry points to discover material which was useful to them. This showed that Orson should be understood as an actor in a research *ecosystem*, as well as a tool or medium for documenting individual research projects; and that research papers or digital research projects should not be considered as self-contained producers of meaning. This underlines the EM approach's emphasis on the situated nature of knowledge, thought communities and the genealogy of thought-styles.

The emphasis in the literature on standardisation for reuse is a poor fit for the philosophy underlying Orson's design. One of the goals of Orson is to retain the situated aspects of knowledge and avoid the tendency to present data as standalone bearers of meaning. There is an echo, therefore, of the conflict reported in §7.3 between transparency and reusability: the more the contingent and localised aspects of a set of results can be shared, the less likely it is to meet with an ideal of standardisation. I conclude that if a tool which aspires to be 'epistemologically modest' is to be used to encourage the reuse of research results, this reuse will have to take a different shape to that typically described in the literature related to 'big science' or 'big data', where the data 'may be great in volume but usually are

[58] This is terminology from code versioning in software management. A 'fork' is a copy of a code repository which can be independently developed to incorporate different features or priorities from the original—a metaphorical fork in the path of a software project.
[59] A unique identifier for researchers. https://orcid.org/

consistent in structure' (Suhr et al., 2020:4). One way to reconcile a wish to make research outputs reusable with a desire for transparency and accountability to context might be to explore the uses of a tool like Orson as a 'boundary object' (Star, 1989). This concept describes the case where an object can function as a nexus for different interests, allowing them to be represented or referred to in the same space without the need for full concordance in goals and understanding. Boundary objects are:

> both plastic enough to adapt to local needs and constraints of the several parties employing them, yet robust enough to maintain a common identity across sites. They are weakly structured in common use, and become strongly structured in individual-site use. (Ibid, p46)

Star describes how the different types of boundary object are dependent on the '*characteristics of the heterogenous information being joined to create them*' (ibid). The types she sets out are:

1. Repositories
2. Ideal Type or Platonic Object
3. Terrain with Coincident Boundaries
4. Forms and labels

What is of particular interest here is the handling of local peculiarities and uncertainties across these categories. In some cases, heterogeneity is a feature: for example in (1) repositories, and in the case of (3) shared terrain, where boundaries are the same but content and data aggregation are different. In the case of ideal types and forms and labels, however, local contingencies are deleted. (Star identifies category (4), 'Forms and labels', with Latour's notion of 'immutable mobiles', standardised forms which are useful for conveying unchanging information across widely differing contexts.) The typical ideals of the literature on the reuse of 'scientific data' are aligned with the qualities of boundary object types (2) and (4), in which data commonalities are emphasised at the expense of local contingencies, what has been described as the 'deracination of facts' (Lucas, 2019:28). The ideals of EM and Orson are better represented by categories (1), and in particular (3), where heterogeneity is not minimised, and the situated aspects of information are not discarded.

The example Star gives of shared terrain is of maps of California created by parties with different interests, namely conservationists and biologists. The maps share the same boundaries but their contents are different (campsites and trails vs biological 'lifezones'). The dynamic is reminiscent of Chapman and Wylie's discussions of archaeological research in terms of Galison's 'trading zone', and the appropriate community conventions to allow for constructive interdisciplinary exchanges (§7.5.3). A tool with EM affordances might constitute a digital 'boundary object' in which overlapping interests can be represented and developed, without the necessity for filtering out local particularities and provenances. Such a tool might be useful component of a research ecosystem which has the characteristics of a successful trading zone. For Star, problem-solving using boundary objects:

> produces workable solutions that are not, in Simon's terms, well-structured. Rather, they are ill-structured: they are inconsistent, ambiguous, and often illogical. Yet, they are functional and serve to solve many tough problems in distributed artificial intelligence. (1989:51)

There is an acceptance in the EM approach that epistemological and ontological understandings are at times messy, mutually inconsistent, and incomplete. For research reuse to be a plausible proposition alongside the EM approach, a space is required in which the characteristics of situated perspectives can be explored alongside shared concerns. A tool like Orson would be a good candidate for productively bringing together a range of perspectives on a topic with shared boundaries, as in Star's Californian maps example; more so than other cultural heritage tools which are designed to produce definitive representations of landscapes or other objects, and in so doing, tend to minimise the conditions of data collection and interpretation. The difficulty may be, as the interviews demonstrate, in persuading researchers of the relevance of perspectives which do not 'speak the same language' as their own, and cannot be summarised with the same set of keywords.

9. Conclusion

> I think that Feyerabend is right in suggesting that until we discard the metaphor of inquiry, and human activity generally, as converging rather than proliferating, as becoming more unified rather than more diverse, we shall never be free of the motives which once led us to posit gods. (Rorty, 2011:373)

In this thesis I have highlighted the symbiotic relationship between digital tools and representational forms in archaeology and the discipline's philosophical research goals. I have noted that representational forms can function as a means of managing complexity, and of stabilising knowledge and the processes of knowledge construction, in the face of 'troublesome knowledge' which has the potential to undermine the idea of a determinate and therefore fully knowable metaphysics. This process of stabilisation is not always a good fit with the ideal of transparency in research, and in particular with the subjective aspects of interpretation. To add to this broad background in addressing the key questions which I set out at the start of this project, it is necessary to bring in the context of the full range of themes I have discussed throughout this thesis.

The Computing Turn

The interviews, observations and articles I discussed in studies 1, 2 and 3 showed a preoccupation with the instrumentalisation of data, one which I take to be a characteristic of a 'computerised culture'—what might be called a 'Computing Turn'. This was particularly noticeable in the observations and articles, with their common theme of the use of remote sensing technology in archaeology. I have characterised the mainstream of the discipline as being 'techno-optimist' in its outlook. There is a perception that technological methods are the key to efficiencies but also to possible knowledge 'breakthroughs'. One consequence of the technological emphasis in archaeology since New Archaeology has been the development of various technical specialisms in the discipline. This may have helped contribute to the 'datafication' of evidence, and a reinforcement of the separations between data collection, analysis and interpretation—the 'deracination of facts'. In some cases, the interviews showed, archaeologists find it difficult to assess the quality of the work of colleagues, because it is so technically specialised. There was also awareness that some

areas of archaeology have become specialised through the use of dedicated software packages which can be opaque in their processes.

It emerged in interviews that the benefits and promises of technological approaches had to be balanced against difficulties in realising their potential. This was most obvious in the problems which interviewees had experienced in accessing digital archives of site excavations, and in issues with the sustainability of digital resources. Institutional hosting of digital projects and long-term support were problematic at times. Digital projects lacked inertia. However, the benefits presented by the use of digital media and tools outweighed these difficulties. For some, ongoing advances in technology use, for example in documenting dig sites, were taken as an indicator that the problems in areas such as digital sharing and access would be solvable within a short number of years.

Research virtues

For those preoccupied with the opportunities of digital methods, the research virtues of transparency, reflexivity and multivocality were generally accepted as worthy ideals. In practice, there was not always provision to build these into work practices. Instead, the main ethical aspect of the character of the research was a notion of *objectivity* realised through the application of what were taken to be impartial technological and analytical means, along with transparency in relation to attribution, and to an extent, methods. In my interpretation, the use of measures which had been recommended to ensure the accountability of technological methods, such as the documentation of process (paradata) and uncertainty, had failed to gain a foothold in contexts where such measures had proven to be un-instrumentalisable, uncomputable, or a challenge to deterministic analyses. In the case of the documentation of uncertainty, for example, this had been adopted most widely where it referred to *ontological* uncertainty, and was therefore compatible with computable, model-based analyses. Overall, the delegation of ethics to *technological* means had tended to find favour over epistemic virtues such as reflexivity or multivocality. There was strong awareness of the potential implications of this from Digital Archaeologists, but significantly less in the mainstream of the discipline.

Knowledge perspectives

Studies of the history of the discipline suggest that theory and practice do not move in synchronisation, or at the same pace. It is possible for established practices to persist, even where they are in conflict with more recent theoretical developments which have general acceptance. My study of thought communities showed that knowledge claims and 'facts' develop, and are only truly meaningful, in the context of groups in which there is consensus about worthy objects of study. As a result, there is an intercalated landscape of progress in research, in which models persist when they are strongly rooted in established communities, are internally coherent and capable of producing authoritative knowers who can make professional progress. I suggested that the methodology established by New Archaeology has persisted over the long term due to its compatibility with a computing turn and its ability to confer a strong sense of scientific credibility.

While the notion of academic research is mostly imagined as pushing back the limits of what we know, I suggest that it is also about constructing who we are: students, academics, specialists, members of expert groups. The way we conduct research may satisfy some of these ends more than others. Building 'knowers', demonstrating competency and maintaining scholarly communities all require, to some degree, performative knowledge work, made against consensual norms, and this is often based on an agonistic model. Such practices are not always conducive to open, collective models of knowledge-building or a self-questioning approach. I have emphasised, throughout the thesis, the significance of 'thought communities' and their 'reasoning styles': I suggested that the use of methods is a matter of academic identity, as well as epistemology, and that the application of scientific methods is often a key part of archaeologists' identities. I noted that archaeology has an eclectic and evolving store of technological methods, but that some research approaches have met resistance for being too 'distant' from the core of the discipline, from its perceived essence. The implication, that research methods have functions in building communities and identities, or 'knowers', as well as in building knowledge, emphasises the important need for critical reflexivity in relation to the genealogy of disciplines.

Translations of evidence

Studies 2 and 3 provided good examples of how data is made processible or computable for archaeological research. The observed project's theme of remote sensing resulted in the

dominant influence of the GIS system which was used to manage the data. The use of the GIS paradigm together with the Arches platform for public delivery of the mapping work meant that the process amounted to something like a factory production line, in which agreed procedures for interpretation and classification were iteratively developed, with the final shape of the published outcome the ultimate arbiter of what qualified for inclusion. This showed the need for data to be *pinned down* before it can be instrumentalised; and that the pinning down is where the *significance* of the data is decided. As significance is not an inherent property of artefacts, and is often a product of complex collective tacit knowledge, this is not easy to capture transparently in a database, if at all. In fact, the iterative aspects of the research and the developments of priorities in classifying the data did not find expression in the observed project's final outputs. Records had to be updated when classificatory schemes changed, but the database structure of the Arches platform was not suited to capturing these developmental aspects of the research process: instead, the affordance of the platform was to present models which appear to be 'natural' and stable, where the nature of significance was safely known. The skilled interpretative work of the project team found better expression in one-off web-based 'case studies' which allowed for the inclusion of multiple datasets and put the narrative aspects of the research centre-stage.

The journal articles of study 3 were useful in demonstrating that the emphasis on datafication and computability tend to be linked to a representationalist form of thinking, where representations are taken to exist in a separate ontological space, without any agency of their own. They prompted my suggestion that the outputs of hi-tech research also function as *independent*, *authoritative* witnesses to knowledge claims, by dint of their apparent mastery of nature and the demonstrably *non-human* perspectives they enable, allowing them to see from space, via radar, and so on. This technological witnessing is another example of outsourcing ethics (and 'skilled judgement') to technology, and of the persistence of the methods of mechanical (or digital) objectivity, even when awareness has been raised (e.g. by Critical GIS theory) of the need for a greater degree of reflexivity about the origins of such methods and the agencies they exercise. This might be considered another case of inputs proving to have a negligible impact when they are by nature difficult to instrumentalise or make computable.

The articles, in their strong commitments to RS methods and analysis, even in the face of inconclusive results in some cases, reinforced the sense of thought communities in conversation with themselves, shoring up their assumptions about knowledge making and their shared sense of identity; and of authors setting out their commitments to be members of those communities and to establish authority in them. They show that technologies and technical expertise can function both as shibboleths and as figurative flywheels, storing up the progress made by a research community's methods and helping to guarantee continued forward momentum.

An epistemological programme

I pointed out examples of archaeological research practice which have aspired to develop greater critical self-reflexivity as a result of the contradictions or fallibilities which have become apparent in certain systematised research methods. These emerged because of the often tenuous nature of archaeological evidence, which ultimately could not sustain deterministic isotope analyses of metal artefacts, or detailed 3D reconstructions of lost buildings.

My perspective is that archaeology is uniquely useful in providing such insights, but that the lessons they provide should be universally applicable: that what can be known is generally more unstable than that implied by the conventional representational forms of research outputs. My research has shown that in spite of these historical lessons, there has been a reluctance in archaeology to adopt methods different from those developed by the processualists. I suggest that one reason for this is that the opportunities presented by the computing turn, and certain epistemological commitments which accompany it, have tended to eclipse other interpretative approaches. Even a New Archaeologist such as Clarke was able to split archaeological research into phases, based on their epistemological commitments, and as a result, note the need for a hybrid approach to interpretation. This hybrid quality was for him the unique essence of the archaeological discipline. However, the recognition of such distinctions was not at all obvious in the archaeological research projects I studied. As a consequence, I have argued that there is a tendency for archaeologists to lack a clearly defined epistemological programme in their research, one which could help them to define a more nuanced relationship to the 'two cultures' of science and the humanities, and better equip them to handle what I see as central

considerations for the discipline: the management of uncertainty and the accommodation of a range of cultural perspectives. In particular, I suggest that a more conscious reflexive practice is required in relation to the computing practices which are at the heart of so much archaeological research.

The need for a conscious epistemological programme in research stems from the limitations brought about by the conventional opposition of positivistic research practices to a version of relativism in which all values are equal (and therefore none have meaning). I suggest that against the backdrop of strong historical associations between positivism, objectivity and impartiality, the notion of ethics in research has become too narrowly associated with objectivity, and therefore tied to a quest for universal meanings and truth, with any alternative seen as ethically debased. I posit that this is a false dichotomy, and in my discussion of an alternative epistemological programme, 'Epistemological Modesty', I advocate for a shift from universal truths to rich contexts, and for a need to live with 'troublesome knowledge' and its implications for what we can actually know. To properly support the research virtues of transparency, reflexivity and multivocality, I propose, as part of EM, the decentring of humans as a source of agency, and an emphasis on constant becoming rather than an ideal of knowledge as final and convergent. Becoming open to other knowledge perspectives and 'complicating causality' makes for a non-exclusive approach to understanding. Research claims must still be fully accountable to their contexts, and the processes of reasoning do not become in any way redundant: but outcomes should be open to being remade, recontextualised and iterated upon.

In my research, the ideal of the *reuse* of research outcomes was made problematic, most conspicuously in interpretative work, by the goals of responding to the particularities of context and being fully transparent about process. I concluded that the notion of reuse was still powerful and important for discursive engagement with existing work, but more for recontextualising or bringing new perspectives to it, than for accumulating critical masses of commensurable data.

Evolving digital practices

My development of the Orson software project as a test case for the EM approach helped to demonstrate the difficulties of manifesting such an ideal, but also illustrated some of the

opportunities for a digital archaeological practice with an alternative focus to the 'datafication' of evidence. In the project I sought to emphasise affordances of digital practice other than computability or the processing of data at scale: I focused instead on the opportunities digital media and interactions provide for networked communication, for capturing process, and for the rewriting and reinterpretation of ideas, evidence and narratives in malleable digital forms.

In spite of the project's goals, the students testing the software responded to it primarily as a potential tool for organising and managing data—for 'bringing everything together in one place'. The affordances for capturing process, rewriting, reinterpreting and for decentred publishing were less apparent. While these were discussed enthusiastically and positively when raised, I was conscious that this enthusiasm may have represented an association of certain activities, such as collective authoring, or reinterpretation, with the requirement for ethics in research. As my other studies showed, a positive disposition towards such ideas does not necessarily translate into their active adoption.

A striking finding from this phase of the research was the extent to which researchers saw themselves as part of particular research communities, and viewed research outputs (including, potentially, Orson-based research projects) not just as standalone sources of knowledge, but as resources for accessing further reading (through literature references), and as potential material for inspiring or backing up their own claims. This emphasised that instances of Orson or similar tools could perhaps most usefully act as nodes in a knowledge network or ecosystem, a prospect which tallied well with the goals of facilitating multivocality and reflexivity.

In spite of the positive feedback participants gave about Orson, it was clear that there were barriers to its envisaged uses, not least in the shape of entrenched systems of academic reward and credit, based on agonistic models of authorship. The pressures felt by participants (who were mostly students) to demonstrate competence in their fields, and to be seen to be ethical practitioners, meant that they were cautious or conservative in their approach to contributing to or iterating on others' work in Orson.

I concluded that further work was required to test the potential of Orson as an enabler of EM-informed research practice. The affordances for rewriting, reinterpreting and publishing need to be made central features, and the invitation to iterate on published work needs to

be stated overtly. The software should enable more experimental approaches to representations and the creation of alternative narrative models which might do justice to the hybrid nature of the disciplines of archaeology. Ideally it should be developed further in the context of the documentation of real-world projects which can feed into the design of its feature-set. Moreover, the content of Orson projects needs to be made separately publishable, to enable sharing and copying, and overcome some of the inhibitions users feel about rewriting or contributing to work which is 'owned' by other researchers. The software should act as a conduit for other web services, allowing it to function usefully as a part of a research *ecosystem.* As there are obstacles to getting research credit for non-conventional outputs, and the entrenched nature of incumbent 'black box' systems raises the cost of trialling alternatives, I also concluded that a good starting point for making Orson useful and embedding its philosophy would be to present it as a *supplement* to existing, conventional research: for example as a web-based resource for filling out the context of a published journal article, and making its content more discursive and interactive. This would be one possible response to the challenge of finding a way to encourage communities to cohere around the use of new tools which offer new modes of interaction and production rather than 'breakthrough' technology.

The future of Orson

The Orson software project is open-source and therefore capable of capitalising on submissions from contributors, such as ideas and features to make it more fit for purpose, adaptations for specific use-cases, additional documentation, and so on. Against this opportunity is the lack of a system of remuneration to support development efforts or maintenance. Already a significant piece of work is required to bring the codebase up to date, as one of its major code components has moved to a new version, with the result that some of the legacy code used will not receive security updates and will be incompatible with new features. Making this update will amount to a major investment of time and effort. As the sole author of the software to date, I am committed to developing it further with the goal of realising the aspirations I have set out for it, and testing whether it can fulfil its intended potential for making a contribution to the research landscape. This is also partly for selfish reasons as I continue to use the software daily in my own research. Realistically, I will need support from other developers and users to make this possible, as my own

contributions will be constrained by my other professional commitments and the need to earn a living. The future development of the software therefore depends either on securing formal funding or on the emergence of a community of users and contributors who see enough potential in the project to commit time and resources to its development. At the time of writing it seems less than straightforward to encourage a community to cohere around the project, because the Orson software does not offer technological novelty in the way that machine-learning software tools or those geared to supporting a specific technical research technique do. Rather, it offers a paradigm for organising and sharing research work, albeit with open-ended opportunities for modes of representation. It is therefore less easy to pitch as a solution to a specific problem, as a potential source of research 'breakthroughs', or as a clear fit for an identifiable research community. In developing the software I have been highly conscious of the need to engage a community of users, including developers, for it to gain traction and long-term viability. This informed many of the design choices, as described in chapter 8. Going forwards, I intend to focus the development work on features which can further increase opportunities for community engagement. One of these will be making contributions of Context and Page plugins the most obvious point of entry for software developer engagement (as opposed to code contributions to the platform overall, which I am now conscious requires more of a learning/time commitment). This would in part be achieved by adding a built-in interface in Orson for downloading and installing third-party contributed plugins from web-based repositories, a feature familiar from code authoring software packages and content management systems.

Another area to explore is to augment the affordances for moving digital resources in and out of Orson instances. In addition to providing conventional import and export features, this could involve the publishing of Orson research resources to a range of web-based community contexts. The mobilisation and decentralisation of research resources is an important ideal in the project, stemming from the epistemologically modest approach, and one which has not yet been properly realised. One straightforward strategy in this vein would be to allow individual resources created in Orson to be embedded in the web pages of other sites, in the way that videos, audio clips and social media posts often are, thereby increasing the presence of Orson resources in other web-based conversations, and signposting readers to the site of their origin. A more sophisticated goal would be to offer

support for the federated distribution of Orson resources, using a technical model such as ActivityPub[60], a protocol familiar from the decentralised web-based community platform Mastodon[61].

Most vital for the encouragement of engagement with Orson, however, is the use of the software for the documentation of real-world research projects which can be used as a means of seeding the use of the software in research communities, guiding the direction of further development, and providing inspiring exemplars for other researchers.

Finally, when the software is more capable, and ideally when inspirational example instances can be shared, energy and resources can be expended on publicising the software more widely, and on encouraging shared public ownership of the code and the ideas it represents. Whether it is widely adopted or not, the project can provide a useful contribution to debates about appropriate approaches to digital research, and as a source of inspiration for features in other software packages. To some extent it has already done so, via my formal research engagements with academics and students, and through informal exchanges with researchers and software developers on social media platforms.

Lessons and opportunities

This project has laid bare the difficulties inherent in getting a 'close up' view of researchers at work, not least where the research is conducted via computer-mediated means, either with regard to online meetings, or the processes of designing, collecting and making interpretations of data. In spite of the great generosity shown to me by archaeology professionals in sharing their ideals, experiences, expertise and work practices, I was left with the impression that had I been more deeply embedded in the profession, it would have been more feasible to gain access to the detail of site excavations, software practices for the manipulation of data, funding discussions, and so on, albeit with limits on social interactions appropriate to Covid containment measures. Similarly, it is more likely that I would have been successful in persuading archaeologists to trial the Orson software in their research or teaching projects, or that I would have developed closer collaborations with archaeologists in designing Orson's features. It may have been that there was some suspicion among

[60] https://www.w3.org/TR/activitypub/
[61] https://joinmastodon.org/

potential collaborators that my objectives in developing a software project were not fully aligned with those of working archaeologists, that the project would not properly reflect their requirements, or that it was inappropriate for an 'outsider' to be working in a field in which software developers with archaeological training were already making innovative work. In my research, the commitment of partners fluctuated over the course of what proved to be a lengthy project. I was fortunate to be able to work with a cohort of enthusiastic students to gain useful insights into the strengths and weaknesses of the Orson software in relation to its aspirations. However, the insights may have been richer if the enquiry was co-conducted with archaeological professionals as part of the documentation of a 'live' project, as was my original intention.

On the other hand, I was able to gain a high-level perspective of the structure of a discipline and its expert communities which would have been difficult to attain for a researcher who was a deeply embedded member of those academic groups. In retrospect then, there may have been an internal tension in my research project between developing an overview perspective of a discipline's creation of knowledge claims, and the effort to develop a prototype software package for use by archaeologists. The former required some level of analytical distance, while the latter may have gained more traction in its goals had it been conducted among familiar colleagues with a shared background and well-established levels of trust and commitment.

I see this tension in my project as a consequence of the interdisciplinary nature of my research. There is a paradox to the 'outsider' perspective sometimes missing out on the 'close up' view, yet in other ways being able to see more, because the accepted, taken-for-granted norms of a specialism are not ingrained. The breadth of the studies undertaken in my project presented challenges to their successful undertaking, and to the maintenance of a consistent critical approach. On the other hand, this multi-perspectival approach was consistent with the tenets of the research philosophy I was exploring, and was vital for providing context for the software design study—context which went far beyond the familiar technical software development parameters of useability and accessibility, to look at assumptions about what can be known in research, and how these assumptions are arrived at. This became apparent to me because the principles I developed for the software design resulted in the creation of features which were outside of my own natural comfort zone as a user, such as the potential to open research resources to alternative publishing

environments or to unpredictable uses by others. Furthermore, I found the multiple perspectives afforded by the interviews, observations and article analyses highly mutually informative, which I took to be a vindication of the project design. At the same time, the commitment to a breadth of analytical approaches undoubtedly came at the expense of greater depth in individual studies.

* * *

There is no single, unified discipline of archaeology to which my research findings can be universally applied. One significant strand in my studies of the mainstream of the discipline, however, is what I have described as techno-optimistic archaeology. In this field, there is a tendency towards a complex and apparently contradictory understanding of facticity. It is a product of fastidious caution about what can be claimed in individual considerations of evidence, and the use of data-driven research approaches which seek to capitalise on the powers of computing, but are not always able to attend to the detail of individual interpretations. Technological witnessing is often used, by an appeal to its independent authority as a non-human, objective arbiter, as a means of lending legitimacy to the 'mechanisms of closure' provided by systematic means of capturing, categorising and representing data. While there is a general awareness of the value of making use of other techniques for legitimising and contextualising research processes, for example, reflexivity, or the inclusion of a range of cultural perspectives, established 'channels' in archaeology can successfully produce knowledge claims and authoritative knowers by appealing to the ideal of a universal, stable model of knowledge, objectively gained, without the need for in-depth considerations of context. Technological tools and software packages which fit with communities' perspectives naturally tend to be the ones which are actively used and developed by them. Expertise with such technologies thus becomes absorbed into the academic identity of research groups, and commitment to their use can be a means of gaining entry to the group, or of increasing personal authority within it. Technologies can thus become a way of fixing or fostering certain epistemological commitments, as well as more straightforwardly reflecting the values of established practices.

The strongest challenges to hegemonical models come from those who tend to be marginalised or oppressed by them. These challenges highlight why *interpretative*

knowledge claims must consciously and conspicuously remain open to the legitimacy of alternative epistemologies. My claim in this thesis is that while archaeological research (and its reception) has often brought such cases into the spotlight, along with the difficulties of dealing with uncertainty, gaps in evidence, and the bias effects of technological representations, the conscious application of epistemological modesty is in fact appropriate for *all* forms of academic research. In archaeology, there is a danger of seeing such an approach as only relevant to post-colonial or feminist perspectives, for example, and therefore not essential to 'normal' archaeological work. I argue that ways of knowing are not just divided along the lines of gender, geography or national culture, but that academic communities and subdisciplines have their own distinctive epistemological and ontological commitments, and that conscious awareness of this should inform all research outputs which strive for transparency and self-knowledge.

Those affordances of computing and digital mediations which hold out the promise of new ways of accessing archaeological facts or universal truths have the potential to obscure other approaches. However, I have demonstrated that digital tools and communications also have intrinsic qualities which can be used to manifest and encourage research values such as reflexivity, transparency and multivocality: namely, networked access, and the capacity for outputs to be copied, rewritten, recontextualised and iterated upon. Examples from the literature show that numerous archaeological research projects are already undertaking this work. The programme of Epistemological Modesty which I have proposed provides a useful template for embedding such values into the design of practical research projects and tools. It is not clear whether digital tools or modes of representation can by themselves be the catalyst for encouraging research methodologies which make their own subjectivities visible and open to critical intervention. But as my previous discussion of the role of technologies and technical expertise in communities of knowers demonstrates, they are surely a vital element in making such practices possible, and can act both as entry points into a research community and as a means of maintaining or furthering its assumptions and methods.

As archaeology continues to explore the research opportunities made possible by the computing turn and new technologies, a corrective is required to avoid the indiscriminate projection of a computer-inspired ontology onto the cultural sphere, and to address the impression that the data can 'speak for itself'. The EM approach, together with the adoption

of tools like Orson or others proceeding from the same principles, has the potential to prompt a considered understanding of the different phases of archaeological research, and the appropriate interpretative expectations for those phases, while still embracing opportunities made possible by technological innovations. Highlighting the 'epistemic registers' fostered by digital networking, malleability, iteration and multi-perspectivism might encourage a reorientation of researchers to the possibilities of computing-based research beyond the familiar areas of technological objectivism.

In reinstating the situated perspective of the author, being more open to the implications of troublesome knowledge, and prompting explorations of the dynamic between detachment and engagement in reporting research, the EM approach has potential to encourage new interactions and new inclusive discussions, demonstrating that there are pragmatic, accountable and ethically robust alternatives to the long-established extremes of positivism and relativism.

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Appendix 1—Study 1 Interview Questions

To establish common points of comparison, questions on the following topics were put to all participants.

Context - Professional / Community

	Question	Rationale
1	Could you tell me about your job? [What is your job title if you have one? If you don't have one, what would you say it should be?]	Get an understanding of the interviewee's own sense of professional identity.
2	How did you come to be working in this field? [What are your professional affiliations?]	Get further details of memberships of knowledge communities.
3	What are the results of your work?	What's the output at the end?
4	Who do you share the results of your work with? [Who is the audience - a client, the public, a researcher? Do they have the same specialised knowledge? Which journals are targeted?]	What and/or who is the driver for research? Do the knowledge claims have to be 'translated' for non-specialists?

Process

	Question	Rationale
5	What is a typical day at work like?	Get an understanding of the building blocks used to construct knowledge claims, and how they are fitted together.

6	How much of your work is learned on the job, or not easily taught? ['Common knowledge, tradition, art'? Or, what aspects of your job couldn't be learned in school or college or university?]	Tacit or embodied knowledge, or explicit knowledge?
7	Do you ever have to rely on someone else's expertise when collecting or using data? [If so, are any aspects difficult to understand or evaluate? Do you have problems communicating your own expertise?]	Explore issues around multi/interdisciplinarity, collaboration, commensurability.

Data / Evidence / Interpretation

	Question	**Rationale**
8	When you are working with data, do you have the sense that some of it is more or less reliable than others? [Where is the unreliability if it is there - in the instrumentation, or process of acquiring, or quality of existing resource?]	Explore understandings of facticity.
9	How do you decide what is reliable and what isn't? [Statistical, authority, other?]	As above. Explore use of mixed data sources, triangulation, 'cable' approach to knowledge building.
10	Are the processes you use standard in your field of work? Where is your approach	An open question to try to personalise the response and avoid rote answers.

	unique or unconventional? [Is there anyone else working in your field who you rate highly? If so, why?]	

In addition, where appropriate, interviewees were asked to discuss their projects and areas of research interest in relation to principles expressed in the London Charter, namely:

- Incorporation of paradata (contextual data about authorship and process) in archaeological research
- Inclusion of measures of uncertainty or varying reliability
- Support for multivocality and multiple interpretation

This area of questioning was linked to their roles and prior readings of their published work.

Those with an interest in the uses of the digital domain for archaeology were asked to recommend projects at the cutting edge of research, and to comment on the prevalence in current research of practices recommended in the London Charter and Seville Principles.

Appendix 2—Study 3 Journal Article Abstracts

Paper 1

DOI: https://doi.org/10.1016/j.compenvurbsys.2015.09.003

Cultural heritage management and monitoring using remote sensing data and GIS: The case study of Paphos area, Cyprus

Cultural heritage (CH) sites are threatened from a variety of natural and anthropogenic factors. Innovative and cost effective tools for systematic monitoring of landscapes and CH sites are needed to protect them. Towards this direction, the article presents a multidisciplinary approach, based on remote sensing techniques and Geographical Information System (GIS) analysis, in order to assess the overall risk in the Paphos district (Cyprus). Paphos region has a great deal of archaeological sites and isolated monuments, which reflect the long history of the area, while some of them are also listed in the UNESCO catalogue of World Cultural Heritage sites. Several natural and anthropogenic hazards have been mapped using different remote sensing data and methodologies. All data were gathered from satellite images and satellite products. The results from each hazard were imported into a GIS environment in order to examine the overall risk assessment based on the Analytic Hierarchy Process (AHP) methodology. The results found that the methodology applied was effective enough in the understanding of the current conservation circumstances of the monuments in relation to their environment as well as predicting the future development of the present hazards.

Paper 2

DOI: https://doi.org/10.3390/su12010240

Cultural Heritage Management Using Remote Sensing Data and GIS Techniques around the Archaeological Area of Ancient Jeddah in Jeddah City, Saudi Arabia

Historic Jeddah is located on the eastern shore of the Red Sea. Historic Jeddah was designated as a UNESCO world heritage site in 2014. The new urban development for the city of Jeddah has resulted in different spatial patterns. The southern part of Jeddah city falls within the moderate zone, because this area is well developed in regard to infrastructure with rainstorm and sewage networks. The middle area of the city falls within high vulnerability risk due to its high population, shallow water depth, flat slopes, and various incomplete network services (i.e., leakage from septic tanks and water pipes). The western and northwestern parts of the city are subject to very high pollution risk, due to the highly permeable area with coralline formation, very shallow water depth, and depressions. Unfortunately, historic Jeddah has been affected by the unplanned development and shallow water depth. Most of the construction and decoration of the ancient buildings are suffering from deterioration. The paper aims to detect the environmental changes, assessing the geo-environmental status, and creating some of the innovative solutions while using the integration between remote sensing and GIS techniques. The combination of SRTM, Corona 1966, Spot 1986, Landsat 1987, Orbview 2003, and Sentinel2A 2017 data will help in monitoring the changes around the study area. The Bands combination and the spatial statistical analysis are considered to be the most effective methods in the examination of the new built-up indices. GIS techniques and some models would be suggested as solutions to protect the archaeological area, according to UNESCO recommendations.

Paper 3

DOI: https://doi.org/10.3390/geosciences10120485

Following the Roman Army between the Southern Foothills of the Cantabrian Mountains and the Northern Plains of Castile and León (North of Spain): Archaeological Applications of Remote Sensing and Geospatial Tools

Sixty-six new archaeological sites have been discovered thanks to the combined use of different remote sensing techniques and open access geospatial datasets (mainly aerial

photography, satellite imagery, and airborne LiDAR). These sites enhance the footprint of the Roman military presence in the northern fringe of the River Duero basin (León, Palencia, Burgos and Cantabria provinces, Spain). This paper provides a detailed morphological description of 66 Roman military camps in northwestern Iberia that date to the late Republic or early Imperial eras. We discuss the different spatial datasets and GIS tools used for different geographic contexts of varied terrain and vegetation. Finally, it stresses out the relevance of these novel data to delve into the rationale behind the Roman army movements between the northern Duero valley and the southern foothills of the Cantabrian Mountains. We conclude that methodological approaches stimulated by open-access geospatial datasets and enriched by geoscientific techniques are fundamental to understand the expansion of the Roman state in northwestern Iberia during the 1st c. BC properly. This renewed context set up a challenging scenario to overcome traditional archaeological perspectives still influenced by the cultural-historical paradigm and the pre-eminence of classical written sources.

Paper 4

DOI: https://doi-org.libproxy.ucl.ac.uk/10.1016/j.culher.2016.04.013

Uncovering the ancient canal-based tuntian agricultural landscape at China's northwestern frontiers

The tuntian system was a state-promoted system of military—agriculture, which originated in the Western Han dynasty (206 BC—9 AD). All the imperial dynasties in Chinese history adopted the practice of tuntian to cultivate and guard frontier areas as an important state policy for developing border areas and consolidating frontier defense. This paper describes the use of satellite remote sensing data to uncover an ancient canal-based tuntian system located in an oasis agricultural landscape adjacent to the ancient Kingdom of Loulan at the southern margin of the Tarim Basin. The remote sensing data examined include Chinese Gaofen-1 (GF-1) VHR imagery, Landsat-8 (LS-8) OLI data and ASTER Global Digital Elevation Model Version 2 (ASTER GDEMV2) products. The effective irrigated tuntian area was

estimated to be 2800 ha and the maximum irrigated tuntian area was found to be more than 8000 ha during the area's most prosperous period. The overall spatial structure of Milan's tuntian agricultural landscape was explored using the patch—corridor—matrix model. By detailed analysis of satellite remote sensing data, this study reconstructed a 3D view of Milan's tuntian agricultural landscape in a GIS.

Paper 5

DOI: http://dx.doi.org.libproxy.ucl.ac.uk/10.1371/journal.pone.0240739

Seasonal movements of Bronze Age transhumant pastoralists in western Xinjiang

The paper explores seasonal movements of Bronze Age mobile pastoralists in the western Tianshan mountainous region of Xinjiang, China. Fieldwork by a team from the Institute of Archaeology of the Chinese Academy of Social Science (CASS) and the University of Sydney, Australia have identified cyclical land use practices associated with the Andronovo cultural complex. Their pattern of seasonal movements has been reconstructed through ethnographic studies and analysis of modern snow and grass cover. Using this detailed combination of data, the study defines requirements for seasonal pastures—winter, summer and spring/autumn—and shows a clear correlation between modern land use and seasonal patterns of movement in the Bronze Age.

Paper 6

DOI: https://doi.org/10.3390/rs12223694

Detecting Change at Archaeological Sites in North Africa Using Open-Source Satellite Imagery

Our paper presents a remote sensing workflow for identifying modern activities that threaten archaeological sites, developed as part of the work of the Endangered Archaeology

of the Middle East and North Africa (EAMENA) project. We use open-source Sentinel-2 satellite imagery and the free tool Google Earth Engine to run a per-pixel change detection to make the methods and data as accessible as possible for heritage professionals. We apply this and perform validation at two case studies, the Aswan and Kom-Ombo area in Egypt, and the Jufra oases in Libya, with an overall accuracy of the results ranging from 85—91%. Human activities, such as construction, agriculture, rubbish dumping and natural processes were successfully detected at archaeological sites by the algorithm, allowing these sites to be prioritised for recording. A few instances of change too small to be detected by Sentinel-2 were missed, and false positives were caused by registration errors, shadow and movements of sand. This paper shows that the expansion of agricultural and urban areas particularly threatens the survival of archaeological sites, but our extensive online database of archaeological sites and programme of training courses places us in a unique position to make our methods widely available.

Appendix 3—The Reuse of Research Results: Mini Literature Review

A3.1 Overview

Much of the literature on research reusability relates to scientific research. Only in a minority of cases are the requirements of different disciplines taken into account: for example in Curty et al. (2016) and Yoon and Kim (2017), where privacy and confidentiality issues in social science research are discussed; and in Faniel et al. (2019), where interviews are conducted with scholars in the fields of social science, zoology and archaeology. For the most part however, data sharing and reuse are seen as synonymous with the scientific method—a typical view is that they are 'the centerpiece of Open Science principles and scientific progress' (Dorta-Gonzalez et al., 2021:2209). For Curty et al.:

> Research data are the backbone of scientific discovery and technological innovation, and are regarded as the prime currency of science […]; the 'building blocks' of research. (Curty et al., 2017:1)

There is a common emphasis in the literature on the sharing of data in particular, and a focus on the question of why researchers are (or are not) motivated to make their data available to others. Linked to this are some considerations of the context of the data, and determinations of its validity and suitability for use. A distinction is made in the literature between 'small science' projects, in which:

> data tend to be small in volume, local in character, intended for use only by these teams, and are less likely to be structured in ways that allow data to be transferred easily between teams or individuals (Suhr et al., 2020:4)

—and 'big science', where the data 'may be great in volume but usually are consistent in structure' (ibid). The notion of 'suitability for use' of existing data is crucial when considering practices to do with reuse, and by extension, reinterpretation. It is a proxy for the philosophical assumptions we adopt when making inference from evidence, and in particular about the generalisability of data to different contexts. In the literature, issues

around the reusability of data tend to be framed in terms of standardisation: has the data been retrieved properly, by a trustworthy colleague from a recognised institution? Is like being compared with like? Such questions are vital if data are to be used as part of a methodological analysis based on strict sampling criteria. Much of the literature around the reuse of research results is concerned with the effectiveness of the standardisation of data descriptors, for example Kim (2021).

The foregrounding of uncertainty and the limits of knowledge models, on the other hand, is unusual. One of the few areas of the literature in which themes of epistemological validity are explicitly considered is that in which analyses are made of decision-making processes and strategies to deal with uncertainty. Kale et al. (2019) recommend strategies for exploring and reporting a 'reasonable subset of analyses' where there is uncertainty, and point out the dangers of using reduction strategies to select a single analysis, thereby suppressing uncertainty. The assumptions about knowledge construction in the literature on reuse of results can be seen on a spectrum, encompassing:

1) 'big science' where the emphasis on data implies that the data are a standalone source of meaning;
2) 'small science' which has less generalisable results;
3) considerations of research in different disciplines (including the social sciences) and of how context is relevant to meaning rather than just a useful indicator of data conformity;
4) explicit examinations of how to cope with uncertainty.

The levels of reflexivity about epistemological security scale proportionately across this spectrum, but the majority of the literature is written from the perspective of sharing independent datasets. In spite of this focus, there are findings in the literature which have general relevance to reuse in a range of contexts and are potentially useful for informing design decisions in the development of research tools. In the case of archaeology, the multi-faceted identity of the discipline means that some sectors of it are more aligned with fields such as biology and materials science than with social sciences like anthropology. With that context in mind, I suggest that it is instructive to consider recommendations from all areas of the literature.

A3.2 Key findings from the literature review

The main findings I identified are as follows:

1. For reuse to take place, trust should be established at the level of the creator and their curation process, the quality of information provided about data collection, and in the detail of the data management. (Faniel et al., 2019, Koesten et al., 2020, Yoon and Lee, 2019)

2. Successful reuse is only possible where data and process is described in high detail. However the effort this entails can be a deterrent to data sharing. (Suher et al., 2020)

3. Different disciplines have distinctive requirements for providing contextual information about data or the research process in general. (Faniel et al., 2019)

4. Information and advice about reuse should be available in the documentation or in the community, or both. Features related to understandability should be prioritised. (Faniel et al., 2019, Koesten et al., 2020, Yoon and Lee, 2019)

5. Standardisation may encourage reusability, as may the existence of suitable repositories. (Kim and Nah, 2018, Kim, 2021)

6. Community norms have a significant influence on the extent to which reuse is practiced. (Kim and Nah, 2018, Kim, 2021)

7. Evidence of the efficacy of reuse has an influence on the practice of reuse. (Curty et al., 2017, Kim and Nah, 2018)

8. Repositories should support the measurement of reuse. (Koesten et al., 2020, Yoon and Lee, 2019)

9. There may be challenges to reuse in the context of decentralised non-collaboratory research efforts, where measures of quality and reputation are more difficult to ascertain. (Faniel et al., 2019)

10. Providing support for the navigation of uncertainty can clarify the utility of project data for reuse. (Koesten et al., 2020, Kale et al., 2019)

Appendix 4—Study 4 Interview Questions

Questions

	Question	Rationale
1	Are you a member of a particular research community? What is your area of study?	Get an understanding of the research community the interviewee is a member of.
2	Is there any specialised technical knowledge required to work in your field?	Understand the processes the interviewee uses in constructing knowledge.

Software tasks script

“This is a research notebook software package, showing an example research project which has been published.

The way this software works is that there are cards, a bit like cards in a filing cabinet, for capturing bits of information or data. The cards can be collected together or visualised in different ways, called contexts. So you could put the cards onto a map, or add them to a timeline, or add some to a group under a heading. It’s like putting the cards into different boxes. These boxes are called contexts.
Finally there are pages where you can write up results and add links to the cards and contexts you have created.

Looking at this project I would like you to imagine that you have seen this project published on the web, you have done research work on the same topic, and you want to update this notebook to reflect your findings.

I'm going to ask you to do a few tasks in the software. This is not to test you in any way, but to gain insight into the software prototype and its features. Please talk out loud about your thought processes as you try things out."

3	~~Can you find the main Project Page, which describes the research project?~~	How discoverable is the Project metadata / paradata?
4	What does the term 'README' file mean to you, if anything?	Is README a useful term for a project documentation page – does it help to promote the idea of active engagement with and reuse of the research outputs?
5	Let's imagine that you are looking at this project with a view to reusing it or some of its contents in your own research project. What kind of information would you like to see here to help you judge if the project's results were of good quality and suitable for being reused? (Try to think about a project in your own field, rather than this particular example, if it's not a good fit for you.)	What kind of paradata is required?
6	~~Can you find any details about the people who have created the content – the people who have created the Cards, for example?~~	How discoverable is information about users?
7	What kind of information would you like to see on the user page to help	What measures are used for researcher authority and trustworthiness?

	you judge if the person was a 'good' researcher? (If you were adding details about yourself to a project, what information would you include to convince others about your credibility?)	
8	Let's go back to the Homepage. Can you find a piece of content in the project which features a map?	Explore the paradigm of context-based representations.
9	What do you think the relationship is between the map and the cards in the project? (If I said cards can be reused in different contexts, would that make sense?)	As above. Is the idea of reusing cards in different contexts easily grasped, and is it practical?
10	Can you make a copy of the map context with a different name?	Explore the idea of reworking existing content. Does the software invite this possibility?
11	Can you add a new marker to the map?	As above.
12	If we look at the pages, can you add a link in a page to the map you copied? To edit text, click on it. To finish, press ESC on the keyboard.	Explore the paradigm of pages as sites of consolidation and conclusion for research findings.
13	Can you see a way of previewing the map without leaving the page?	As above.

14	As I said before, the software has different content types for visualising data, like maps and timelines. Is there a particular content type that would be useful for your field of study?	To what extent does the software fit the interviewee's specialism? Are custom content types feasible or desirable?
15	How would it feel to edit the content of the project directly like this, rather than, for example, referencing it from a publication of your own? (How would you highlight the changes or additions you have made?)	Explore possible resistance to reuse and reinterpretation using shared materials.
16	Would you feel comfortable sharing a research notebook of your own project in this kind of form on the web or in a research repository? (Explore possibility of curated content)	How receptive is the interviewee to an open scholarship approach? Is that linked to norms in their discipline?
17	If you were publishing research data in a web resource to support a research article, what kind of data would you be likely to share?	To what extent is interviewee open to sharing process and data? Introduce the notion of sharing notebook-based resources.
18	If you were documenting your own project in a research notebook, are there any aspects of your research you would rather keep private?	What are the practicalities of sharing context, paradata, sensitive information, etc?

19	(Briefly) Do you have any idea how common it is for researchers to reuse published research results in your area of research? (Are there any recognised repositories where results are published?)	Find out what the norms are in the interviewee's community, when it comes to reusing research.
20	(Optional) How important is it for your research to use standardised categories or descriptions? (Would the use of standardised descriptors influence your willingness to reuse or reinterpret someone else's work?) What about standard or official repositories?	To what extent are the outputs of the discipline framed using shared conventions? (How much scope is there to challenge these?)
21	Thinking about the software we've been using today, do you have any thoughts on what the software is good for, and what it is not so good for? Would it force you to work in a particular way?	What is the agency of Orson?

	(Do you think it is suited for a particular type of research – or researcher ?)	
22	Do any adjectives come to mind to describe this kind of software? (If it was a person, what kind of person would it be?) (If it was a room in a building, what kind of room would it be?) How technical is it 1 – 10? How formal is it 1 – 10?	As above. What is the character of Orson?

Appendix 5—Screenshots from an Orson project

I created an online resource in Orson to accompany one of my conference presentations, as a small-scale test of some of my ideas and as part of my self-reflexive practice. I referred the conference session participants to the website while I gave the presentation, allowing them to navigate through the content in a non-linear fashion, and to explore some of the detail and textual sources behind the statements in the formal talk.

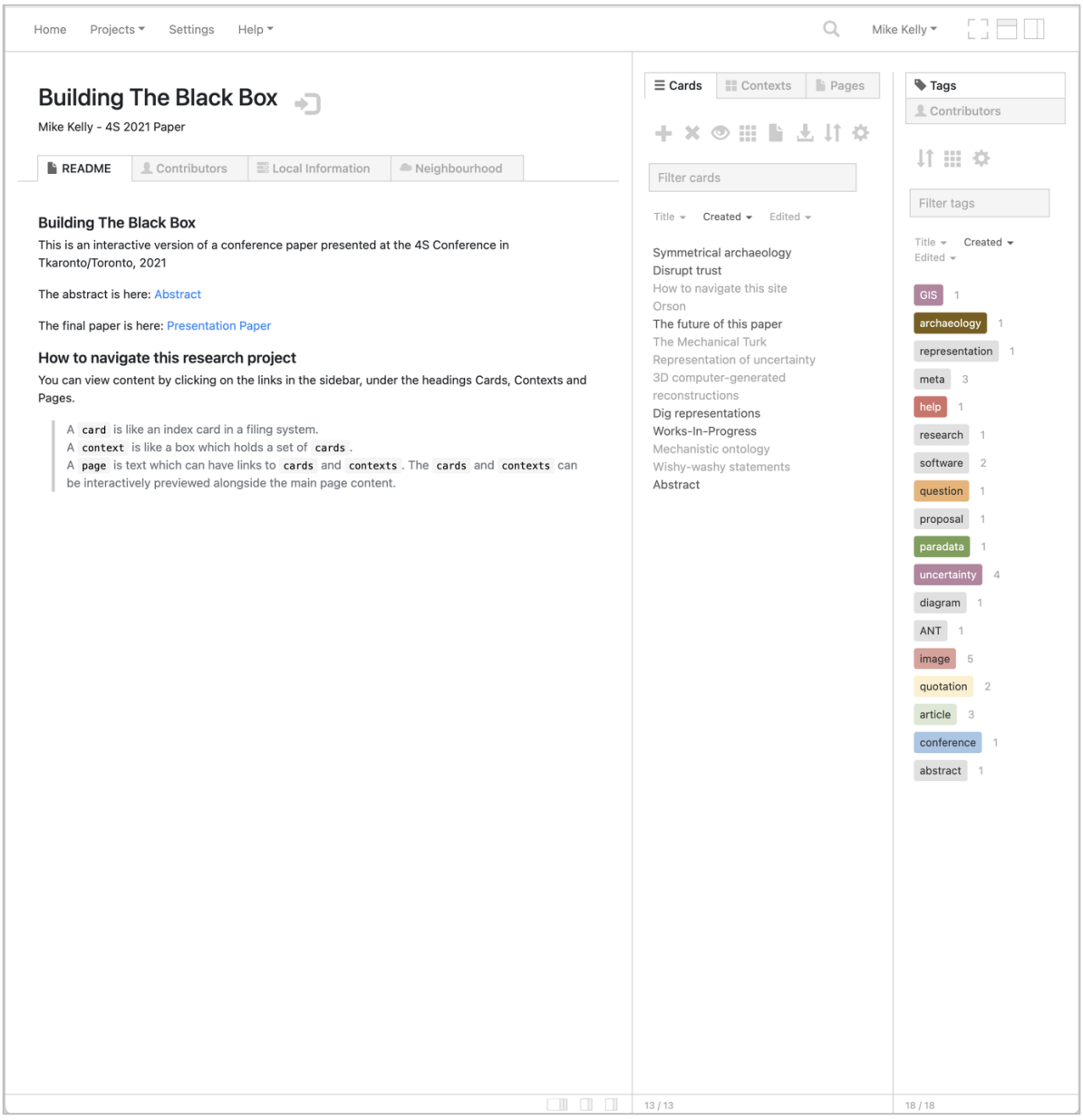

Figure 18. Conference presentation project in Orson, featuring README home page.

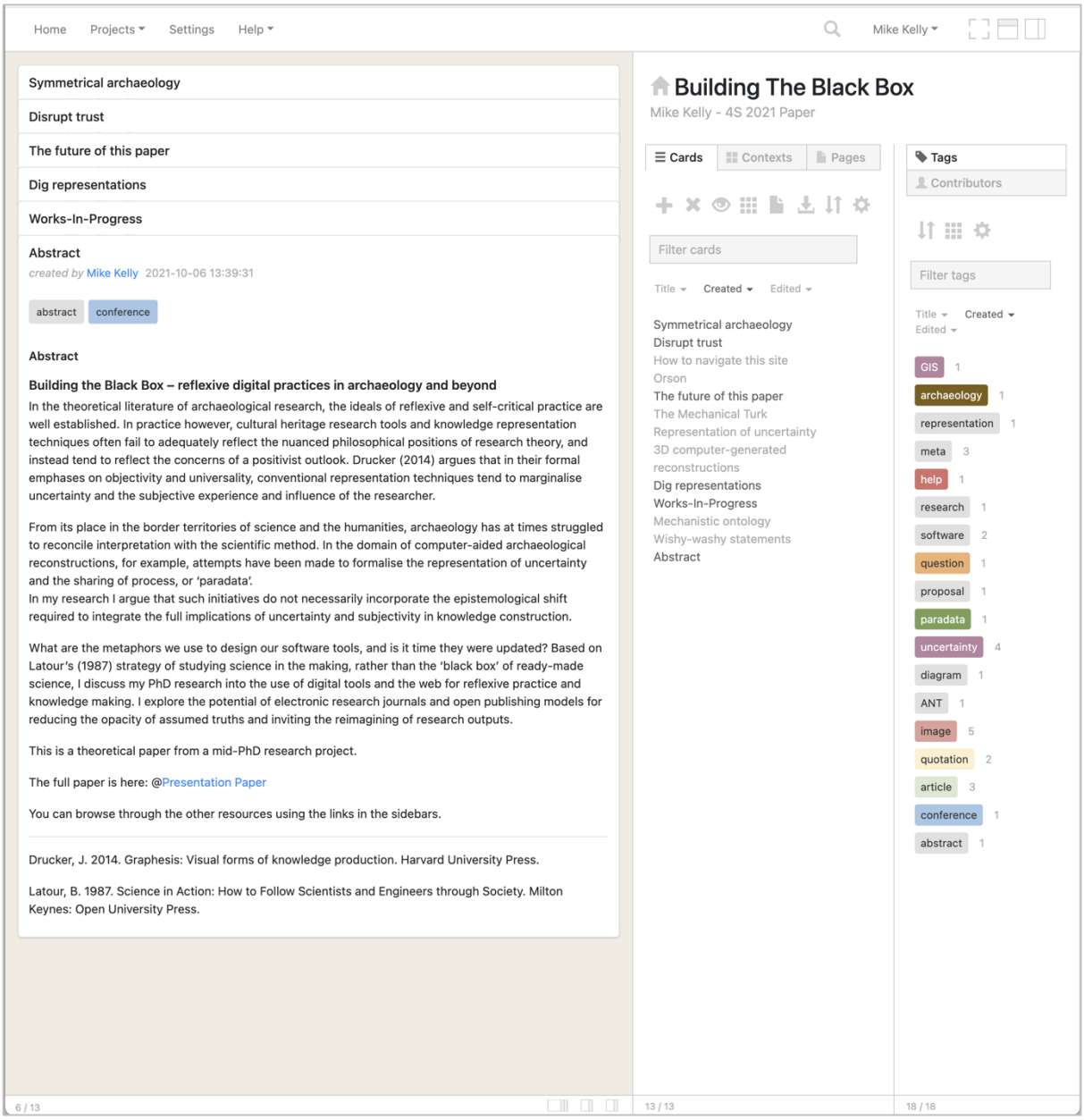

Figure 19. Conference presentation project in Orson, featuring the Cards 'stack'.

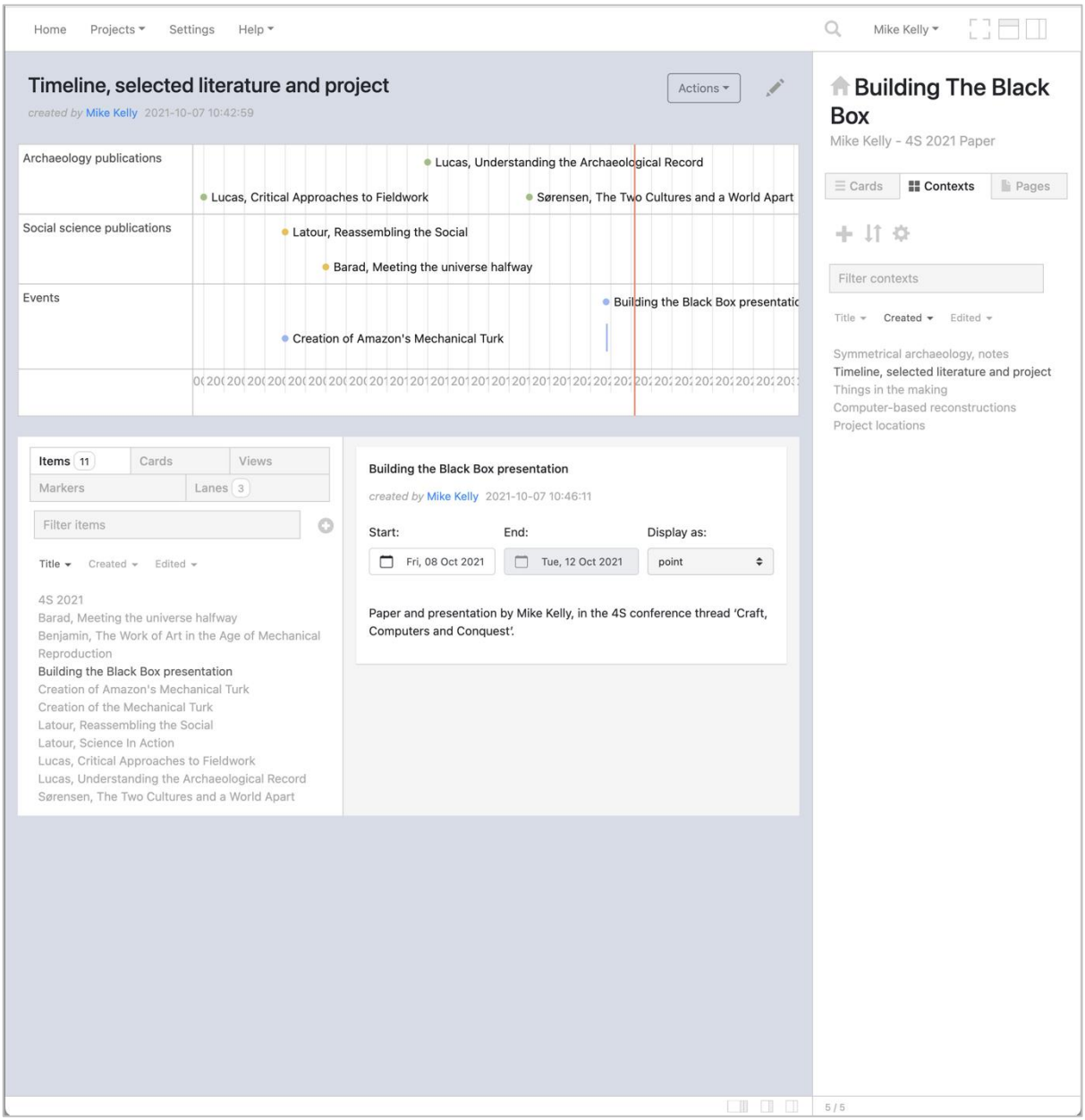

Figure 20. Conference presentation project in Orson, featuring project Timeline Context, including literature references.

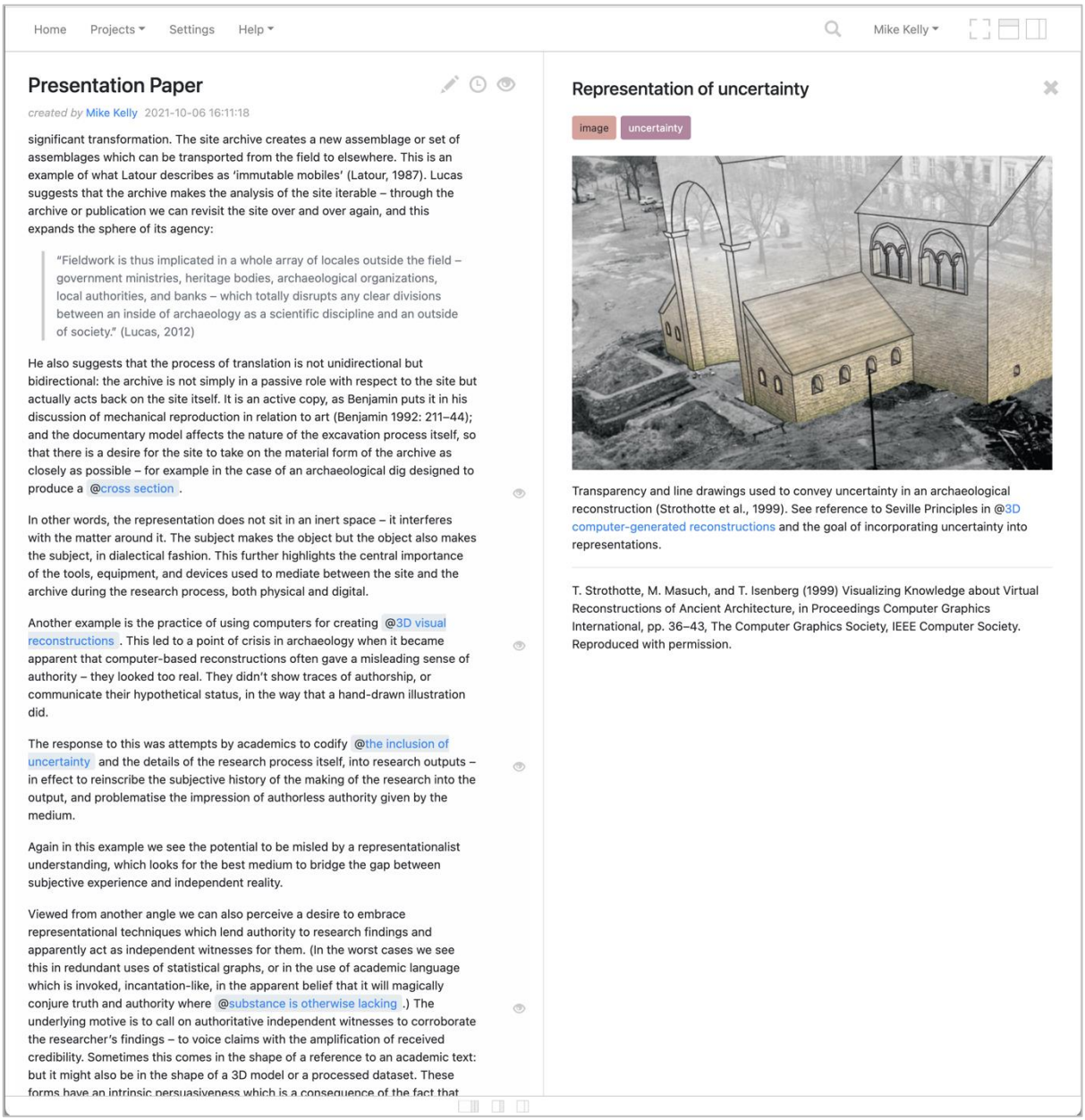

Figure 21. Conference presentation project in Orson, featuring final paper, with links to internal content embedded in the text and interactive preview displayed alongside.