Historiographical Approaches to Past Archaeological Research

Gisela Eberhardt
Fabian Link
(eds.)

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WRITING THE HISTORY OF ARCHAEOLOGY has become increasingly diverse in recent years due to developments in the historiography of the sciences and the humanities. A move away from hagiography and presentations of scientific processes as an inevitable progression has been requested in this context. Historians of archaeology have begun to utilize approved and new historiographical concepts to trace how archaeological knowledge has been acquired as well as to reflect on the historical conditions and contexts in which knowledge has been generated. This volume seeks to contribute to this trend. By linking theories and models with case studies from the nineteenth and twentieth century, the authors illuminate implications of communication on archaeological knowledge and scrutinize routines of early archaeological practices. The usefulness of different approaches such as narratological concepts or the concepts of habitus is thus considered.

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Gisela Eberhardt – Fabian Link

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Summary

For many years now, developments in the historiography of sciences and humanities have led to the call for a revised history of archaeology and a move away from hagiography and presentations of scientific processes as an inevitable progression. Historians of archaeology have begun to utilize approved and new historiographical concepts and tools to trace how archaeological knowledge has been acquired as well as to reflect on the historical conditions and contexts in which this knowledge has been generated. This volume seeks to contribute to this trend. By linking theories and models with case studies from the nineteenth and twentieth century, the authors illuminate implications of communication on archaeological knowledge and scrutinize routines of early archaeological practices. The usefulness of different approaches such as narratological concepts or the concept of habitus is thus considered.

Keywords: History of archaeology; history of science; historiographical approaches.

Ausgehend von Entwicklungen in der Geschichtsschreibung von Natur- und Geisteswissenschaften wird seit vielen Jahren auch eine veränderte Historiographie der Archäologie(n) gefordert und die Abkehr von Hagiographie und Darstellungen wissenschaftlicher Prozesse als unvermeidlichem Fortschreiten. Archäologiehistoriker/innen nutzen bewährte und neue historiographische Konzepte und Instrumente, um zu untersuchen, wie archäologisches Wissen erworben wurde, und um die historischen Bedingungen und Kontexte der archäologischen Wissensgenerierung zu reflektieren. Zu diesem Trend will der vorliegende Band beitragen. Die Autorinnen und Autoren verknüpfen Theorien und Modelle mit Fallstudien aus dem neunzehnten und zwanzigsten Jahrhundert, um Auswirkungen von Kommunikation auf den archäologischen Wissensprozess zu beleuchten und Routinen früher archäologischer Praktiken zu hinterfragen. Überprüft wird auf diese Weise der Nutzen verschiedener Ansätze wie narratologischer Konzepte oder des Habituskonzepts.

Keywords: Geschichte der Archäologie; Archäologiegeschichte; Wissenschaftsgeschichte; historiographische Ansätze.

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How to write the history of archaeology? For many years now, developments in the historiography of the sciences and the humanities have led to the call for a revised history of archaeology and a move away from hagiography and presentations of scientific processes as an inevitable progression. Historians of archaeology have begun to utilize approved and new historiographical tools in order to trace how archaeological knowledge has been acquired as well as to reflect on the historical conditions and contexts in which this knowledge has been generated. Thus, past achievements of the archaeological disciplines are no longer presented without historiographical reflection. It is understood that the goal of archaeology's history cannot be "to legitimize current practices by giving them a respectable ancestry"[1] and the risk of 'presentism' or 'present-centeredness', resulting in studies carried out from a present perspective with an according modern agenda, has been identified.[2]

Actual history of science, respectively of archaeology, is supposed to trace the production of scholarly knowledge instead of reviewing past research from a more advanced modern view or to extract normative guidelines for current research.[3] It aims at scrutinizing concepts and practices in light of their historical contexts, asks how discoveries were made and how they were identified or defined as such, how archaeological research categories developed, in what ways they were conditioned by social and political interests, or how specific topics were emphasized by biographical aspects, individual preferences or social interaction. However, in order to trace how archaeological knowledge has been produced and to reflect on the historical contexts in which this knowledge has been generated, it is important to carefully inspect the historiographical approaches, the models, theories and methods that are applied, and to discuss their merits and limitations in light of the specific needs of the historian of archaeology.

Still, there is an alternative perspective claiming that the history of archaeology is considered to be complementing theoretical discussion, critically assessing modern archaeological practices and enabling archaeologists "better to understand the orientation of current research and potentially enable changes."[4] This way of investigating historical sources is based on archaeological research, i. e. on modern scientific standpoints and current questions, and not on ideas of the past (that might no longer be central to modern research). Bert Theunissen therefore suggested to rather characterize those "as scientific criticism or scientific review than as history of science."[5] In order to tell standpoints apart it is indeed important to clearly distinguish studies aiming at normative guidelines for today's archaeology from actually historical analyses of past archaeological research.

1 Corbey and Roebroeks 2003, 1.
2 Kaeser 2008, 11.
3 For detailed information on objectives and trends in the history of science see Hagner 2001.
4 Gramsch 2006, 15. – Translation by the authors.
5 Theunissen 2001, 150.

Most of the authors' ideas collected here were first presented during a workshop in 2010, entitled "New historiographical approaches to archaeological research". The workshop explicitly focused on discussing methodologies and sought to raise questions concerning not yet approved approaches towards the history of archaeology. It thus aimed at exploring and possibly broadening the spectrum of available historiographical frameworks, concepts, and methods for novel histories of archaeological research.

However, the appropriateness of the term 'new' in the workshop's title has been questioned by the participants (contribution Serge Reubi).[6] It was claimed that the application of methods of literary studies (contribution Felix Wiedemann) or prosopography (contribution Amara Thornton), the consideration of social entanglements and communication structures behind scientific facts and processes following Ludwik Fleck (contribution Marianne Sommer), Bruno Latour's model of actor-networks (contributions Amara Thornton, Géraldine Delley) or Pierre Bourdieu's habitus concept (contribution Fabian Link) are not new but have been utilized in investigations for decades.

While this stands without question, it does not, however, hold true for the history of archaeology. Such models and theories were first applied to the history of archaeology only around the beginning of the twenty-first century, when the trend was to make history of archeology a more significant part of history of science. Since then historians of archaeology have regularly questioned the notions of continuous advancement and cumulative progress of concepts and knowledge in archaeological research. Accordingly, historians of archaeology have made various efforts to approach past research from different starting points and all sorts of new perspectives were embraced. Thus, new key aspects were discovered after the history of archaeology took its 'practical turn' at the beginning of the twenty-first century. In consequence, there have been various investigations, publications and conferences concerning the history and sociology of archaeological practices, notably field work practices.[7] In addition, with the adoption of new research perspectives, the scope of sources widened as well. For one, the research network "Archives of European Archaeology" was launched in 1999 claiming to more intensively investigate unpublished material since previous histories of archaeology had mainly been dealing with published sources of past research only. The network's primary focus is the exploitation of all sorts of "archives of the discipline", be it the unpublished material in libraries, museum depots or government archives.[8] Another kind of new sources introduced to the history of more recent archaeological research were oral histories, e. g. interviews with former staff members of particular excavations or senior scholars.[9] The revival of history of archaeology did not only offer new research opportu-

6 Reubi in this volume, esp. 225.

7 E. g. Lucas 2001; Davidovic-Walther 2009; Eberhardt 2011; Jensen 2012.

8 http://www.area-archives.org/index.html (visited on 07/07/2015); Schlanger 2002; Schlanger and Nordbladh 2008; Huth and Moro Abada 2013.

9 Smith 2006.

nities or open new source categories. Methodological issues were also raised anew taking up points from historiographies of other scientific disciplines as e. g. scientific biography as a research method.[10] This volume seeks to contribute to the presented trend by linking theories and models with case studies and rearrange the sets in which archaeological thinking is believed to develop.

Marianne Sommer discusses the implications of communication on scientific knowledge. She deals with the controversies regarding the scientific evidence of the so-called eoliths in eighteenth and nineteenth century archaeology, i. e. the question of whether these objects were archaeological artifacts or created by natural processes. Sommer follows Ludwik Fleck among others in explaining how the popularization of scientific knowledge is less a top-down phenomenon but rather a cycle which again generates scientific knowledge. Thus, Sommer shows how scientific objects, namely eoliths, came into being by verbal and visual communication, and by their incorporation into current thinking patterns such as evolutionary progression.

Irina Podgorny tracks back routines of early archaeological observation and documentation methods. How the practices of other fields not only influenced but also shaped the archaeological grip on evidence is presented in her contribution in detailed case studies. Political administrative forms, engineering drawings and medical perspectives could impact the ways in which archaeological features were seen and recorded. It becomes apparent how complex the origins of communicative practices are and how these practices – instead of being invented in matter-of-factly scientific strategies – regularly evolved out of habits and routines.

Amara Thornton presents a combined approach that consists of biography, prosopography and network analysis to identify the specific participants and members of early archaeology in twentieth century British Mandate Palestine and Transjordan. She broadens the scope of already known network categories such as disciplinary or gender-based networks attempting to meticulously encompass all parties that have been involved in defining and establishing the discipline of archaeology in early twentieth century Palestine and Transjordan. She thus explores how the wider archaeological network, including protagonists such as professional archaeologists, political authorities or private elites, operated.

Géraldine Delley researches the so called 'natural science methods revolution' in Swiss archaeology that is related to radiocarbon and tree-ring dating methods. She investigates the impact of these two methods on the research practice of the archaeology of ancient Swiss lake-dwellers between 1950 and 1985. Delley shows the profound changes that methods from the natural sciences provoked within Swiss archaeological research of the 1960s. However, these changes were primarily not rooted in a general modernization

10 Kaeser 2004; Link 2014.

in the sense of progress, but were influenced by other activities of scholars generally, e. g. mobilizing financial resources from politicians by applying certain rhetorical strategies.

How scientific objects come into being in archaeological research is the topic of Ulrich Veit's contribution. Drawing on Hans-Jörg Rheinberger's conception of 'experimental systems', Veit focuses on the case of Iron Age 'princely seats' *(Fürstensitze)*. In doing so, he presents a discourse that has undergone many years of discussion and critique within the German community from a previously unknown perspective. Veit traces how this epistemic object developed in several steps of knowledge transformation and reveals that the places of archaeological research are not scientific environments themselves, but results of concrete processes followed by researchers with different social groups involved.

Three case studies on the presentation and self-presentation of colonial archaeologists in Dutch East India during the 1920s and 1930s are explored by Marieke Bloembergen and Martijn Eickhoff. Referring to post-colonial theory, the authors investigate to what extent early archaeology continues to affect the archaeology of post-colonial Indonesia and whether the idea of colonial archaeologists as actual 'discoverers' of the prehistoric past remains valid until today. Bloembergen and Eickhoff are able to show that the creation of archaeological knowledge not only reproduced colonial hierarchies but included various forms of indigenous involvement as well.

Felix Wiedemann considers the possibilities of applying narratological concepts for studying the historiography of archaeology. Drawing on Hayden White, Paul Ricoeur and others, he uses for one the example of archaeological narrations from the nineteenth and early twentieth century on human migration in the Near East. Wiedemann analyzes how archaeologists arranged supposed historical events (such as migrations) within their accounts to arrive at coherent plots. The historical role that was ascribed to migrating groups or 'peoples' such as "founders or destroyers of human culture" relied less on archaeological findings but rather on the composition of a specific plots, subject to the political context of the time.

Fabian Link explores the epistemic changes in the scientific constructions of prehistoric archaeology from the 1930s to the 1960s, taking the example of the East German archaeologist Gotthard Neumann. For this purpose, he uses a combined approach of conceptual history and Bourdieu's field- and habitus-theory. Focusing on the impact of *völkisch* thoughts in Neumann's publications he argues that the importance of these ideas in prehistory was strongly linked with the social interactions Neumann had with Nazi politics but, primarily, with the professional success he had with this strategy.

In the closing contribution, Serge Reubi takes up the discussion about the alleged 'novelty' of the approaches adopted in this volume. Examining the differences between historiography of the natural sciences and historiography of the social sciences including archaeology, Reubi discusses the difficulties of establishing joint methodological stan-

dards due to the different research traditions of the two fields. In his view, most history of the social sciences is still concerned with normative ideas within one discipline. Such history – as for example the history of archeology – does not go beyond the space of established perspectives of the discipline under examination. Due to this "single-disciplinary approach" historians of archaeology are unable to escape the 'presentist trap'. Reubi sees this buttressed by the name of our workshop in 2010, when we identified approaches as "new" because we had in mind an 'isolated' history of archaeology instead of regarding archaeology as one field within a general history of science. He claims that historians of archaeology are to give up single-disciplinary approaches and should consider a broader view by embracing approaches from other disciplines and experts from a general history of science.

This book includes a wide range of concepts, from the history of experimentation in the life sciences to methods drawn from literary studies, and it is written by archaeologists, historians of modern history and historians of science. Its aim is thus to add to the demanded 'modernized' history of archaeology, that is, to a multi-disciplinary approach in researching the history of archaeology.

We would like to thank the Excellence Cluster Topoi for making the fruitful workshop possible on which the present book is based and for having the volume published. Within Topoi scholars examine the relation between spatial orders and knowledge in antiquity. Like all research enterprises, their projects are based on questions, methods and concepts established for decades within their disciplines. This was accounted for during the first phase of Topoi, when the research group CSG-V provided a platform for the investigation and discussion of the history of archaeology in general, and also a framework for our workshop.[11] Each of the contributions here have been peer-reviewed twice, and we are most thankful to all anonymous reviewers for their valuable feedback and comments that helped in improving the papers. Furthermore, many thanks are due to all participants of the workshop in 2010, especially the speakers Felicity Bodenstein, Stefanie Klamm and Pamela Jane Smith who did not see their papers through to publication. Last but not least we wish to thank Alison Borrowman, Joshua Crone, Will Kennedy, Nadine Riedl, Jutta Schickore and Dominika Szafraniec for their essential contribution to this volume with regards to content, proofreading, typesetting and organization.

11 The research group CSG-V (short for "Cross-sectional-group V") was a subsection of research area E in Topoi I (2007–2012): http://www.topoi.org/group/e-csg-v-topoi-1/ (visited on 07/07/2015). – Special thanks go to Kerstin Hofmann, coordinator and creative mind of the CSG-V, for her support in every respect.

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Marianne Sommer

(Net)working a Stone into a Tool. How Technologies of Serial Visualization, Arrangement, and Narration Stabilized Eoliths as Archeological Objects

Summary

This paper deals with issues surrounding so-called eoliths in the nineteenth and early twentieth century: Were these very crudely chipped stones from European Tertiary deposits really human-made? The focus is on the visual, spatial, and narrative arguments used by some of the eoliths-proponents. One powerful strategy consisted in integrating the supposed tools into existing geological, archeological, and paleoanthropological series, relying on established scientific knowledge and the wider cultural significance of the serial. However, the flints first had to be translated in cascades of inscriptions from actual stones in situ into drawings and series of drawings in publications to eventually gain a high level of abstraction as elements in formalized tables of juxtaposed series. My discussion of the eoliths focuses on these aspects in the production of knowledge in transit between communities, spaces, and media.

Keywords: History of archeology; history of paleoanthropology; knowledge circulation; eoliths; human evolution; visualization; serialization.

Dieser Beitrag setzt sich mit den historischen Kontroversen um die sogenannten Eolithen auseinander: Waren diese sehr rudimentär abgeschlagenen Steine aus europäischen Tertiärschichten tatsächlich das Resultat menschlicher Arbeit? Der Fokus ist auf die narrativen, visuellen und räumlichen Argumente einiger Eolithen-Verfechter gerichtet. Eine wirkmächtige Strategie war die Integration der vermeintlichen Werkzeuge in geologische, archäologische und paläoanthropologische Serien, um damit an etabliertes Wissen und an die kulturelle Bedeutung des Seriellen anzuschließen. Zuvor mussten die Feuersteine jedoch in Transkriptionskaskaden von Objekten *in situ* in Zeichnungen und serielle Abbildungen in Publikationen übersetzt werden, um schließlich den Abstraktionsgrad von Elementen in hoch formalisierten Tabellen einander gegenübergestellter Serien zu erreichen. In meiner Diskussion nehme ich diese Aspekte der Wissensgenerierung im Transit zwischen unterschiedlichen Gemeinschaften, Räumen und Medien ins Visier.

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Keywords: Geschichte der Archäologie; Geschichte der Paläoanthropologie; Wissenszirkulation; Eolithen; Evolution des Menschen; Visualisierung; Serialisierung.

I am grateful to the American Museum of Natural History for giving me access to the Osborn Papers in the Special Collections Library, and to the incredibly helpful staff of the archive. I would also like to thank the editors of the *Notes and Records of the Royal Society of London* for giving me permission to base this article on Sommer 2010. Special thanks are due to Gisela Eberhardt and Fabian Link for the organization of a wonderful conference in 2010 and the editing of the volume. The research for this contribution has been carried out within the project 'History Within: The Phylogenetic Memory of Bones, Organisms, and Molecules' that was financed by the Swiss National Science Foundation.

In 1921, the paleontologist and president of the American Museum of Natural History (AMNH), Henry Fairfield Osborn, ordered a series of supposed stone tools by letter from the English amateur archeologist James Reid Moir. Reid Moir had been digging in East Anglia and brought to light what he took to be human-made tools from Tertiary deposits. The human workmanship of such stones was very controversial, but by the time Osborn ordered the series, the crudely chipped flints, to which I will generally refer as eoliths, were at the height of their acceptance in the international scientific communities as human-made tools. Eoliths extended the antiquity of hominids in Europe from the Pleistocene into the Tertiary; they expanded the archeological record from the Paleolithic into the Eolithic. When he ordered a series, Osborn was in the last steps of preparing a new exhibition hall on human evolutionary history. The AMNH should not fall short of its British counterpart, the British Museum of Natural History, where the keeper of paleontology, Arthur Smith Woodward, had included eoliths in the exhibit as early as 1909.[1]

Ludwik Fleck has described the communication of scientific knowledge as integral to the formation of a scientific fact in his canonical *Entwicklung und Entstehung einer wissenschaftlichen Tatsache* of 1935.[2] The communication from specialists to non-specialist audiences goes along with the translation of the cautious formulation of a phenomenon in a scientific journal article into the objectifying and generalizing language of the textbook and popular text that harden the finding into a fact. Fleck regarded this process rather as a cycle than as a one-way transfer, because popular science forms the specific

1 American Museum of Natural History, Special Collections Library, Henry Fairfield Osborn Papers MSS O835 (hereafter AMNH, Osborn Papers), correspondence with J. Reid Moir, Box 15, Folders 15–17.

2 First translated into English in 1979 as *The Genesis and Development of a Scientific Fact,* eds.: T.J. Trenn and R.K. Merton, Chicago: University of Chicago Press.

public opinion and the worldview that influence the specialist as part of the wider culture. Fleck therefore already reinterpreted the process of popularization, which has since become a central concern for historians of science.[3] Except as an actor category, the term *popularization* has been largely abandoned in its nineteenth-century meaning of a one-way communication of objective scientific knowledge from its hermetically closed context of discovery to a diffuse mass of people who are in need of education. Popularization in this sense was understood as an instrument of social progress, of secularization and rationalization. While we still see in the communication of scientific methods and contents a factor of socio-cultural change, our understanding of the practices, sites, protagonists, media, and forms of representation that partake in the generation, communication, and adaptation of knowledge about the natural world has become considerably more complex and diverse.[4]

In his keynote lecture for the Three-Societies Meeting in Halifax of 2004,[5] James Secord has suggested to unite the diverse approaches in the history of science and science studies under the label "knowledge in transit".[6] The label not only suggests a symmetrical treatment of scientific knowledge production with popular and indigenous knowledge; it, too, goes along with an understanding of all science as a form of communication. We may object to this move by pointing to the fact that science in action is also about the lack of communication, about black-boxing processes and the materialization of theories and concepts in technological setups and natural phenomena as brought to light by scholars in the tradition of historical epistemology.[7] The communication of knowledge depends on the representation and re-representation of phenomena prior to circulation, with Latourian cascades of inscriptions that are as much processes of translation as the circulation of objects of knowledge between diverse geographical, social, and cultural spheres.[8] The lesson from a knowledge-in-transit approach remains, however, that we need to take account of the non-Western, the non-elite, and the non-male. The history of science since the cultural turn has expanded its sources to include the subaltern, the vernacular, and the market- and media-oriented products of popular culture as much as the so-called high culture of literature and the arts. We turn to the places and media where scientific practices, forms of representation, values, and ideas are communicated, negotiated, transformed, and rejected.

The AMNH is one such place, and my research into its history has shown how the collection of data, their interpretation, and the representations of the findings in expert and popular contexts all have to be seen as intimately linked; knowledge was indeed in transit: in movement, translation, and transition between people, spaces, and

3 Fleck 1936; also Fleck 1935.

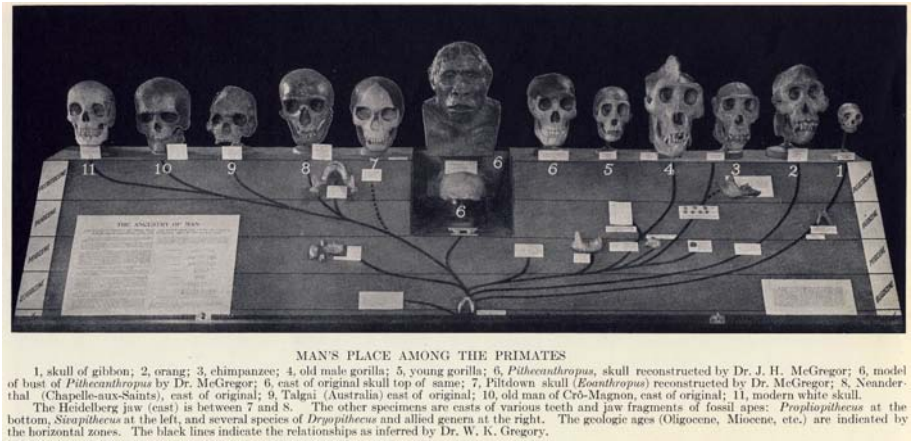
4 For a classical treatment, see Cooter and Pumfrey 1994.

5 The British Society for the History of Science, the Canadian Society for the History and Philosophy of Science, and the History of Science Society.

6 Secord 2004.

7 Rheinberger 1997.

8 Latour 1987, ch. 6.



MAN'S PLACE AMONG THE PRIMATES

1, skull of gibbon; 2, orang; 3, chimpanzee; 4, old male gorilla; 5, young gorilla; 6, *Pithecanthropus*, skull reconstructed by Dr. J. H. McGregor; 6, model of bust of *Pithecanthropus* by Dr. McGregor; 6, cast of original skull top of same; 7, Piltdown skull (*Eoanthropus*) reconstructed by Dr. McGregor; 8, Neanderthal (Chapelle-aux-Saints), cast of original; 9, Talgai (Australia) cast of original; 10, old man of Crô-Magnon, cast of original; 11, modern white skull.
The Heidelberg jaw (cast) is between 7 and 8. The other specimens are casts of various teeth and jaw fragments of fossil apes: *Propliopithecus* at the bottom, *Sivapithecus* at the left, and several species of *Dryopithecus* and allied genera at the right. The geologic ages (Oligocene, Miocene, etc.) are indicated by the horizontal zones. The black lines indicate the relationships as inferred by Dr. W. K. Gregory.

Fig. 1 Showcase in the Hall of the Age of Man at the American Museum of Natural History in the 1920s.

media. At Osborn's time, the AMNH was a site where humans, discourses, and objects from various social, cultural, and geographical contexts met in the production and consumption of knowledge about the natural world, and from where hybrid objects were internationally distributed. Among these were eoliths: They traveled as stones, as casts, as photographic representations and drawings, and re-represented in diagrams, tables, and narratives. As a result, when the Hall of the Age of Man finally opened to New Yorkers and visitors to the city in 1924, they did not encounter the contested history of the eoliths, or carefully phrased statements and hypotheses. They encountered rock-hard facts. They saw artifacts left behind by European hominids of the Tertiary. They also saw casts of fossil bones, busts of extinct hominids, and murals showing entire life-scenes from evolutionary history that were expertly set in communication with each other to provide a panoramic view and the necessary spectacle to engage the visitor attuned to the visual culture of the great exhibitions and circuses.[9]

Fig. 1 represents one of the pieces of the exhibit – if a rather drily didactic one. From this showcase, the visitor may have learned about man's place among the primates. The genealogy of the primates is a branching structure, and the line leading from the last common ancestor of all lines, *Propliopithecus* – here signified by a jaw –, to living humans is empty. There are no fossils to document our history and evolution from Oligocene times. But the message is ambiguous, because the horizontal series of skulls might be read as a descending ladder from modern white man (No. 11 in the legend), via Cro-Magnon, Neanderthal, Piltdown, *Pithecanthropus* (the bust in the middle – today called *Homo erectus*), gorilla, chimpanzee, and orangutan, down to the gibbon.

9 See Halttunen 2008.

In a special issue of the journal *History of Science*, Nick Hopwood, Simon Schaffer, and James Secord have brought together articles on the role of seriality in the long nineteenth century.[10] Series became important in periodical publication, production processes, and economic management. From their application in mathematics, the words *series*, *série*, and *Serie* entered philosophical and social programs, historical reconstructions, and the sciences of statistics, geology, paleontology, chemistry, botany, and anthropology. While the social, economic, and cultural was increasingly serially organized (such as in literary publication, workflows, travel experience, photography, and cinema), the natural, too, seemed to be serially structured. The question of the existence of progressive forces and developments in heaven and on earth troubled the sciences. In order to capture such spatio-temporal phenomena, researchers innovated and adapted serial iconographies: In geology, new visual techniques allowed to get beyond the mapping of the superficial distribution of minerals to seemingly render transparent vertical stratification; embryologists produced serial images of developmental stages; even electromagnetic phenomena were communicated in series of images documenting serial experimentation. However, as the papers collected in *History of Science* render evident, iconic and narrative seriality was not simply suggested by the scientific practice or the natural phenomena under investigation. Rather, processes of translation, manipulation, and abstraction were involved in the production of iconic and textual – or mobile – series. This suggests that the power of series was such that scientists worked hard to exploit their appeal. They developed serial visual rhetoric for making spatial and temporal successions compelling, not only in communication between experts but also to larger publics. Serialized forms of imagery and publication, series of museum displays, and serial exhibition of objects drew on and encouraged the serialization of everyday experience.[11]

But series were never uncontested. This is inscribed in the AMNH showcase. The contradiction between the two messages of, on the one hand, a bushy family tree and, on the other, a scala naturae arrangement of the horizontal skull series captures something significant about the time. Around the turn to the twentieth century, the paleoanthropological and archeological communities mostly abandoned the linear view of evolution held by their predecessors; partly due to the discovery of many more fossils, they adopted branching models of human descent. This did not mean, however, that they also abandoned all notions of progressive development.[12] Correspondingly, Constance Areson Clark has convincingly shown that a public trained to literacy in the visual language of progress – in which the series played a crucial role – may have read images as conveying linear progressive development even where such was not consciously communicated.[13] Furthermore, as illustrated by the primate tree, the visual rhetoric of the

10 Hopwood, Schaffer, and Secord 2010.
11 Hopwood, Schaffer, and Secord 2010, 251–285.
12 Sommer 2007, Part II.
13 Areson Clark 2001.

series was not abandoned when the ancestral line of living humans was cleared from fossil remains, which were relegated to sidelines; rather, we will see how series were made to work differently, a process that rendered them dangerously ambiguous. The same points may be made with regard to the verbal representation of human evolutionary history that strongly relied on literary genres and cultural tropes that facilitated the rendering of evolutionary history as a series of progressive stages. However, narratives were much more contextual and idiosyncratic than Misia Landau has suggested with her reduction of evolutionary scenarios to an arrangement of a fixed set of elements in a single narrative structure.[14]

In general, anthropologists and archeologists in the early decades of the twentieth century continued to use many of the tools of the trade established in the nineteenth century. They drew evolutionist analogies between stages of biological, cultural, and mental evolution, and between so-called primitive or savage and prehistoric peoples and cultures. It is only through this continuity that artifacts of a certain prehistoric culture could still stand in for a fossil human type. The early split between the anthropoid and hominid lines, visualized in the primate tree of Fig. 1, was justified in the exhibition guidebook by supposedly human-made tools from the European Miocene – by eoliths. It was the tool-making ability of Dawn Man – as the hypothetical human ancestor was called – that put at a distance the cultureless ape. In other words, different series – the geological, archeological, and paleoanthropological series – could mutually reinforce each other and fill each other's gaps. In order for series to do this powerful work, however, eoliths first had to be translated in cascades of inscriptions from actual stones in situ into elements in highly formalized tables of juxtaposed series. Series circulated through the international networks of archeologists and anthropologists in several degrees of abstraction from the typological tool series to the column of archeological cultures in context. In agreement with the analytical turns towards the visual, the narrative, the spatial, and the performative, my discussion of the eoliths therefore focuses on these aspects in the production of knowledge in transit between communities, spaces, and media.[15] The series is key here, because serialization is a technology of visual narration that performs compelling arguments for spatio-temporal processes.

In this way, in following the process of (net)working stones into tools, I hope to shed new light on the history of eoliths as an example of how scientific knowledge is produced, circulated, and in transformed, but also stabilized, in the interaction between different scientific communities. Previous engagements with the discourses around eoliths have aimed at the unraveling of a forgery,[16] the explanation of the resolution of a controversy within the history of Paleolithic chronology and classification,[17]

14 Landau 1991.
15 Bachmann-Medick 2009.
16 Spencer 1988.
17 O'Connor 2003; O'Connor 2005; O'Connor 2007, ch. 5.

or at an ontology of current views, even the understanding on the basis of cognitive science, specifically cultural cognition,[18] with the processes of visualization, narrativization, and spatialization playing a marginal role at best. From my different perspective, I begin with the work of Gabriel de Mortillet to illustrate how the eoliths profited from the persuasive power of the serial in Western cultures. I then turn to Great Britain to focus on Reid Moir's detailed work of translating eoliths into stones marked by human intention and integrating them into particular typological, archeological, geological, and production series. At the end, I return to the American scene and ask what kind of work the eoliths and their serial integration performed in the novel paradigm of a branching hominid phylogeny in which no known fossils were considered ancestral to modern humans.

1 Gabriel de Mortillet: The first eoliths and the performance of progress in France

If the year 1859 brought a consensus with regard to the coexistence of humans with the extinct Pleistocene fauna, the rejection of a conservative Biblical timeframe for the age of humankind opened up vast spaces of time for investigation. How far back could human history be traced? In France, the acceptance of human antiquity was followed by a controversy about eoliths, supposedly human-made flint tools from Tertiary deposits. Gabriel de Mortillet (1821–1898) of the École d'Anthropologie in Paris was the most prominent supporter of the dawn tools and the creator of the term *Eolithique*, the Dawn Stone Age, which for him referred to the Tertiary period. But de Mortillet is most remembered today for his classification of the Paleolithic industries as a progressive series from the Chellean or Acheulean, to the Mousterian, Solutrean, and finally to the Magdalenian. By the time de Mortillet accepted the supposedly Eolithic stone tool cultures, his archeological system was already in place.

That the archeological series was part of a larger scheme becomes evident in the prehistoric section of the Universal Exhibition in Paris of 1867, for which de Mortillet had been responsible. The series of world fairs organized in the Western metropolises of the nineteenth century itself epitomized stages in the progress of industry and empire. In the case of the history of industry *(l'histoire du travail)* at the Parisian Universal Exhibition, the exhibits occupied the first concentric ring around the central garden in the oval exhibition building. Each nation filled a section of the ring, with the most important nations appearing first and being allotted larger sections.[19] Within the territory of each nation,

18 Ellen and Muthana 2010.

19 See image at http://commons.wikimedia.org/wiki/File:Exposition_universelle_de_1867.png?uselang=de (visited on 07/07/2015).

the halls were ordered chronologically, from prehistory to the present. From the main entrance, the visitor could either enter Great Britain or France, but de Mortillet used the guide he wrote for the prehistoric exhibits to suggest turning left and beginning the tour with France (moving clockwise).[20] Here, France's technological progress unfolded before the visitor's eyes from the hall *La Gaule avant l'emploi des métaux* to those celebrating recent innovations. Within the prehistoric part of this progress, the halls represented the Paleolithic, Neolithic, dolmen, lake-dwelling, Celtic, Gaul, and Gallo-Roman periods. Within the Stone Age hall, artifacts were again arranged chronologically on the basis of archeological sites.

Thus, de Mortillet led the visitors on a tour through the inner exhibition circle during which the prehistoric epochs were repeated in national sections, pointing to the parallel development in different geographical regions. He also directed the guide readers towards more current technologies and customs found in the exhibits of the civilized nations that indicated continuity in form or use beyond prehistory. At the same time, he drew attention to the galleries on the colonies of France and of other European nations. Here, he referred the visitors to the similarities with objects from Western prehistory – illustrating the possibility that the universal technological development may take place at different times for different peoples. In other words, de Mortillet used his guide narrative, the architecture of the exhibition, and the serial arrangement of the exhibits in space to enact for the visitors what he conceived of as the great laws of human evolution. They should witness the law of universal human progress, the law of similar developments in all human races, and the great antiquity of humankind. In doing so, de Mortillet emphasized that *l'histoire du travail* illustrated by the progressive series in material cultures signified a respective mental and anatomical progress.[21]

In his guide through the 1867 exhibition, de Mortillet assured the reader that the French committee had taken particular care to exclude from the Paleolithic exhibits any object the origin or authenticity of which was doubtful. There was therefore no trace of Tertiary Man, such as had been brought forward by Jules Desnoyers in 1863. But after the Universal Exhibition, the reports grew, and some of the Tertiary stones claimed to have been shaped by an intelligent toolmaker were accepted by French prehistorians of great renown. These de Mortillet included in *Le Préhistorique: Antiquité de l'homme* of 1883 as positive proof of Tertiary Man in Europe. He explained that "[f]ollowing an excellent method applied in geology, – one is not to forget that paleoethnology is directly derived from geology, – I have given each period the name of a very typical site […]".[22] Thus, the geological series literally became the series of cultural stages. De Mortillet therefore

20 Mortillet 1867.

21 On the exhibit see also Schlanger 2006.

22 "Suivant une excellente méthode adoptée en géologie, – il ne faut pas oublier que la paléoethnologie découle directement de la géologie, – j'ai donné à chaque époque le nom d'une localité bien typique […]." Mortillet 1883, 29, my translation in main text.

described not only the human industries and their distribution from the *Eolithique* to the *Neolithique*, but also gave an account of the geology, fauna, flora, and of possible fossil human remains, for each epoch. In other words, he verbally painted the grand story of human evolution as the geological, paleontological, paleoanthropological, and archeological series in parallel progression. This method pointed to a void in the paleoanthropological series vis-à-vis the Eolithic cultures, because de Mortillet rejected the human remains that had been reported for Tertiary deposits. However, where there were tools, there must have been a shaper. De Mortillet therefore invented *Anthropopithecus*, a missing link between the highest anthropoid ape and the lowest savage that had fashioned the eoliths from France and Spain and that had evolved into the Neanderthals and eventually the Cro-Magnons.

As Michael Hammond has described in a by now classical paper, de Mortillet's linear view of human evolution was strongly interwoven with his politics.[23] He extrapolated the prehistoric progressive series of biological, cultural, and mental development to an inevitable historical succession from the reign of the nobility, to the reign of the bourgeoisie, and finally to the reign of the socialists. In other words, the eoliths fitted well into the pattern of lawful series found to prevail in geology, anthropology, archeology, as well as history that were internationally commemorated in such events as the Universal Exhibition.

2 James Reid Moir: The production of series and the serial production of Eoliths

The British, too, had had their reports of eoliths, and the controversy became most heated around the work of the Ipswich amateur archeologist James Reid Moir and his allies. Those who did not accept the human workmanship of the eoliths generally brought forward the following set of objections that denied the integration of eoliths into meaningful series: Paleontologically, an Oligocene and Eocene hominid appeared to be an impossibility due to the state of evolution of the entire mammalian branch at this early epoch. Geologically, forms identical to the so-called eoliths could be picked up from many a modern beach or gravel. Paleoanthropologically, those eoliths that were taken from older strata were not part of a human settlement or shelter, but integral to geological formations, themselves often thrown violently into place. Technically, sea waves, river torrents, and ice sheets, sudden changes of temperature, pressure or compression through landslides, folding, etc. were observed to produce eoliths naturally. Moreover, eoliths could be reproduced mechanically and were even among the spontaneous products of a cement-mixing machine.

23 Hammond 1980.

The eoliths-proponents answered the critique on the one hand by focusing on human intentionality. The question of human design had already been foremost for de Mortillet, who treated the visible traces of human intentional action on a stone in some detail. There can certainly be made an argument for the evidential strengths of single tool representations. The simplicity, regularity, and repetitiveness in design that were associated with the traces of human intention on stone were visually formalized for paleoliths, and relied on by the eoliths-proponents in their visual arguments for artificiality. There exists considerable scholarship on visualizations of entire prehistoric life scenes with regard to their persuasiveness for certain theoretical stances, their conservatism, and their gender and race stereotypes. There has been far less analysis of the history of lithic visualization. The development of a universal language in lithic drawing, the pervasiveness and advantages of drawing over photography, and the role of the concept-content of images in the history of archeology still present promising research questions.[24]

But even more strongly than the particularity of an individual tool, a series could help a stone type's establishment as human artifact. This is a strategy that Reid Moir made wide use of. Of particular importance in Reid Moir's textual and visual arguments was a specific type of eolith: the so-called rostro-carinate. It was an invention of Ray Lankester, the former Oxford zoologist and director of the British Museum of Natural History.[25] During the initial decades of the twentieth century, Reid Moir and Lankester discovered and described eoliths from below the Upper Pliocene marine deposit referred to as Crag, which covers a considerable part of East Anglia.

The rostro-carinate was so central for the integration of the Tertiary tools into the existing classification of sites and technologies because it could function as a missing link. In order to refute the general belief that the Sub-Crag eoliths had no cultural relationship to the paleoliths, Reid Moir experimentally produced a typological series from the earliest eoliths to the earliest paleoliths. The intermediate stages of the process of fashioning paleoliths from eoliths he identified with actual stones ('tools') found at different sites that he arranged in an analogous series. In the sense developed by Steven Shapin and Simon Schaffer, this experimental and evidential re-enactment of tool-type evolution could be virtually witnessed by means of visual representation and distribution in renowned scientific journals.[26]

Fig. 2 is a schematic representation of a rostro-carinate that Reid Moir copied from Lankester which emphasizes its characteristic carina (keel) and beak. Fig. 3 shows how it is produced: A flake is detached from a potato-shaped flint to produce the ventral plane, blows are then applied at a particular angle to both sides of the surface to form the keel, finally the ventral side is flaked to achieve a concave form. Fig. 4 is a representation of the

24 Lopes 2009.
25 Lankester 1912.
26 Shapin and Schaffer 1985.

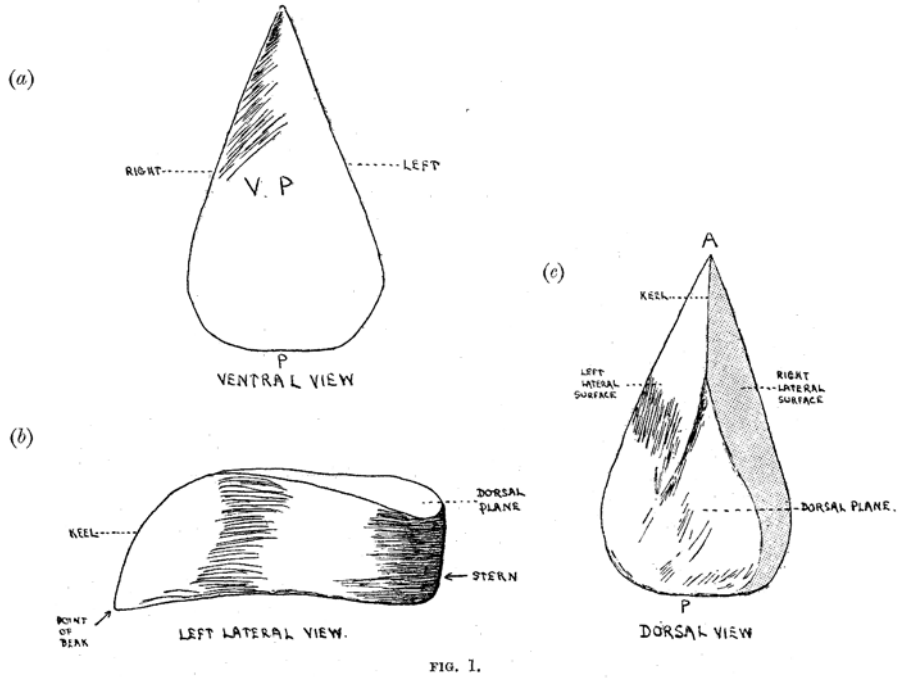


Fig. 2 ‚Rostro-carinate' eolith according to Reid Moir and Lankester.

typological evolution from the earliest Eolithic types into the rostro-carinate illustrated on the basis of actual 'tools' – it shows the same general steps as the experimental production, that is the flaking of the carina and the beak. Reid Moir wanted to demonstrate that the rostro-carinate type existed at different stages of refinement that characterized different Eolithic cultures. Moreover, he believed that the finished rostro-carinate could be flaked into a Paleolithic tool type to provide an entire series. Again Reid Moir came up with a series of actual stones that matched his experimentally produced stages. Figures 5 to 14 represent such a series of stones from various sites that showed bilateral flaking to form a beak, flaking of the stern, flaking of the ventral and dorsal planes, and then the gradual extension of the keel until it met the stern. The next steps were represented by Paleolithic tools called Chelles, in which Reid Moir still recognized a keel.[27]

27 Reid Moir 1916.

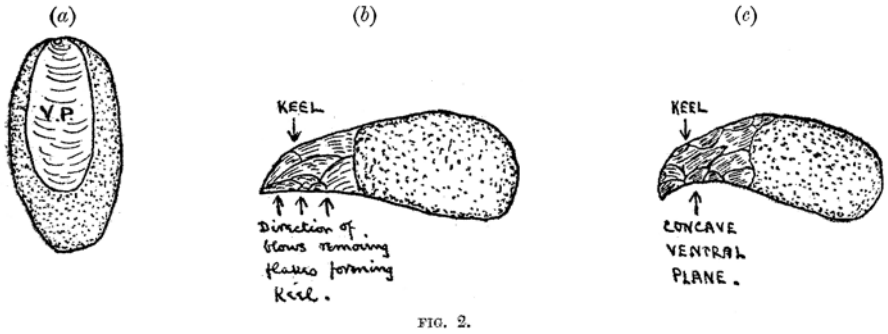


Fig. 3 Experimental production of rostro-carinate.

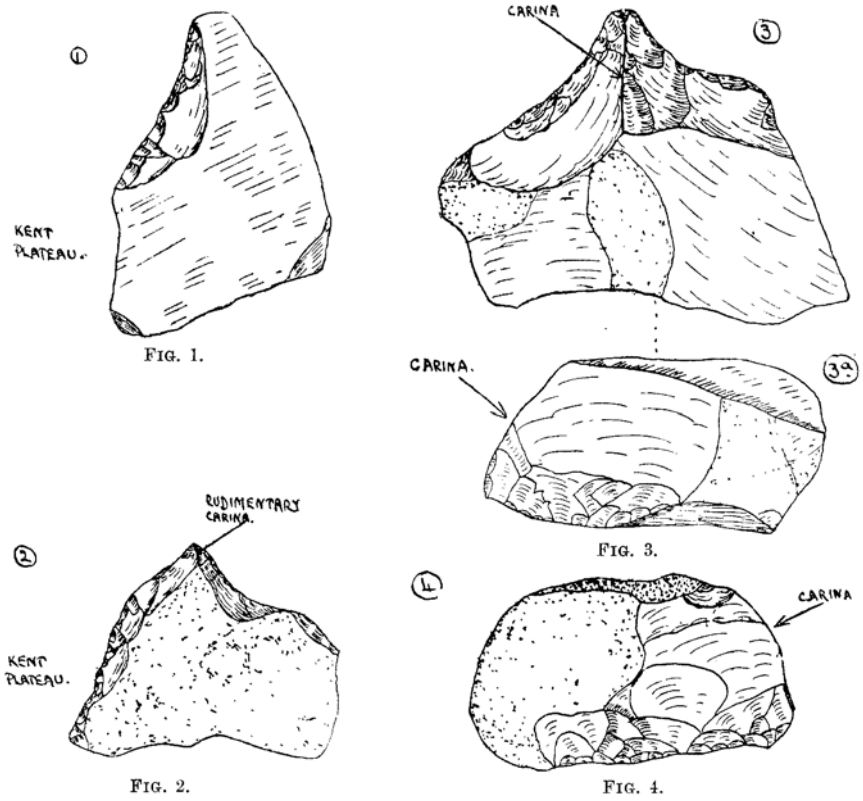


Fig. 4 Alleged precursors of the rostro-carinate form.

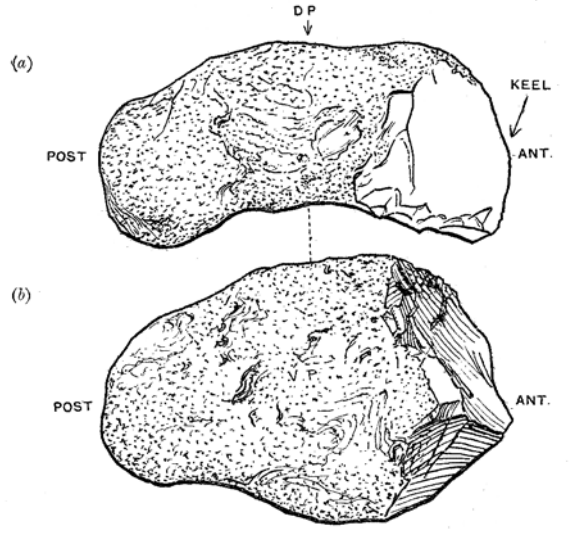


FIG. 3.—VIEW OF (*a*) RIGHT LATERAL SURFACE ; (*b*) VENTRAL PLANK OF MOST PRIMITIVE TYPE OF ROSTRO-CARINATE IMPLEMENT. ($\frac{2}{3}$ NATURAL.)

Fig. 5 After Reid Moir.

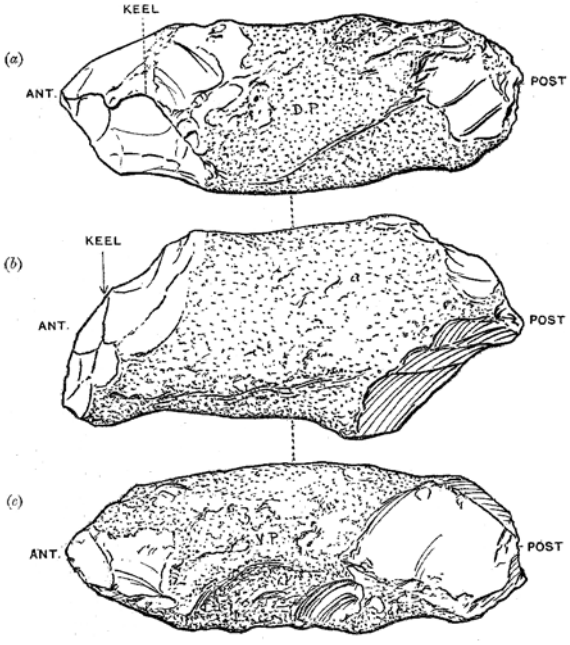


FIG. 4.—VIEW OF (*a*) DORSAL SURFACE ; (*b*) LEFT LATERAL SURFACE OF ROSTRO-CARINATE IMPLEMENT SHOWING SECOND STAGE OF EVOLUTION ; (*c*) VENTRAL SURFACE. ($\frac{2}{3}$ NATURAL.)

Fig. 6 After Reid Moir.

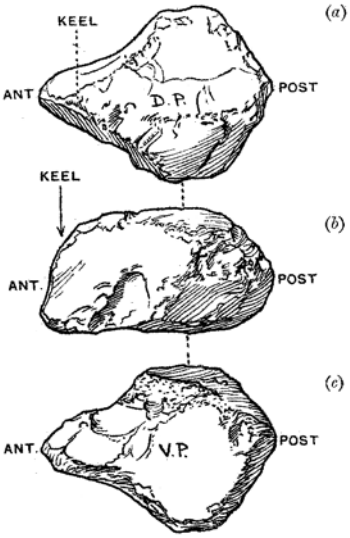


FIG. 5.—VIEW OF (*a*) DORSAL SURFACE ; (*b*) LEFT LATERAL SURFACE OF ROSTRO-CARINATE IMPLEMENT SHOWING THIRD STAGE OF EVOLUTION ; (*c*) VENTRAL SURFACE. (⅔ NATURAL.)

Fig. 7 After Reid Moir.

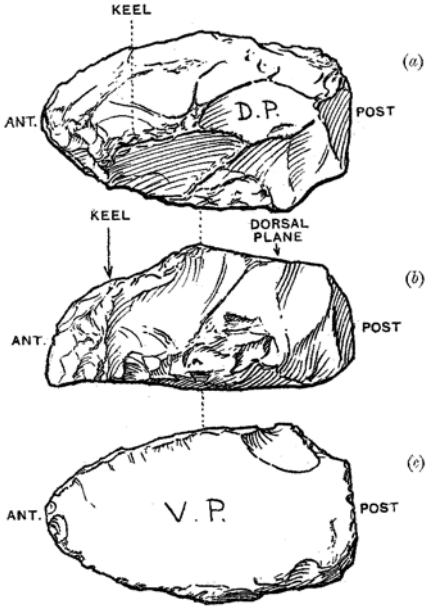


FIG. 6.—VIEW OF (*a*) DORSAL SURFACE ; (*b*) LEFT LATERAL SURFACE OF ROSTRO-CARINATE IMPLEMENT SHOWING FOURTH STAGE OF EVOLUTION ; (*c*) VENTRAL SURFACE. (⅔ NATURAL.)

Fig. 8 After Reid Moir .

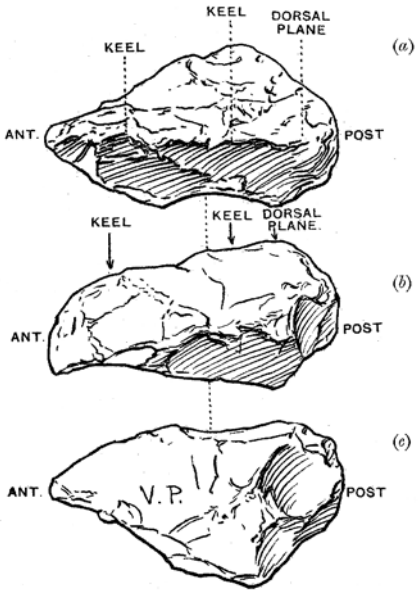


FIG. 7.—VIEW OF (*a*) DORSAL SURFACE; (*b*) LEFT LATERAL SURFACE OF ROSTRO-CARINATE IMPLEMENT SHOWING FIFTH STAGE OF EVOLUTION; (*c*) VENTRAL SURFACE. ($\frac{2}{3}$ NATURAL.)

Fig. 9 After Reid Moir.

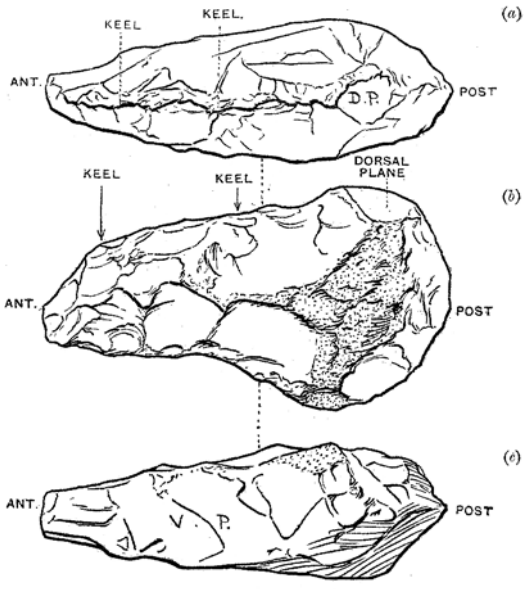


FIG. 8.—VIEW OF (*a*) DORSAL SURFACE; (*b*) LEFT LATERAL SURFACE OF ROSTRO-CARINATE IMPLEMENT SHOWING SIXTH STAGE OF EVOLUTION; (*c*) VENTRAL SURFACE. ($\frac{2}{3}$ NATURAL.)

Fig. 10 After Reid Moir.

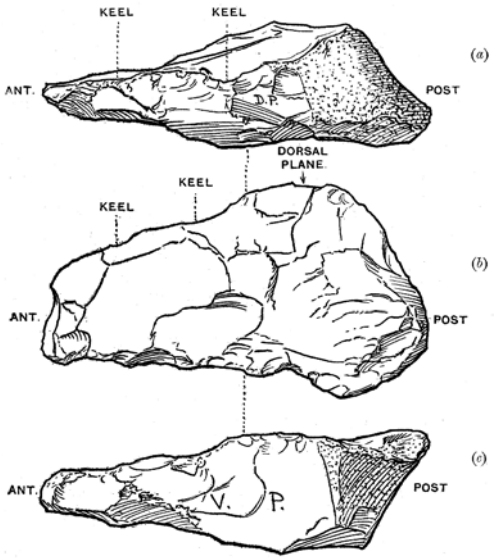


FIG. 9.—VIEW OF (*a*) DORSAL SURFACE; (*b*) LEFT LATERAL SURFACE OF ANOTHER ROSTRO-CARINATE IMPLEMENT SHOWING SIXTH STAGE OF EVOLUTION; (*c*) VENTRAL SURFACE. (⅔ NATURAL.)

Fig. 11 After Reid Moir.

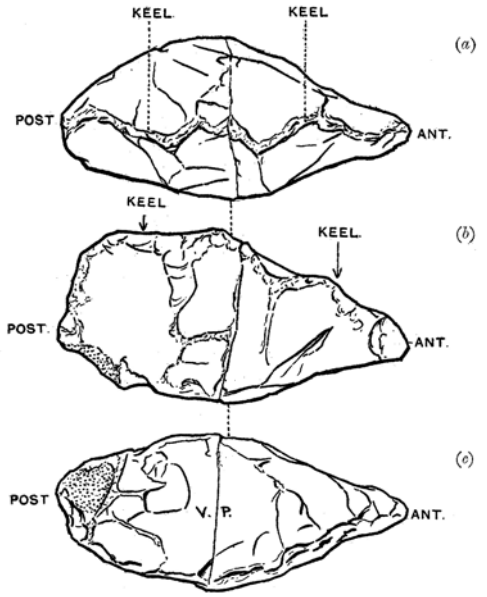


FIG. 10.—VIEW OF (*a*) DORSAL SURFACE; (*b*) RIGHT LATERAL SURFACE OF ROSTRO-CARINATE IMPLEMENT SHOWING SEVENTH STAGE OF EVOLUTION; (*c*) VENTRAL SURFACE. (⅔ NATURAL.)

Fig. 12 After Reid Moir.

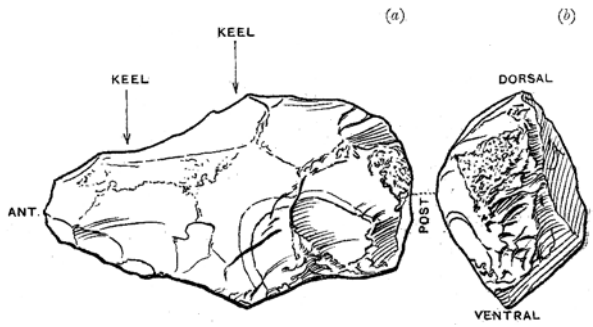


FIG. 11.—VIEW OF (a) LEFT LATERAL SURFACE AND (b) STERN (RHOMBOIDAL IN SECTION) OF EARLY CHELLES PALÆOLITHIC IMPLEMENT, SHOWING FORM EVOLVED FROM ROSTRO-CARINATE. (⅔ NATURAL.)

Fig. 13 After Reid Moir.

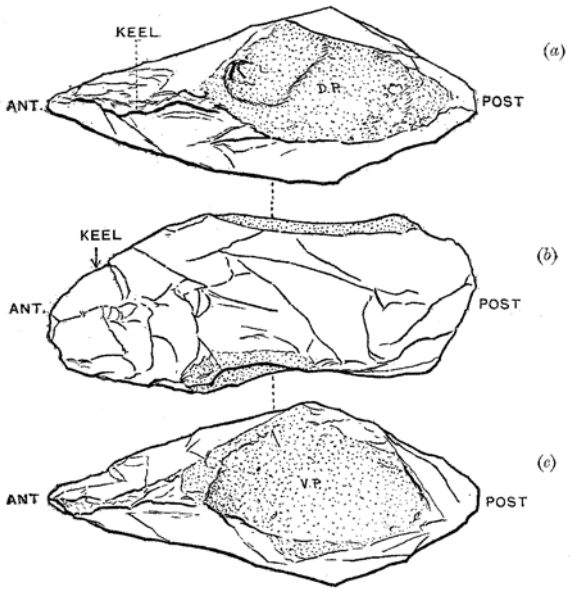


FIG. 12.—VIEW OF (a) DORSAL SURFACE; (b) LEFT LATERAL SURFACE OF PALÆOLITHIC IMPLEMENT OF CHELLES TYPE SHOWING ADVANCE ON PREVIOUS FIGURE; (c) VENTRAL SURFACE. (⅔ NATURAL.)

Fig. 14 After Reid Moir.

Reid Moir's production of evidence through serialization gained strong support when he discovered what seemed to be a Pliocene workshop actually containing different stages of the tool-shaping process from the rostro-carinate to the earliest Chellean types.[28] It was in fact this new evidence of an in situ series that convinced the great French archeologist Henri Breuil of the human workmanship of these tools, and that led to a peak of eoliths acceptance in Europe and the U.S. Of course, the experimental

28 Reid Moir 1921.

FIG. 1.—*Stratigraphical Table of the Implementiferous Deposits of East Anglia.*

Geological Periods.	Climate.	Deposits.	Cultures.
Recent.	Temperate. ...	Surface soil and latest alluvium of valleys	Neolithic and later.
P	? Cold	Deeper levels of alluvium in valleys ...	Magdalenian and ? Solutrean.
L	Glacial	Brown Boulder Clay, Hill Washes, and ? Flood Plain Gravel	Derived Implements.
E	Warm in earlier part.	Floors in lateral valleys, Ipswich	Aurignacian and Mousterian.
I	Glacial	Upper Chalky Boulder Clay	Derived Implements.
S	Inter-Glacial ...	Brickearths and Gravel, Hoxne, High Lodge and Ipswich	Clacton III and late Acheulean.
T	Glacial	Kimmeridgic Chalky Boulder Clay and ? Contorted Drift	Derived Implements.
O	Inter-Glacial ...	Corton and ? Mundesley Sands ("Middle Glacial")	?Lower Acheulean.
C	Glacial	Cromer Tills and ? Norwich Brickearth ...	Derived Implements.
E	Inter-Glacial ...	Cromer Forest Bed, ? Norwich Crag in part, and ? Foxhall horizon	Chellean and Pre-Chellean.
N	Increasing Cold ...	Red Crag	Some derived Implements.
E		Suffolk Bone Bed	Eolithic.
Pliocene	Warm	Coralline Crag.	Some derived Implements.
		Suffolk Bone Bed	Eolithic.
Eocene	London Clay.		

The Red Crag and the Suffolk Bone Bed.

Fig. 15 Stratigraphical Table of East Anglian deposits containing implements after Reid Moir.

and typological series had ramifications beyond the archeological series. Reid Moir further abstracted his serial representation of a serial production and analogous evolution process into tables with parallel columns. In these highly schematized representations, the overlapping arguments from series – geological, cultural, typological, and processual – were played out simultaneously.

However, one column is conspicuously missing from Reid Moir's table shown in Figure 15: the one showing the fossil hominid remains. Reid Moir was aware that in this visual argument the placement of fossil human bones in the column adjacent to the tool cultures at the same level as the Eolithic industries would be the strongest sup-

port for their human workmanship. He therefore was not content with the introduction of fossil-less taxa, as de Mortillet had done, but set out to find the remains of an eoliths-shaper. As early as October 1911, he thought his wish had come true, when a partial human skeleton was found beneath the Chalky Boulder Clay near Ipswich. Reid Moir sent the bones of the possible maker of the Suffolk eoliths to no lesser authority than the anatomist Arthur Keith, conservator of the Museum of the Royal College of Surgeons. Although Keith found that the bones were of a modern anatomy, could not be said to be fossilized (mineralized), and still contained a comparatively high percentage of organic matter, he concurred with Reid Moir's ascription of the skeleton to the Pliocene.[29] At least the anatomical features fit into the growing belief that hominids of a relatively modern body had existed much earlier than so far suspected. Ipswich Man never gained general acceptance, but as the belief in eoliths grew, many scientists tried to fill the void in the paleoanthropological column. There was first of all Piltdown Man, a spectacular discovery made in a late Pliocene or early Pleistocene deposit at Piltdown, in Sussex, in 1911 and 12. Piltdown Man was a forgery; but it took decades to expose the bones as that of a modern human skull and an orangutan jaw.[30] Besides Piltdown Men, Homo erectus-like races were proposed as having roamed Pliocene England. In his book *The Antiquity of Man in East Anglia*, Reid Moir, too, suggested the necessity of a paleoanthropological series matching the archeological one:

> So far as actual evidence of man's former presence goes, we have in East Anglia, as those who have read these pages will, I think, agree, a wonderfully complete record of nearly every stage in human progress from the earliest and most primitive flint implements, to the advanced types made at the close of the Stone Age. Thus, it is possible, that what is now England was the home of the earliest men, and there can be little doubt that if a tithe of the money spent upon researches in other parts of the world were expended upon archaeological work in Eastern England, still further and more important discoveries, bearing upon the question of man's origin, would be made.[31]

The progressive series of archeological cultures through the series of geological layers referred to the existence of a series of hominid types that must have improved step by step in anatomy and mind. That such a success story had taken place on the soil of Reid Moir's home country made English archeology appear like a patriotic duty. However, such interpretations of local series were by then contested. In fact, it was especially this aspect that had been strongly opposed by eoliths-skeptics such as the famous French paleontologist Marcellin Boule. Due to their migratory model of human evolution, they

29 Reid Moir and Keith 1912.
30 Sommer 2008.
31 Reid Moir 1927, 162.

had no problem accounting for the sudden appearance of a relatively sophisticated culture in Europe, the Chellean, without any precedent. They had no need of eoliths and their derivates.[32] To the contrary, for Reid Moir and Keith, the search for the remains of modern Englishmen in Pliocene deposits was embedded in the idea that each modern human race had evolved a long time ago in the area where it was now found.

While the evolutionary scenarios that emphasized migration often drew direct parallels to historical and contemporary processes of imperialism, Keith might well have been motivated by a desire to distance the 'European races' from non-European ones – and ultimately the English from the rest of Europe – by providing them with long parallel evolutionary lines. During the war years, Keith began to develop the theory that human evolution had been driven by racial conflict; he even suggested that current nations were in a race-formation process. Lankester, on his part, seems to have envisioned the shapers of the eoliths as pertaining to the Nordic master race.[33] In his 1912 paper, he speculated that the Tertiary tools of Suffolk and Kent were made at a time when England was still connected to Scandinavia by a land bridge. The Pliocene races might thus have reached England from the very north of the European continent. Clearly, the idea of a Tertiary toolmaker in England flattered British national pride: "There is, perhaps, no other part of the world richer in remains of our remote ancestors than that of Suffolk and Norfolk […]."[34]

3 Henry Fairfield Osborn: Eoliths and a story of serial progress free from apish stain

When the eoliths traveled to America, the paleontologist and president of the AMNH, Henry Fairfield Osborn, eventually became so enthralled with the visions of prehistory they held that he financially supported Reid Moir's research and used his tight network with the English and French communities to help stabilize them as tools.[35] This move coincided with the apex of the tendency of thinking of modern human anatomy in terms of a great antiquity. Osborn eventually made the hominid line bypass even that of the anthropoids. In the United States, evolutionary theory and Osborn in person were attacked by William Jennings Bryan and like-minded in the upsurge of religious fundamentalism, and the bulk of the spite was directed at the 'ape theory' of human origins. In combination with Osborn's own religious background, much has been made of this context by historians of anthropology in explaining Osborn's Dawn Man theory as a strategy to soften protest and to gratify his own desire for the compatibility of religion

32 Boule 1905.
33 Sommer 2007, 197–212.
34 Reid Moir 1927, Preface.
35 AMNH, Osborn Papers; Sommer 2010.

and evolution. In this view, it was for religious and political reasons that Osborn freed human ancestry from the stain of the ape and the primitive.[36]

However, Osborn's extreme Dawn Man theory was rather the apotheosis of preceding international tendencies than an altogether local and idiosyncratic phenomenon. It was furthermore in line with a set of theories that located the origin of the hominid branch in Eocene lemuroids or tarsioids.[37] Although the specific American religious context is important for this acumination, there are other developments that need to be taken into account. Osborn did not immediately embrace the English eoliths and the Piltdown fossils – those hallmarks of speculation about a dawn-age human ancestor of relatively modern anatomy. In the book *Men of the Old Stone Age* of 1915,[38] he only included a quick note on the tools found in Europe and claimed to be of Tertiary age. He shared the doubts of the great men of archeology.

By the time Osborn changed his mind about the supposedly Tertiary European bones and tools, several developments had converged. He had become aware of a quasi-modern horse in the Pliocene. Drawing inferences from the paleontological on the paleoanthropological series, this Pliocene horse alerted Osborn to the possibility of hominids of a relatively modern anatomy in this early epoch. Furthermore, in 1917, bone fragments of a second Piltdown Man had been discovered that swerved general opinion in favor of acceptance, and that Osborn went to examine in the British Museum in 1921. In the summer of the same year, Osborn visited the British sites in East Anglia and Suffolk, and Reid Moir's discovery of a workshop containing a production series finally did its part in convincing him of the existence of Tertiary Man. On his return to New York, Osborn made this conviction public in *Natural History*,[39] and soon thereafter, it was confirmed by the leaders of the archeological community, Breuil and Louis Capitan. From there, the acceptance of eoliths grew to a peak and the East Anglian tools found their way into the Hall of the Age of Man and the Archeological Hall at the American Museum of Natural History. Osborn eventually published newspaper articles to create a stir among those who still clung "fondly to the ape ancestry theory". It was only now that he pushed to their conclusion the dawn-man theories that were associated with European Tertiary Man as toolmaker.[40]

When Osborn expelled the ancestors of the great apes from the hominid line, the branch leading to modern humans became long indeed; and there were no fossils left to

36 Areson Clark 2008, ch. 6, here especially 115–116; Gould 1989; Rainger 1991, 231–232; Regal 2002, particularly 154–173.

37 Drinker Cope 1893, 316–335; Wood Jones 1919; Wood Jones 1929.

38 Osborn 1916 [1915].

39 Osborn 1921; also Osborn and Reeds 1922.

40 AMNH, Osborn Papers, correspondences with J. Reid Moir (Box 15, Folders 15–17), N. Nelson (Box 16, Folder 11), G. E. Smith (Box 20, Folders 15–16), and A. Smith Woodward (Box 23, Folder 31); for the quote see letter from H. F. Osborn to J. Reid Moir, February 1, 1927, Box 15, Folder 16. – Reid Moir did not agree that his discoveries contradicted a common ancestry for apes and humans (Feb. 5, 1927).

occupy it. However, this did not amount to a denial of evolution – as has sometimes been suggested. There was nothing a priori 'creationist' about this move. As we have seen, anthropologists searched for the fossil remains of Dawn Man and used non-ancestral hominids as models for ancestral ones of a more distant age. Most importantly, the eoliths and their evolution were the strongest precarious 'evidence' for the dawn men and their ascent. This is where series retained their persuasive power.

Osborn's *Man Rises to Parnassus: Critical Epochs in the Prehistory of Man* of 1927 was designed as another edition of *Men of the Old Stone Age*. However, because Osborn had "witnessed proofs of the existence of intelligent man and his flint culture over 1,250,000 years ago", a new title seemed in order. Osborn's conception of human evolution was so processual that it was theatrical; he made it unfold in front of the reader's eyes in a series of acts that successively fleshed out the parallel geological, archeological, and paleoanthropological series for each horizontal layer. To this purpose, he used Aeschylus' description of the progressive development of human reason, language, and the practical arts and sciences in "Prometheus Bound" – his account of man's gradual rise to Parnassus – as a structuring device.[41] For good reason, the book is not titled 'Man's Rise to Parnassus', but *Man Rises to Parnassus*. Its form imitates the Greek drama, including prologue and epilogue. The rising of man towards the top of Parnassus is re-enacted as driven by demigods like Prometheus, by the pioneers and innovators of humankind.

As a mechanism for man's gradual rise, Osborn suggested a steady increase in intelligence in a process of co-evolution with tool technology. He traced the insights into the role of a trained hand in mental development back to Anaxagoras:

> Expressed in modern terms, manual training is one of the modes of mental training. In this sense the use of the hand becomes one of the causes of the development of the brain. In my own observation, in the enormously long period of the Stone Age the working of flint tools was the chief stimulus to the working of the mind. So there is a strong prehistoric argument for this thought of Anaxagoras.[42]

This is where the Tertiary tools from East Anglia came in that were now given a full chapter. Osborn in fact based the first acts in his drama of human evolution mainly on the eoliths. In absence of fossil evidence of Tertiary humans, Osborn's performance of the Dawn Man drama only worked if tool cultures could stand in for hominid types. Osborn therefore experimented on his own tool typological evolution – or more precisely, he forced his curator Nels Nelson to do so. Fig. 16 is a beautiful series of archeological cultural layers combined with the attempt to establish evolutionary lines throughout the typologized series. And just like the fossil evidence was inferred to reach back to an

41 Osborn 1927a, ch. 1.

42 Osborn 1927a, 11.

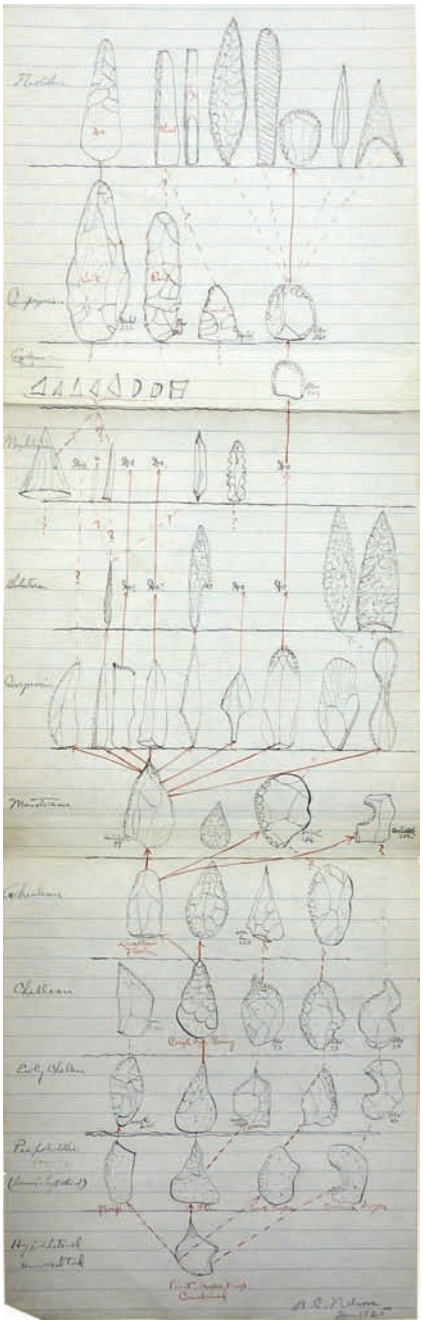


Fig. 16 "Rough scheme suggesting some of the possible genetic relationships of the successive levels of typical stone implements found in Western Europe". Drawing by Nels Nelson.

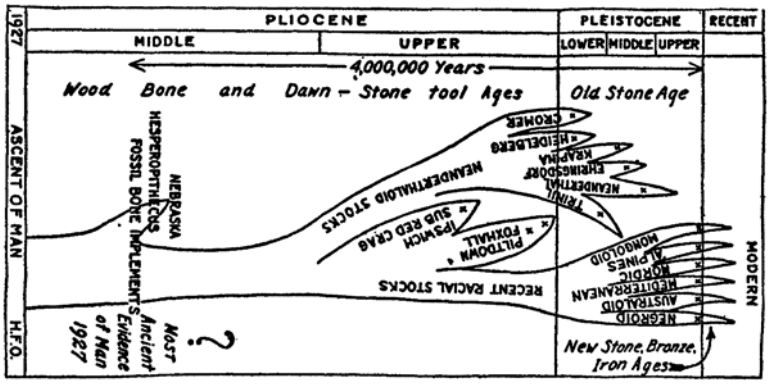


FIG. 1.—Prehistoric and recent racial stocks. Neanderthal stocks (left); partly known Pliocene stocks (center); six Pleistocene and recent racial stocks (right). (Below) Level of the supposed fossil bone implements of the Hesperopithecus quarries in Nebraska—possible evidence of Middle Pliocene bone-tool age in America.

Fig. 17 Visualization of prehistoric and recent racial stocks by Osborn.

as-yet unfound Dawn Man, at the beginning of this tentative cultural evolution stands a hypothetical universal tool. That there was a connection to the by now tree-like structure of hominid phylogeny becomes clear from the Osbornian imagery. In Fig. 17 the "[p]rehistoric and recent racial stocks" are inferred in certain cases from osseous remains and in others from archeological finds.

The inference of hypothetical dawn men from Eolithic cultures depended on the intertwining of series that we have seen carried out in France as well as England. In Osborn's table that was included in the 1924 guide to the Hall of the Age of Man, a cultural, a racial, and a paleontological series were visually brought together, so as to make the viewer mentally substitute the gaps in one column with the content from both or one of the other two (Fig. 18). If such images did not suggest to the museum visitors parallel progressive lines of parallel progress in culture, anatomy, and environment, I know not what would have. Of course, as Figures 16 and 17 illustrate, for Osborn, these columns did no longer represent simple evolutionary series. His ascent of man was complicated by his viewing every material evidence of hominids as a kind of shadow of what had already happened on the line leading to living humans. Nothing seemed good enough for Dawn Man.

But even as shadows of true ancestors, Osborn wanted to rehabilitate the prehistoric human types. His advocacy of their manual dexterity and correlated mental prowess on the basis of eoliths was itself a spiritual quest. The long search for the bones of the perfect, large-brained Dawn Man, to fill the void created by the Eolithic cultures in our direct ancestry, was never achieved; but there was hope in his shadows:

STONE CULTURES		HUMAN RACES	CONTEMPORARY MAMMALS
HISTORICAL		PERIOD	EXISTING MAMMALS
NEOLITHIC		PERIOD	MASTODON(?)MAMMOTH
AZILIAN MAGDALENIAN SOLUTRIAN	PALEOLITHIC	GRENELLE CRÔ-MAGNON	
AURIGNACIAN		GRIMALDI	
COLD MOUSTERIAN		NEANDERTHAL	REINDEER MAMMOTH WOOLLY RHINOCEROS
WARM MOUSTERIAN			ELEPHAS ANTIQUUS HIPPOPOTAMUS
COLD ACHEULEAN			
WARM ACHEULEAN		KRAPINA	
LATE CHELLEAN		EHRINGSDORF	
CHELLEAN			ELEPHAS ANTIQUUS HIPPOPOTAMUS
EARLY CHELLEAN			ELEPHAS ANTIQUUS
		HEIDELBERG	RHINOCEROS ETRUSCUS HIPPOPOTAMUS SABRE-TOOTH
CROMERIAN			ELEPHAS PRIMIGENIUS MUSKOX
		PILTDOWN?	REINDEER
FOXHALLIAN			

Fig. 8. Sequence of Old Stone Age (Paleolithic) in Europe.
The order in which the races of primitive men appeared in Europe and the most striking mammals living at the same time.

Fig. 18 Osborn's 1923 sequence of Palaeolithic men and animals.

On Sunday morning, July 24 [1921], after attending a most memorable service in Westminster Abbey, the author repaired to the British Museum to see the fossil remains of the now thoroughly vindicated Dawn Man of Great Britain [i. e. Piltdown Man, M.S.]. The few precious fragments of one of the original Britons,

> which had been preserved in a steel fireproof safe from the bombs thrown by German aviators and which will probably be thus guarded from thieves for all future time, were taken out and placed on the table by Smith Woodward [keeper of palaeontology at the BMNH and strong promoter of Piltdown and eoliths], so that full and free opportunity was given for the closest comparison and study.[43]

This scene of worship at both the religious and the scientific altar represents the climax of Osborn's quest for the origin of human spirituality. The scene at once makes clear that religion and science are not at odds; that his scientific search for truth is inspired by a belief in God. But scientific truth will not be obstructed by religious fundamentalism, or by enemies of civilization such as the Germans, and certainly not by common thieves. The scientific fetish presented to Osborn on the museum's altar is palpable evidence of the victory of scientific reason over religious superstition and human barbarism. The relic of Tertiary Man – with his large brain case – suggests the noble history also of the direct human line, man's steady rise to Parnassus. The house in which it is worshipped is a house of science that stands for equal opportunity, openness, and democratic exchange in a common search for knowledge. But despite this hopeful tenor, the events in world history cast a doubt on the optimistic universal progressive series; a doubt that is audible in Osborn's *Man Rises to Parnassus*: Will the human races continue to rise each to its own capacity? Or will the current interbreeding of types, the lack of struggle in the modern environment, or the puncture of this tranquility in the brutality of war, continue to sap man's virility, as foreign influence had degraded Neanderthaloid culture, as the lush jungle habitat had once kept back the apes, and as some prehistoric tribes had been extinguished by others? At stake was the next stage in human serial ascent.

4 Finis

From the times of de Mortillet, eoliths had been incorporated into pre-existing notions of technological progress, celebrated for example at the great expositions. They were transferred from a very controversial status to a short life as scientific facts through the hard work of translation by English paleontologists and archeologists who gave them strength through incorporation into series: typological series, production series, cultural series, and geological series that themselves were transfused by the notion of linear progress in culture, body, and mind. This idea of progress as structuring the history of life, and human life in particular, lost some of its power towards the end of the century. Simultaneously, scenarios of human evolution began to take the shape of 'trees'

43 Osborn 1927a, 52–53.

with many dead-ending branches. In the dawn-man theories, there were no fossils to animate the long surviving line. This at first glance seems to signify a vehement breakdown of the serial argument. However, as in the case of Osborn, inferences about the human ancestral line could be made from Eolithic cultures, from non-ancestral fossils that could stand in as models for earlier ancestral ones, and also still from 'primitive living humans.'

In spatial arrangements, verbal performances, and visual representations of human evolutionary history, the parallel progressive series continued to structure an overall steady progress propelled in a mutual catalyzing between environment, tool-invention and -fashioning, motor skills, intelligence, and psychology. This not only hints at some continuity in scientific argument and thought. The retaining of verbal and visual strategies from the old paradigm also increased the problem of unambiguous knowledge transition, especially to wider publics, as I have discussed at the beginning of the paper for the primate tree in the Hall of the Age of Man showcase. Finally, from their beginnings, eoliths were not purely epistemic, but also political objects. They became enmeshed in views of the prehistoric past that carried diverse but strong lessons for the present: the inevitable succession of political systems in socialist aspirations, the long history and noble identity of European nations increasingly in competition, and the warning against interbreeding and other supposedly negative consequences of modernization. These histories and their incumbent futures were themselves serially structured; the spatialization, narration, and visualization of series of objects, events, and developments on all levels from tool typology to universal progress mutually reinforced each other and gave the eoliths the evidential power that fed back into the series.[44]

44 Sommer 2010; Sommer 2004.

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Illustration credits

1 Osborn 1923, 5. Courtesy The American Museum of Natural History. **2** Reid Moir 1916, 198, Fig. 3. Courtesy Wiley-Blackwell. **3** Reid Moir 1916, 199, Fig. 2. Courtesy Wiley-Blackwell. **4** Reid Moir 1917, 42. Courtesy Wiley-Blackwell. **5** Reid Moir 1916, 200, Fig. 3. Courtesy Wiley-Blackwell. **6** Reid Moir 1916, 201, Fig. 4. Courtesy Wiley-Blackwell. **7** Reid Moir 1916, 202, Fig. 5. Courtesy Wiley-Blackwell. **8** Reid Moir 1916, 203, Fig. 6. Courtesy Wiley-Blackwell. **9** Reid Moir 1916, 204, Fig. 7. Courtesy Wiley-Blackwell. **10** Reid Moir 1916, 205, Fig. 8. Courtesy Wiley-Blackwell. **11** Reid Moir 1916, 206, Fig. 9. Courtesy Wiley-Blackwell. **12** Reid Moir 1916, 207, Fig. 10. Courtesy Wiley-Blackwell. **13** Reid Moir 1916, 208, Fig. 11. Courtesy Wiley-Blackwell. **14** Reid Moir 1916, 209, Fig. 12. Courtesy Wiley-Blackwell. **15** Reid Moir 1935, 344. Courtesy Wiley-Blackwell. **16** Henry Fairfield Osborn Papers, Box 16, Folder 11, MSS 0835. Courtesy The American Museum of Natural History, Special Collections Library. **17** From Osborn 1927b, 482, Fig. 1. Reprinted with permission from AAAS. **18** Osborn 1923, 32, Fig. 8. Courtesy The American Museum of Natural History.

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Towards a Bureaucratic History of Archaeology. A Preliminary Essay

Summary

This paper shows that the protocols for observing and recording employed by different bureaucratic departments of state administration were fortuitously incorporated into the practices of several disciplines, including archaeology. Circulars or questionnaires, inventories and records, the French model for post-mortem medical examinations, and the protocols used by topographers, pilots, and military engineers moved from bureaucracy to scientific practice. Thus, objects were brought into collections having been formatted by procedures inherited from diverse traditions of state administration, construction, or commercial procedures.

Keywords: Post-mortem medical examinations; archaeological recording; military engineers; instructions; Spanish monarchy; bureaucracy; political curiosity.

Der Aufsatz zeigt, dass in staatlichen Administrationen angewendete Verfahren der Beobachtung und der Aufzeichnung auf zufällige Weise in die Forschungspraktiken verschiedener Wissenschaften eingingen, so auch in die Archäologie. Rundschreiben oder Fragebögen, Bestandslisten und Berichte, das französische Modell medizinischer Post-mortem-Untersuchungen sowie Verfahren, die Topographen, Piloten und Militäringenieure verwendeten, gingen aus der bürokratischen in die wissenschaftliche Praxis über. Die archäologischen Objekte, die in die Sammlungen kamen, waren somit (wissenschaftlich) aufbereitet mittels Verfahren, die ursprünglich für die staatliche Verwaltung, die Konstruktionstechnik oder die Wirtschaft entwickelt worden waren.

Keywords: Medizinische Post-mortem-Untersuchungen; archäologische Aufzeichnung; Militäringenieure; Anleitungen; spanische Monarchie.

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1 Introduction

In the early 1950s, André Leroi-Gourhan described prehistory as practiced by three kinds of prehistorians: the professionals (*préhistoriens de métier*), the *grands amateurs*, and the *petits amateurs*, the latter being the most abundant group, composed of priests, physicians, university professionals, teachers, workers, boy and girl-scouts, students, and young people in general. Leroi-Gourhan concluded: "Notre milieu de préhistoriens est donc un milieu foncièrement composé d'amateurs dont la formation scientifique est très variable."[1] According to him, professionals and vocational scientists should work together following the instructions set by metropolitan institutions or professional archaeologists. And indeed, Leroi-Gourhan published his seminal work *Les Fouilles préhistoriques* with the explicit goal of providing such a set of instructions. However, as Courbin remarked: "À Pincevent même, A. Leroi-Gourhan a commencé par utiliser les coupes résultant de l'ancienne exploitation de la sablière."[2] Thus, excavation techniques and procedures used in the operation of a quarry have determined what archaeologists could observe and how they were observing, a situation that, far from being unique, can be found in many episodes of the history of archaeology. Subtle drifts, unexpected transfers, and contingencies shape scientific practices.[3] This is one of the reasons that have led historians of science to look for unexpected articulations as a way to understand scientific change.[4] In this framework, the history of procedures and protocols has assumed a central role in a historiography that examines the forces that shape knowledge through technical media and the repetition (or emergence) of the programmed gestures.[5]

Whereas the attempts to standardize scientific observation by instructions has been the subject of research in fields such as botany or anthropology,[6] less attention was paid

1 Leroi-Gourdan 1950, 1.
2 Courbin 1987, 328, referring to the year 1964.
3 Cf. Coye 1997; Rowley-Conwy 2007.
4 Cf. Rheinberger 1997; Galison 1997.
5 Kittler 1985, see also Blair 2010.
6 Bourguet 1997; Kury 1998; Puccini 1995.

to the impact that the existing expertise, material conditions or the training that 'amateurs' received in other disciplines had on archaeological practices. Far from being "uneducated people," many of them, as Leroi-Gourhan admitted, were actually trained in how to organize facts and data in other fields. Thus, Patrick McCray argues that amateurs or vocational scientist cannot be treated as merely passive collectors of data.[7] Undoubtedly, instructions shaped the way in which they organized data and objects. However, the way in which these instructions related to the collectors' former training and, more importantly, how this training and the existing order of information shaped the professional practices of modern archaeology, if at all, still deserves further research.

Around 1900 archaeologists addressed the crucial role of record keeping as "the absolute dividing line between plundering and scientific work, between a dealer and a scholar".[8] In that sense, objects had to be properly recorded, collected, stored, linked to those recorded facts that give them historical and scientific value; if not, museums would simply be houses of "murdered evidence".[9] In modern archaeology, creating data became a procedure for grouping and locating objects, both in the fabric of excavation and in the repository of artifacts. However, this linkage of things took place 'on the move'.[10] Antiquities and fossils, for instance, were traded and introduced into the circulation of goods by several agents: physicians, priests, military engineers, bureaucrats, consuls, quacks as well as local and traveling experts. All of them, for ordering what they collected, automatically appealed to what they had learnt to do as part of their everyday practices. This commerce shaped the ways of collecting, storing, and classifying objects as well as a new remote scientific space where scientists depended on personal networks that included many local people engaged in other activities, such as colonial bureaucracy, the Church, or medicine. Thus, physicians described objects following their medical observational protocols; military engineers and field workers used theirs to give sense to things or objects not described before.

While for many years this was happening in a contingent but quite performative way,[11] by the late-nineteenth century the bureaucratic system of recording was incorporated into archaeological practices defining the essence of archaeological method.[12] Excavation and recording began to be taught in universities and systematized in handbooks for students and professionals. Around 1900, several handbooks were published presenting the field as a space to be controlled by the archaeologist, who was defined as the ever-present excavation supervisor. Once archaeologists started organizing the professional teaching of field practices, they considered themselves responsible for a task that required an "engineering training of mind and senses" and the "combination of

7 McCray 2006, 636, see notes 5 and 6 for specific literature on archaeology.
8 Petrie 1904, 48.
9 Cf. Podgorny 2008.
10 Cf. Appadurai 1986.
11 In the sense of Tanner 2008.
12 Petrie 1904.

the scholar and the engineer, the man of languages and the man of physics and mathematics".[13] This was not expressed as an outlook, but reflected something that was already happening: the systematization of the techniques for recording and grouping facts and objects. Archaeological recording combined the descriptive skills of the scholar with the protocols of surveying and legal medicine as well with the methods of book-keeping and accounting, including listing, inventories, and catalogues.[14] Thus, protocols of observation, grouping, and the description of 'what is before the eye' actually originated in state or private administration. Along with these techniques, archaeological data collection methods appear to be connected with the bureaucratic (colonial) system and its instructions on what and how to observe. In order to analyze the emergence of this fundamental relationship between objects and recording, this essay, inspired by the work of Spanish historians of science from the last thirty years and German media historians Wolfgang Schäffner,[15] Bernhard Siegert,[16] and Arndt Brendecke,[17] will first refer to the role of management of information and bureaucracy in the Spanish Empire.[18] Subsequently, three paradigmatic procedures will be taken into consideration in order to show a constellation where bureaucratic practices and manual expertise contributed to make visible new objects from the South American past: 1. military engineering and 2. post-mortem medical examination and 3. land administration and transportation of antiquities.

2 Bureaucracy and media history

Whereas Harold Cook has been analyzing the Dutch experience to study the connections between the rise of global commerce and the development of global science,[19] German media historians Wolfgang Schäffner and Bernhard Siegert have proposed to look at the *Casa de Contratación* (established in Seville in 1503) and the Council of the Indies (1520) as two of the institutions connected with the emergence of modern knowledge and the reliable gathering of "experience" and data.[20] Far from the 'protestant values' and Puritan ethos, beyond the social origins of the members of the Royal Society,[21] Schäffner and Siegert analyzed how bureaucrats and bureaucratic devices that emerged in the Spanish Monarchy shaped a new way of both assessing what reality was and governing what the king would never see with his own eyes. This kind of 'telemathic rep-

13 Petrie 1904, 3 and 33.
14 See, among others, te Heesen 2005.
15 Schäffner 1999; Schäffner 2001; Schäffner 2002.
16 Siegert 2000; Siegert 2003; Siegert 2006; see also Siegert and Vogl 2003.
17 Brendecke 2009a; Brendecke 2009b; Brendecke 2009c; Brendecke 2010; Brendecke 2011; Brendecke 2012.
18 López Piñero 1979; Pimentel 2003.
19 Cook 2007.
20 Schäffner 2001; Siegert 2003, in particular Part 1, 'Die Große Bürokratie', ch. 'Bürokratie und Kosmographie in Spanien 1569–1600'.
21 Cf. Shapin and Schaffer 1985.

resentation,' based on bureaucratic media of transmitting data from the New World to Spain, created new kinds of evidence.[22] Inspired by media philosopher Friedrich Kittler and by the seminal work of the Spanish historian of science, José María López Piñero,[23] Schäffner und Siegert turn functionaries and devices of the *Casa* and the Council – maps, reports, instructions, memoranda – into key actors in the making of modern Europe. Paraphrasing Bernhard Siegert, whereas until the sixteenth century, governance was possible only by the presence of the king, in Spanish America, information media from the Casa de Contratación, namely a space controlling bureaucracy, took the place of the Sovereign.[24] Furthermore, Arndt Brendecke has focused on the Spanish Monarchy in order to understand the crucial relationship between "Empirie-Gebrauch und kolonialer Herrschaft".[25] Thus, in current historiography the rise of modern knowledge is primarily a result of the development of modern commerce as well as the Spanish colonial administration with their procedures and protocols. Brendecke's investigation is founded on two basic premises:

> First, we assume that the process of European expansion had a formative influence on the emergence of the modern European culture of empirical knowledge. Colonial rule intensified the need to produce reliable descriptions of remote realities, hence, to systematically acquire empirical knowledge, to legitimize it by means of standard methods or authorities and to arrange it in such a way that decisions could be made on that basis in Europe. At first this task was performed not by scientists but, in the case of Spain in particular, by royal officials – "bureaucrats," if you will. This leads us to the second assumption, which is that significant elements of the modern culture of empirical knowledge can only be understood in relation to the practices of dominion and administration that took shape during the period of expansion and colonization.[26]

The Casa and the Council, on the other hand, are deeply connected with the expansion of (rag) paper as the reliable medium for recording, transmitting, archiving, and finally

22 Schäffner 1999; Schäffner 2001; Mundy 2000.

23 López Piñero 1979.

24 Siegert 2003, 67–68.

25 Brendecke 2009a.

26 Brendecke 2009a, 12. – Translation by the author. Originally: "Erstens wird davon ausgegangen, dass der Vorgang der europäischen Expansion die Entstehung der modernen empirischen Wissenskultur Europas prägte. Kolonialherrschaft verschärfte die Notwendigkeit, verlässliche Beschreibungen entfernter Wirklichkeiten zu produzieren, also Empirie systematisch zu erfassen, durch feste Methoden oder Instanzen zu autorisieren und so aufzubereiten, dass man auf dieser Basis in Europa Entscheidungen treffen konnte. Diese Aufgaben wurden zunächst nicht von Wissenschaftlern erledigt, sondern, gerade im Falle Spaniens, von königlichen Amtsträgern, von 'Bürokraten', wenn so man will. Es wird deshalb zweitens angenommen, dass signifikante Elemente der modernen empirischen Wissenskultur nur dann zu verstehen sind, wenn man sie in Bezug zu den Herrschafts- und Verwaltungspraktiken stellt, die sich in der Expansions-und Kolonialzeit herausbildeten."

governing of data.[27] In a recent lecture, James Secord has recalled that the history of paper manufacture is generally seen to belong to the mundane history of everyday technology, an approach that diminishes the fact that the circulation of the raw materials of literary production are potentially important elements in achieving knowledge.[28] If knowledge and 'paperwork' went together,[29] this connection was particularly relevant in the realms of the Spanish Monarchy, where paper was established as a medium of distant administration.[30] Knowing and governing was linked to ways of remote witnessing, the creation of reliable forms of transmission of data and experience, and its accumulation and processing in Seville/Cádiz, for many years not only the most important Spanish administrative centers but also the relays of the commerce with the Indies and Genoa, the Atlantic, the Mediterranean, and the Pacific.[31]

In particular, the systematic collecting as well as the making knowledge available to the court and the Council began in the early 1570s, with the creation of the position of the Major Cosmographer-Chronicler of Indies and an ordinance decreeing that every functionary of the Crown in the Americas was committed to the permanent description of those territories.[32] But Brendecke states:

> In structural terms, though, one can say that in an expansive empire, such as that of Spain, knowledge at the center failed to keep pace with the empire's growth. On the contrary: that portion about which the sovereign had immediate and personal information grew ever smaller, the number of mediation processes ever greater. Thus the importance of mediality increased, i. e., of processes of mediation on the part of human agents (officials, visitadores, viceroys) and media (reports, witness statements, geographical maps). In their different ways, they promised to provide access to that which was remote, but they also produced a particular 'mediacy' that cut the sovereign off from direct knowledge of his empire.[33]

27 On the history of expansion of paper in Europe and in the Americas, see Balmaceda 2004; Burns 1981; Calegari 1986; McCrank 1993; Giry 1925; Thiel 1932; also Siegert 2003; Siegert 2006.

28 See Jim Secord, „Darwin on Paper: From Rags to Wood-pulp", Institute's Colloquium, Max Planck Institute for the History of Science (Berlin) on March 18 2014, also in Uppsala on May 25, 2014: http://www.vethist.idehist.uu.se/index.php/seminars/page/60/eng/ (visited on 07/07/2015).

29 Latour 1990.

30 Schäffner 2002; Siegert 2003; Brendecke 2009c.

31 H. Chaunu and P. Chaunu 1955–1960; García Fuentes 1980; García Baquero 1976; Otte 1996; Pike 1962.

32 See Vila Villar, Acosta Rodríguez, and González Rodríguez 2004; Siegert 2003, 85–91.

33 Brendecke 2009a, 17–18. – Translation by the author. Originally: "Strukturell lässt sich aber sagen, dass in einem expansiven Reich, wie dem spanischen, die Kenntnis des Zentrums dem Wachstum des Reiches nicht hinterherkam. Im Gegenteil: Der Anteil, über den der Herrscher unmittelbar und persönlich Bescheid wusste, wurde immer kleiner, die Zahl der Vermittlungsprozesse immer größer. Es gewann also 'Medialität' an Bedeutung, d. h. Verfahren der Vermittlung durch dazwischentretende Personen (Amtsleute, Visitatoren, Vizekönige) und Medien (Gutachten, Zeugenaussagen, Landkarten). Sie versprachen auf je eigene Weise, das Ferne verfügbar zu machen, produzierten aber auch eine

The history of "political curiosity", says Brendecke,

> is full of promises to provide the sovereign with omnividence, a panoptic overview, and to place useful helpers, selfless advisors and perfect media at his disposal. That it always turns out differently though, that the ruler and his thirst for knowledge can never extricate themselves from the political fabric surrounding them, is worthy of great attention.[34]

Because the sovereign's contemporaries are aware of the opportunities to bring their own interests into play: "Already the many intermediaries, the agents of the sovereign's curiosity, ensure that the king is not fed with information alone but, essentially always, with interests as well."[35]

What is called "a bureaucratic history of knowledge" here, is a history of the media that resulted from the intersection of political curiosity and the interest of curious individuals. It is a history of displacements, a constant back and forth between administrative practices and bottom-up initiatives; it is a history of encountering the automatisms of filling out forms with both curiosity and new facts.

This essay refers to a constellation from the Spanish domains after the Bourbon Reforms of the eighteenth century[36] and to the administrative structures adopted and transformed after their independence in the early nineteenth century.[37] Whereas the new independent republics had to create a new administration apparatus, bureaucrats, bureaucratic writing and forms survive political changes.[38] Bureaucrats continued doing what they used to do, paper forms continue being used until they cease to exist. In doing so, agents and paper forward these forms they contain or they are used to correspond not only to the new political structures but also to new fields of expertise: former colonial functionaries or state employees were involved – by chance, duty, or private interest – in the collection of antiquities and fossils. Confronted with unknown realities – such as the ruins of an ancient city in Chiapas, or the skeleton of an unknown animal

eigentümliche ‚Mittelbarkeit', die den Herrscher von unmittelbarer Kenntnis seines Reiches abschnitt."

34 Brendecke 2009a, 18. – Translation by the author. Originally: "… ist voller Versprechungen, dem Herrscher Allsicht, einen panoptischen Überblick zu verschaffen und ihm nützliche Helfer, selbstlose Ratgeber und vollkommene Medien an die Seite zu stellen. Dass es dann dennoch immer anders kommt, dass sich der Herrscher und seine Wissbegier nie aus dem Gefüge des Politischen, das sie umgibt, herauslösen können, verdient hohe Aufmerksamkeit."

35 Brendecke 2009a, 19. – Translation by the author. Originally: "Schon die vielen Vermittler, die Agenten herrschaftlicher Neugier, sorgten dafür, dass der König niemals bloß mit Information, sondern im Grunde immer auch mit Interessen beschickt wurde."

36 See, for instance, Capel Sáez 1983.

37 The Bourbon Reforms attempted to change the complex administrative system introduced by the Habsburgs in Spanish America.

38 See Kafka 2012 on bureaucracy and writing; Socolow 1987 and Podgorny 2011b on the bureaucrats in the Rio de la Plata Provinces.

in the Pampas – they did what they were used to doing: they filled or generated descriptions that followed the protocols set in the realm of "political curiosity".[39] In doing so, they introduced forms from state administration to disciplines that were in the making. However, it is worth remarking that far from 'instructions' set by the State, learned societies, or professional bodies, what is at stake here is the problem of how to deal with the unknown and the contingent encounter of forms, media, particular individuals.

3 Military engineering

In the Spanish domains, the ruins of ancient cities were approached in two different ways: as a work of art, to be described by the antiquarians, and as an engineering problem. Engineers were an essential part of the Spanish bureaucratic system. They were also in charge of recording and describing the ruins according to the procedures set by the central administration in Madrid and in the viceroyalties. They used the same matrix and tool for this observation that they did to describe the environment and social life in the Americas:[40] A number of engineers, pilots and officials of the Royal Navy (*Real Armada*) destined for Naples, California, the Chiapas jungle or Asunción in Paraguay, even without 'instructions' knew how to organise the historical and contemporaneous narratives of the territory and its inhabitants according to a matrix incorporated into the work of the Royal Corps. The description of the topographic, physical and moral conditions included an overview of the history of the occupation of the territories of the Americas, the boundaries of the provinces, the layout and quality of the land, climate and winds, waters and rivers, minerals, plants, birds and land mammals, insects and reptiles, inhabitants and a statistical profile of the population. The practices of antiquarians, mathematicians, lawmakers and surveyors came together in those reports, which was useful both for governance and settlement strategies. The visit to the archives – available for consultation only by permission of the king – was combined with field measurements and coordination of local data. A political essay was a summation of practices for collecting and processing data, including details of plants and animals. Their job of analyzing materials from antiquity was no different from their tasks as reporters on contemporary life in the New World. Methods on how to dig, register, draw up plans, and take measurements were problems left to the engineers and surveyors and were not reflected in the antiquarians' publications.

Military engineers in Spain were employed as technicians for military and civil works, which required the skills of drawing façades and ground plans, of measuring elevations as well as knowing arithmetic and practical geometry. In the Academy of

39 *Sensu* Brendecke 2009a.

40 Podgorny 2007.

Barcelona, for example, engineers were trained in general arithmetic, practical and speculative geometry, calculus of the size of plane figures and bodies, theory of plane table and leveling, drawing, and plotting of plans and profiles.[41] In the Spanish Empire – both in Europe and America – the military engineers, architects and professionals were educated in mathematics and drawing in the military academies and were often called upon to observe and work in the technical description and recovery of ancient ruins. Because of their work in construction, they were also engaged in the discovery of buried antiquities.[42]

The work done in Pompeii, Herculaneum and Stabiae by the military engineers Pierre Bardet (1742–1744), Karl Weber (1750–1763), and Francisco de La Vega (1764–1804) reflects the development of excavation methods from a mere search for antiquities to a process that included the design of plans and interest in architecture.[43] As Parslow has shown, Weber proposed excavating Herculaneum following the lines of the streets and actively pursued investigations of the urban fabric as a whole. His interests extended to both public and private architecture and he showed a concern for the context of his discoveries. He was interested in where the objects were displayed and how they had been meant to be viewed in antiquity, how individual spaces worked, and what architectural clues could be read to determine how architectural units functioned.[44] However, as Mora underlined, one cannot describe the Bourbon excavations as the emergence of a new technique for the study of antiquity.[45] These excavations were not the method for a new archaeological science; they were the common techniques and practices of engineers, architects, topographers, and mining experts.

The military engineers' vision was also determined by 'architectural iconography' and by their training in the rules for ordering and grouping things on maps and in reports. Military and civil engineers, as it is well known, were central to the French expeditions to Egypt and Morea, and also to the new field of prehistory.[46] Although the large-scale excavations of the Vesuvian cities of Pompeii and Herculaneum did not forge a method to be applied to other cities, the survey techniques used there by engineers and surveyors created a parallel history to the philological tradition for the study of antiquity. Engineers, following their contemporary procedures and protocols of description, created a corpus of documents, which referred to the cultural history and life of ancient cities. Hidden by a tradition rooted in the work of Johann Joachim Winckelmann or in philology, the engineers have been as invisible as the remains that were discovered.

41 Capel Sáez 1983, 124.
42 Mora 1998, 90.
43 Parslow 1995; Podgorny 2007.
44 Parslow 1995, 4.
45 Mora 1998, 60.
46 Bourguet et al. 1998; Coye 1997.

4 Post-mortem medical examinations

Among the many agents involved or interested in excavations and the study of ancient human bodies, unrolling of mummies, and prehistoric remains were also physicians and surgeons. Histories of archaeologies and archaeological societies are full of titled medical doctors. However, not much attention has been paid to the practices in dissection and the protocols of post-mortem medical examinations and their connection with the history of archaeological observation. Post-mortem medical examinations and the relationship between doctors and surgeons and the bodies found in public places or the corpses of people who died suddenly, violently, or due to poison or errors in medication has a long history.

The legislation of the French Revolution and the Napoleonic initiatives systematized the knowledge that came from translations of different languages and medical traditions. In this framework, a series of works was published that systematized the procedures for opening cadavers during judicial medical examinations. Out of this grew a fundamental difference between the examination of the corpse's exterior and the general anatomical dissection. The initial examination was no longer limited to just a body lying on a table, but also included the location where the corpse had been found, its proximity to other places, the prints or marks found on the ground, the machines or instruments that could be found there, etc. Thus, the field examination of the corpse began to include the context around it. The record of this examination, from which new judicial evidence would be constructed, included the anatomical description of the body, the relationship between the body parts, measurements, height, birthmarks, size, age, sex, weight, clothing and any other information deemed to be useful. The special dissection of the body parts was preceded by a very detailed observation of the skin, the position of the feet and the state of the hands, with the aim of understanding the situation or attitude in which the subject had died. The general examination of the body was followed by one of the head and a detailed documentation of the ear canals, nasal cavity, neck, thorax, and abdomen.[47]

The principal idea was to omit nothing, avoiding any error that could condemn or free another individual and to get as close as possible to an all-encompassing observation.[48] Unlike the examinations of those who died of illness, where repetition of examination was possible because the causes repeated themselves, the observation of a person who died of violent causes created a unique situation where the circumstances of death were different with each victim and, in a poorly conducted autopsy, one risked removing the traces accidentally. The corpse of a person that had died by unnatural causes was transformed into irreplaceable evidence that would only reveal itself once to the

47 Chaussier 1816.

48 Chauvaud 2000; Menentau 2004.

observer as an act created by man and with special characteristics in every case. In this sense, judicial observation adopts a similar character to those observations done during a voyage of exploration, precisely because of the experience's unrepeatable nature.

At the same time, the judicial autopsy makes the crime a peculiar, profoundly historical event. The protocols try to document evidence and that, at the same time, will disappear in the very act of observation, which is an unavoidable step in finally authorizing the body's burial. By doing so, the evidence will be contained in the media in which it is documented.[49]

The systematization of the medical observation reports was framed, precisely in this dynamic, as the examinations destroyed the evidence through the visual inspection and the need to register information in order to present the complete evidence to the judge. In the early nineteenth century, a "rapport" was the document written by one or more doctors at the request of the appropriate authority about a particular fact. It aimed to document the evidence together with its context allowing the required conclusions to be drawn by the judiciary or the administration. Given that the life of the citizens depended upon it, the rapport required absolute clarity and discretion. Moreover, the author had to be understood by the magistrates who were unfamiliar with the technical terminology of medicine. When examining a corpse, it was recommended that special attention be paid to the clothing and the location of any objects around the body. If it was necessary to describe the trajectory of a wound caused by a pointed or sharp instrument, attention should be paid to the distribution and relationship between the elements that constituted the evidence.[50]

The protocols for post-mortem medical examinations created the matrix that was used to group details registered in a context of the deposition of corpses, again as part of the judicial evidence. In countries affected by the Napoleonic reforms, such as the nineteenth-century Spanish-American republics, surgeons and physicians were appointed as external experts for the police. These surgeons not only analyzed murdered people: they learned how to register facts that could be connected to the crime or enlighten observers on the circumstances in which it had occurred. As analyzed elsewhere, these judicial archives can help us understand the protocols for describing ruins and fossils that were emerging in the first part of the nineteenth century in the parallels between the practices and routines of medicine and the new prehistoric research.[51] As Jakob Tanner points out, bureaucratic routines and administrative measures had a performing power.[52] Surgeons and physicians used to fill out protocols and reports to describe corpses using the same standards automatically incorporating these

49 Podgorny 2003.
50 Moreau 1827, 455–456: "Rapport".
51 Podgorny 2011a.
52 Tanner 2008.

bureaucratic routines into other domains and including the description of remains from the distant past.

5 Fossils, garbage and mosaics

After their independence, the governments of the new republics from Spanish America recruited various individuals in Europe to compose new technical corps that, upon arrival, found a different situation from what was promised, a circumstance that was to be repeated indefinitely. In the1820s and 1830s another actor appeared on stage: the consuls of the countries that recognized the existence of the new republics. Great Britain, France, the United States of America and the Kingdom of Sardinia, Savoy and Piedmont sent or appointed their representatives to promote and protect the commercial interests of their countries. The consuls actively collected objects, maps and documents from these territories and rapidly constructed chains of information, linking educated people, in particular compatriot physicians and merchants, who could collect new data from different parts of the territory. All these actors exchanged data and objects in the form of commercial transactions, complimentary gifts or diplomatic gestures. The corpus of documents produced by the Spanish military engineers or the Jesuits' manuscripts, kept as confidential information of the colonial administration would lose this feature in the aftermath of the independence due to the instability of the new governments and the inability to control them. Paradoxically, they would be deemed new discoveries and used as evidence of Spain's veiled intentions for its colonies. The copies of maps and manuscripts were then transformed into a commodity, which, depending on their originality and rarity, could command a high monetary value in Europe. Under these circumstances, the manuscripts were introduced in scholarly circles, in private collections or on editors' tables. The publication, circulation and dissemination of these reports awakened an unusual 'fever' for collecting fossil skeletons, antiquities, and colonial documents that display how scientific and commercial value fed into one another.

For instance, in the Río de la Plata provinces news of these fossils emerged thanks to the chain of information that linked the field with the Buenos Aires landowners ('estancieros'): the dry season revealed a considerable number of skeletons, and the farm labourers reported the remains of dead animals, following instructions regarding hygiene in these rural areas. In 1819, Juan Manuel de Rosas, owner of one of the estancias where huge bones were being found, compiled a series of instructions for the administrators of his extensive estate in the pampas. These instructions defined a hierarchy of observers and emphasized the need for constant observation and the recording of even small events. Every man on the estancia who was able to read and write kept pen and paper at hand to register observations that would be forwarded to his superiors. Even in

the private domain, administration had adopted the forms of remote ruling. Years later, Rosas, as governor of the province between 1829 and 1832, would have an excellent relationship with the British consul, who benefited indirectly from the instructions Rosas gave to estancia administrators to keep an eye on the bones of dead livestock in order to maintain the ranches in clean and proper order.

> Garbage must be deposited in the place selected to dispose of it. In no way should there be scattered bones [...] Men should not live surrounded by rubbish. I insist: it is unacceptable for bones and little bones to be scattered everywhere, everything must go to the rubbish dump [...] Skeletons of every kind of animal, regardless of their quality, must be gathered in a place devoted to this end. Therefore, there must be no skeletons in the field, all must be collected and brought together for the branding of livestock.[53]

In one of the moves that characterized the configuration of knowledge, procedures relating to the hygiene of rural establishments were fortuitously incorporated into comparative anatomy. In this case, thanks to the diplomatic skills of the British consul in Buenos Aires, the giant bones were transferred from the garbage pit into the anatomists' hands.

With some of these bones, in the late 1830s and after long controversies, Richard Owen in London created the genus *Glyptodon* for an armoured fossil mammal from South America, roughly the same size as a small car.[54] For many years, reports on the fragments of what seemed to be the carapace of a gigantic armadillo had been sent to the collections in Buenos Aires, London, Montevideo, Paris, Rio de Janeiro, and Berlin. When *Glyptodon* was defined as a giant cataphracted mammal in late 1838, no single complete carapace of this animal had been seen in Europe: the shells had been well preserved as a whole while in the earth, but once they were exposed to the air they broke into pieces. Therefore, the new genus was created on the basis of a tooth and a sketch sent in a letter from Buenos Aires[55] and the carapace would only arrive in London several years later as a result of a commercial transaction between a local collector and the Royal College of Surgeons in London. The details of the first successful attempt to ship a shell to Europe display the intricacies of such enterprises and the combinations of skills and knowledge required to ship fossils abroad.

The local provider of bones was Pedro de Angelis (1784–1859), a Neapolitan antiquary, collector of colonial documents, dealer in bones and other vestiges from the South American past.[56] Aware of the interest that fossil bones had for European anatomists, de Angelis invested in fossil collections in order to resell them at good price. He employed local people to search for bones in different localities of Buenos Aires and

53 Rosas 1908, 28 and 31.
54 Rupke 1994.
55 Podgorny 2013.
56 See Sabor 1995.

bought books from London and Paris. He learnt how to classify what he was gathering in his collections. Furthermore, he developed a technique for preventing the cracking of the glyptodont's carapaces: the moment it was drawn out of the earth, he applied a coat of pitch, resin, and plaster from Paris to the inside of it to prevent its crumbling into pieces and then protected them with sheepskins and ponchos. As he explained, four specimens had to be sacrificed to transmit one and a half and he had to send a great quantity of so called tesserae taken from other individuals for completing a single shell. De Angelis remarked: "The restoration can be affected as is done in the case of separate ancient mosaics. The thickest disks belong to the upper part of the Shell where the rosettes are most marked. They gradually diminish at the edge of the Carapace"[57]. The principal parts were numbered and it sufficed to place the numbers next to each other to re-compose the armor.

The comparison with ancient mosaics was not just a metaphor:[58] it was a clear indication about how to proceed and also of the knowledge and skills employed to preserve the shell. Pedro de Angelis, a former preceptor of Joachim Murat's family in the court of Naples, was well acquainted with the works done in Pompeii, Stabiae and Herculaneum. Murat, as King of Naples, in 1808 had ordered that the floors of the Naples Royal Society be paved with some of the mosaics extracted from the ruins.[59] The transportation of the mosaics to the Accademia Ercolanese and the museums of Portici and Borbonico, had required not only a great deal of work but also to study the ancient techniques employed in mosaic pavements and the creation of devices to remove the mosaics from the ruins. Thus, antiquarians and engineers in charge of this transportation analyzed the mortar and the cement that were used to keep the tiles or square tesserae together by direct observation and by studying the ancient sources. Following Pliny's descriptions, the nineteenth century constructors made use of rubbish, charcoal, sand, and lime well mixed with small cinders. Observations of broken mosaic pavements showed that the natural soil had been filled up with materials such as plaster (in which the tesserae were set), stone pitching, ashes, and residues of burnt matter.[60] At the same time, the reconstruction of the mosaics was done based on the depictions and plans of military engineers in the eighteenth century and those that the Napoleonic commissioners could find in the archives of Naples.[61]

57 Pedro de Angelis to William Clift, Buenos Aires, August 12 1841, Translation of a letter respecting the *Glyptodon* and *Mylodon* by R. Owen, received November 1841, Natural History Museum Archives, London, LMSS C11 BRN 31229.

58 Whereas a glyptodont's carapace is composed by about 1000 osteoderms, the mosaics discovered in Pompeii in the 1830s had about 7000 pieces per palmo quadrato (around 100,000 per square meter). – Niccolini 1832; Burmeister 1870–1874. I am thankful to Juan Fernícola for his insight on glyptodonts' osteoderms.

59 Milanese 1998.

60 Clarke 1832, 10.

61 Pisapia 2002, 111.

As Maria Stella Pisapia noted, in the 1810s the use of ancient marbles and mosaics to pave modern floors followed not the desire of restoration but the contemporary taste, namely the adaptation of ancient objects to a practical end, i. e. they were recomposed according to the spaces to be paved by adding tesserae from other mosaics or sources of stone tiles.[62] In this very same sense, Pedro de Angelis was trying not to obtain an animal from the past but a "museum specimen," the object that the British museums were urging him to ship. For removing and transporting the carapace he resorted to the same procedures, materials, and techniques used to reconstruct mosaic pavements. In doing so, he made up a new object that brought together the tesserae of different specimens, the skills that artisans used for paving, and the expertise acquired in Naples to transport ancient mosaic patterns from the field to the museum. Furthermore, he translated the Plinian vocabulary that antiquarians used for the mosaic tiles to name the pieces that formed the carapace of the new animal: tesserae. When the bones arrived in London in late 1841, the reconstruction could only be done with the help of those instructions explaining which fragment went with what in order to reconstruct the whole pattern of the bone tesserae.

Many authors have noted the importance of the eighteenth century Bourbon excavations of Pompeii for understanding the kind of questions posed by Spanish and Spanish-American antiquarians.[63] What is less commonly known is the impact that Pompeii had on the creation of South American fossil mammals. Martin Rudwick, however, has noted, the impact of Pompeii on natural history and on Cuvier's research program.[64] Cuvier, in fact, wanted to render his reconstructions of extinct animals authoritative and "to 'revive' these strange animals in the mind's eye – just as the antiquarians tried to bring Pompeii back to life." Rudwick also compared the work of antiquarians with the methods of comparative anatomists by underlining Cuvier's appeal to naturalists to imitate antiquarians methods.[65] Pedro de Angelis had not only met Cuvier when he lived in Paris, he was also aware of Cuvier's research program and, before Cuvier died in 1832, de Angelis corresponded with him and offered to Paris the bones he collected in Buenos Aires. But in the case of de Angelis' transactions, it is clear that the impact of Pompeii on the practices of comparative anatomy followed more complicated pathways and do not directly reflect Cuvier's ideas. As mentioned before, the excavations and survey of Pompeii, rather than transforming the practices of antiquarians, created a constellation that associated the military engineers' bureaucratic procedures with the study of antiquities. In this frame, the archaeological object was connected to the bureaucratic system of colonial administration, shaped by instructions on what and how to observe. Plans, drawings, and measurements made and used by the engineers created

62 Pisapia 2002.
63 Alcina Franch 1995, Cañizares Esguerra 2002.
64 Rudwick 1997, 34.
65 Rudwick 1997, 35–41; Rudwick 2005, 370.

'portable antiquities' that shaped the coming into being of the archaeological object. In the case of de Angelis and his reconstructions of fossil skeletons, one could say that the animals 'emerged' from engineering recording practices and the artisanal expertise to reconstruct a mosaic pavement.

6 Concluding remarks

In the Río de la Plata Provinces, as throughout the Spanish Empire, the former officials had introduced a system of providing data according to the instructions handed down from the Iberian Peninsula. The bureaucratic practices of the artillerymen, the draughtsmen, scribes, clergymen or surgeons of unknown biography and the papers from transatlantic communications would be responsible for shaping the world governed from Seville, Cádiz and Madrid. Once the colonial tie was broken, a collector's sociability, driven now by private interests, continued working on the basis set by distant administration.

This paper argued that the protocols for observing and recording employed by different bureaucratic departments of state administration contributed to the creation of a matrix that would be fortuitously incorporated into the practices of several disciplines, including archaeology. Circulars or questionnaires, inventories and records, the French model for post-mortem medical examinations, and the protocols used by topographers, pilots, and military engineers would move from bureaucracy to scientific practice. Thus, objects were brought into collections having been formatted by procedures inherited from diverse traditions of state administration, construction, or commercial procedures.

Scientific practices are shaped by the articulation of different agents and cultural spheres. Practices, protocols and procedures used in one field drift into another with such an unperceivable pace that they normally go unnoticed in the routines of everyday scientific life. Public notaries witnessing facts and signatures, surgeons recording post-mortem examinations, military engineers drawing plans, surveyors measuring the landscape, officials answering questionnaires, clerical officers arranging inventories, priests compiling data from their parishes, and traders preparing their catalogues all contributed in some way with their expertise to shape the practices of modern archaeology. In that context, the archaeological object was connected to the bureaucratic system of administration, that moved to archaeology without intention of the actors. These drifts not only traversed disciplines, they also crossed time and space, traditions, and linguistic barriers. In that sense, working on the genesis of these practices requires openness to cross contemporary disciplinary borders and to rethink the geography of knowledge.

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Social Networks in the History of Archaeology. Placing Archaeology in its Context

Summary

This paper explores the value of social networks in the history of archaeology, combining them with biography and prosopography to produce a practical method for examining the development of the discipline, and an alternative to the traditional history of archaeology narrative. It presents broad categories for the interpretation and visualization of social networks, illuminated by case studies focusing on linked political and archaeological networks in early British Mandate Palestine and Transjordan. Social networks are a tool for understanding the historical context of archaeological work, and can be utilized to explore the role of men and women, politicians, soldiers, artists, architects, funders and others, in the excavation, interpretation, presentation and reception of archaeology.

Keywords: Archaeology; social networks; biography; prosopography; history; British Mandate Palestine and Transjordan.

Der Artikel untersucht die Bedeutung von sozialen Netzwerken in der Geschichte der Archäologie. Im Rückgriff auf biographische und prosopographische Ansätze soll eine geeignete Methode zur Untersuchung der Entwicklung des Fachs herausgearbeitet werden und eine Alternative zu den traditionellen Erzählungen in der Archäologiegeschichte. Anhand einer Fallstudie über die miteinander verbundenen politischen und archäologischen Netzwerke in den frühen britischen Mandaten Palästina und Transjordanien werden allgemeine Kategorien für die Interpretation und Visualisierung von sozialen Netzwerken diskutiert. Die Analyse sozialer Netzwerke gibt Einblick in den historischen Kontext archäologischer Arbeit und erlaubt es, die Rollen von Männern und Frauen, Politikern, Soldaten, Künstlern, Architekten und Sponsoren bei der Ausgrabung, Interpretation, Präsentation und Rezeption von Archäologie zu untersuchen.

Keywords: Archäologie; soziale Netzwerke; Biographie; Prosopographie; Geschichte; britische Mandatsgebiete Palästina und Transjordanien.

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1 Introduction

In March 1929, Agnes Conway arrived in Jerusalem. She planned to survey Petra, a monumental Nabataean site in the British Mandate Territory of Transjordan. Agnes Conway was a trained historian and museum curator; an alumna (sans degree) of Newnham College Cambridge. She had been associated with the British School at Rome, and a student at the British School at Athens.[1] Struck with Petra after a 1927 trip there she was given the opportunity to investigate the site for herself, using her father's connections with the wealthy Mond family to secure funding for a two month excavation at Petra. The Chief Inspector of Antiquities in Transjordan, George Horsfield, had granted her permission to work at the site; the funds she raised enabled them to conduct the first 'scientific' excavations at Petra.[2]

Agnes Conway and George Horsfield's 1929 excavation at Petra incorporated a number of interconnected networks centering in London and Jerusalem. Piecing together these networks reveals the historical context of archaeology in British Mandate Palestine and Transjordan. They highlight archaeology's position within the political administrations in London and Jerusalem and, through the role of the British School of Archaeology in Jerusalem (BSAJ), they played a pivotal role in forming a social and intellectual hub for Palestine and Transjordan that was considered politically valuable by Mandate, Foreign Office and Colonial Office officials. Hitherto neglected contributors to archaeological research are exposed through visualizations of the networks using three broad relationship categories, personal, transactional and organizational, linking people to each other and to related organizations or institutions.

As Shapin and Thackray noted, drawing a definitive framework around the idea of a 'scientist' often eliminates those people who shaped a discipline without directly engaging in it as a full-time occupation.[3] Macleod's examination of political-scholarly network in relation to government grants to the Royal Society is a particularly useful example of the benefits of extending prosopographical studies outside disciplinary boundaries.[4]

1 See Evans 1966; Thornton 2011b.

2 See Conway, A. 30 August, 9 September 1928. Diary Entries. Cambridge University Library: MSS Add 7676/Z30. George Horsfield and Agnes Conway were married in Jerusalem on 28 January 1932; Thornton 2011a.

3 Shapin and Thackray 1974, 3–4.

4 MacLeod 1971.

Lines must be drawn, for practicality of scope if nothing else, but they need not be disciplinary. In order to examine a social network in any comprehensive sense sponsors, patrons, friends, spouses, teachers, families, clubmates should all be considered; this information builds up a more complex picture and contributes to reconstructing and interpreting the historical context. In this way, the history of archaeology moves beyond the still popular narrative of great excavators, sites and objects, towards a more nuanced understanding of archaeology within social, cultural, political and economic arenas. It presents a broader view of contributors to the archaeological field, incorporating individuals such as politicians, funders and administrators amongst a host of archaeologists, artists, architects, assistants, volunteers and labourers present on site, as recent scholarship on the history of Egyptology and the history of the British School at Athens have shown.[5]

While many prosopographical and biographical studies in the history of archaeology focus on intellectual and disciplinary history and the reception and impact of research, the following examination derives and evolves from the author's doctoral thesis, which uses these three relationship types to examine the role and value of social networks in analyzing the social history and professionalization of British archaeology in the Eastern Mediterranean and Middle East, and its links to institutions and movements both within and outside the scholarly community, between 1870 and 1939.[6] This article first presents a brief overview of the value of a combined social network, prosopography and biography approach to evaluating archaeology's impact beyond the discipline.[7] It then discusses the broad framework for archaeology in Mandate Palestine and Transjordan, highlighting the political, intellectual and social organizations involved.[8] It next applies the method to analyze the contexts of the BSAJ government grant and the 1929 Petra excavations as case studies, examining the function of small-scale social networks in more detail. It concludes by reflecting on the value of studying these networks for interpreting and analyzing the impact of archaeology in non-academic settings.

2 Combining social networks, prosopography and biography

2.1 Social networks for historical analysis

Networks have become an increasingly popular medium of exploration. In the past two decades network analysis and prosopography, the study of a group of people linked by

5 E. g. Bierbrier and Naunton 2012; Quirke 2010; Gill 2011.

6 This examination derives and evolves from the author's doctoral thesis of 2012, Thornton 2011a.

7 This is based on the methodology presented in Thornton 2011a, ch. 1.

8 See also Gibson 1999; Thornton 2011a; Thornton 2012a.

common interests, have been used to reconstruct historical intellectual, political and business networks in a variety of contexts.[9] A wide range of scholars draw on published sources for prosopographical data and use quantitative methods to explore historical periods or themes.[10] Many of these quantitative methods incorporate complex mathematical models to produce almost impenetrable visualizations of a particular network over a period of time.

As Verbruggen outlines in his overview of the subject, using social networks for the purposes of historical reconstruction continues to be problematic for historians given the fragmentary nature of historical archival material.[11] Considering the problems inherent in using published prosopographical lists with organized, assembled, predetermined categories,[12] as Schlanger discusses,[13] assessing archival documentary material, though not without its problems such as organization, manipulation or removal by owners, executors or archivists, can allow researchers to deepen their understanding of historical context and complicate, change or enhance the narratives presented in published sources.

Social network analysis and actor-network theory, as outlined notably by Bruno Latour,[14] provide useful *practical* tools for exploring and understanding a network in a given historical context. These methods also, crucially, enable a researcher to look beyond the boundaries of a field, subject or geographical area.[15] Emirbayer and Goodwin outline a number of key terms in social network analysis that influenced the research presented here.[16] They define network analysis as a means of "investigating social structure", and the social network as sets of relations in that structure linking "actors" (e. g. groups, organizations, individuals) together.

Their work stresses the importance of understanding the ties between people and organizations, and the need to include a broad definition of "actors" in any analysis of social networks.[17] Stevenson and Greenberg provide further insight into investigating social networks.[18] Their research presents a valuable case for a nuanced understanding of strength and weakness in ties, as outlined initially by Mark Granovetter.[19] Additionally, they highlight the complexities of personal agency within what they term a "political opportunity structure", in which the actor on the "periphery" of a network is able to use his or her position on the edge of power to act quasi-independently of the "central" network; conversely an actor in the "center" of the network might have less ability to act due to ties within the established framework.[20] The center versus periphery theme

9 E. g. Brayshay, Cleary, and Selwood 2005; Keats-Rohan 2007.
10 Bearman 1993; Rosenthal et al. 1985; Verbruggen 2007.
11 Verbruggen 2007, 579–581.
12 See Kay 2007.
13 Schlanger 2002, 130.
14 Latour 2005.
15 Thornton 2011a.
16 Emirbayer and Goodwin 1994, 1414 and 1417.
17 Emirbayer and Goodwin 1994, 1417.
18 Stevenson and Greenberg 2000.
19 Granovetter 1973.
20 Stevenson and Greenberg 2000, 651–657

is particularly important for the history of archaeology, which during this period (like today) was considered to be outside the political sphere despite its place within the administrative framework.[21]

2.2 Prosopography, biography and the history of archaeology

The history of archaeology has only recently begun to have a notable historiography of its own, in which archives are used alongside published sources as essential tools for exploring disciplinary development.[22] Biography is an unceasingly popular method for exploring archaeology's past,[23] as it has been in the history of science.[24] As Kaeser notes, alongside biography prosopography is also valuable.[25] Both prosopography and collective biography[26] have been used in various ways for charting the history of archaeology, and in particular have enabled more detailed explorations of the history of women in archaeology[27] and the history of specific sub-disciplines such as Egyptology[28] and Romano-British archaeology[29], or institutions such as the British School at Athens.[30] However, prosopographical compilations tend to focus on the archaeologists themselves; few branch out of the 'scientific community', as Shapin and Thackray urge so powerfully in their 1974 investigation into nineteenth century science.[31]

Identifying relationships can circumvent artificial borders such as geographical, disciplinary and gender-based themes in the history of archaeology.[32] Padgett and Ansell's exploration into the role of the Medici family in the fifteenth century identifies nine different kinds of connection, including economic, political and personal links.[33] They also acknowledge that strength and weakness within these ties is determined through inductive reasoning, highlighting the importance of the researcher's interpretation in social network analysis.[34] For the research presented here, three broad categories of relationship were identified and defined (see Tab. 1).

These categories, represented by three different colors, enable illustrative visualizations[36] of social networks to be created, enhancing the prosopographical and biographi-

21 Thornton 2011a.
22 E. g. Schlanger and Nordbladh 2008; P. J. Smith 2009; Gill 2011.
23 E. g. Kaeser 2008; Givens 2008 [1992]; Murray 1999b.
24 See Söderqvist 2013.
25 Kaeser 2008, 13.
26 E. g. Murray 1999a.
27 E. g.Cohen and Sharp-Joukowsky 2004; Díaz-Andreu and Sørensen 1998.
28 Bierbrier and Naunton 2012.
29 Wallace 2002.
30 Gill 2008, Gill 2011.
31 Thornton 2011a. – *Who Was Who in Egyptology* (Bierbrier and Naunton 2012) includes funders, politicians and military officers particularly interested in Egyptology. See also Fabian Link's article on the German Castle Society network, which explores the political and economic contributions to this circle (Link 2009).
32 Thornton 2011a.
33 Padgett and Ansell 1993, 1265–1266.
34 Padgett and Ansell 1993, 1274–1275, n. 28.
35 Padgett and Ansell 1993, 1266 n. 16.
36 The visualizations here (Figs. 1–4) have been created by the author; distances between 'actors' are used for clarity only.

Organisational	Formal or informal membership in an organisation. A relationship of participation, rather than merely a paid service, it differs from a transactional relationship (defined below). Examples include: serving on a board of trustees, organising committee or council; election to a learned society or club; being an employee within an organisation.
Transactional	The exchange or transfer of resources, knowledge and/or connections. Examples include: sponsorship/funding, employment/training, logistical/practical assistance.
Personal	Friendship or familial relationship. As Padgett and Ansell have explained, this type of relationship can be difficult to define.[35] They chose to assess such relationships conservatively; the same approach has been adopted here.

Tab. 1 Categories of relationships, adapted from Thornton 2011a.

cal elements of the social history of archaeology. While they could be further subdivided as Padgett and Ansell's work demonstrates, the simplicity of the three broad categories helps to create meaningful visualizations, which in turn may lead to more detailed exploration and analysis within archival or published sources. Additionally, categories may be juxtaposed if necessary – for example, blue transactional relationship lines and green organizational relationship lines could highlight two different facets of an individual's connection to an organization. Being paid for a job is a transactional relationship, while contributing to the management or administration of an organization or group is an organisational relationship.[37]

The contexts of the BSAJ grant and Conway and Horsfield's 1929 Petra excavation provide examples of how a combined prosopographical, biographical and social network method can be used to investigate the history of archaeology in British Mandate Palestine and Transjordan. This method exposes a number of key players; some fit within the stringent definition of a 'scientific' discipline, others do not. Using the three relationship categories identified above, the following sections will reconstruct this historical network, simultaneously revealing the political, social and economic context of archaeological work in British Mandate Palestine and Transjordan.

37 Thornton 2011a.

3 London, Palestine and Transjordan: Archaeological and political networks

Examining the organizational networks involved in the management of archaeology in Mandate Palestine and Transjordan reveals how archaeology fitted within local and imperial contexts. By assessing these groups in London and Jerusalem it is possible to see the impact of archaeology across national borders, and how decisions made by a small network of men and government departments in London affected a larger group of archaeologists, officials, expats and local communities in the Mandates. For the most part analysis of archaeology and its impact continues to remain outside of or marginal to the interests of historians of Mandate Palestine and Transjordan.[38] However, Shimon Gibson's 1999 article on British archaeological institutions in Mandate Palestine and Nadia Abu el-Haj's exploration of archaeology's role in the development of the state of Israel both specifically address this gap.[39] A special issue of Public Archaeology also examines the interplay between archaeology and heritage tourism in Mandate Palestine and Transjordan from a number of angles.[40]

The early post-war period (1919–1920) is a critical one in the history of archaeology in the Holy Land. Systems were set in place for managing archaeological exploration, excavation, research and conservation that governed the way archaeological activity was conducted in the following decades. As the First World War drew to a close archaeological groups in London and Jerusalem began to organize the management of antiquities and conservation of archaeological sites, developing the examples of antiquities services in Cairo and Constantinople to meet this new administration.[41] There were several groups involved in constructing a management system for archaeological activity in post-war Palestine. The London-based Archaeological Joint Committee (AJC) and the Jerusalem-based International Archaeological Advisory Board (IAAB) provided advice, while the Department of Antiquities of Palestine (DAP), the British School of Archaeology in Jerusalem (BSAJ) and the Pro-Jerusalem Society (PJS) actively promoted archaeological exploration in various forms and formed significant social, intellectual and political spaces for the promotion and discussion of archaeological work (see Fig. 1).[42]

By the end of 1918, the Foreign Office had solicited the British Academy to establish the Archaeological Joint Committee (AJC).[43] The AJC brought together representatives of London-based museums, learned societies and British schools of archaeology with

38 E. g. Abū Nūwār 2006; Adelson 1994; El-Eini 2006; Fromkin 1989; Kedourie 2004; Salib 1993; Wasserstein 1978; Wilson 1987)

39 Gibson 1999; Abu El-Haj 2001.

40 Thornton 2012b.

41 Gibson 1999; Thornton 2011a; Thornton 2012a.

42 Gibson 1999; Thornton 2011a.

43 Kenyon 1920, 5; Gibson 1999, 128.

interests in the region to advise on archaeological policies.[44] The Director of the British Museum and President of the British Academy, Frederic Gerard Kenyon, was the AJC Chairman.[45] With the AJC's formation a government-sanctioned advisory board was born lobbying for archaeology and providing scientific expertise for a government embarking on a new era of imperial expansion through the Mandate system.

The AJC's remit was publicly presented in the British Museum's 1920 handbook, *How to Observe in Archaeology,* which drew together expertise on archaeological methods, interpretations and antiquities legislation for the ordinary educated traveller embarking on a journey to "the Near and Middle East" – defined as Greece, Asia Minor, Cyprus, Syria, Egypt, Palestine, and Mesopotamia. By 1929, the AJC was highlighting its place as the middleman between the academy and government and at the forefront of the archaeological sector. In a second edition of *How to Observe* their approved "Proposals for the Administration of Antiquities in Mandated and Similar Countries", dated November 1921, laid out principles for (Western) archaeological exploration and research. These included the need to encourage local contributions to and support for archaeological research, the creation of museums in country, controlling (though not ending) the division of antiquities between excavators and government, and managing the provision of excavation permissions to restrict unskilled or untrained persons from access to sites.[46]

Inside Whitehall,[47] in 1919 George Nathaniel Curzon became Secretary of State for Foreign Affairs, heading the Foreign Office. Curzon was deeply interested in expanding British educational activity overseas and in encouraging and harnessing British expatriate communities' ties to Britain.[48] Supporting British archaeological research in Palestine fitted into these plans; by 1920 Curzon was also lobbying for support for Britain to manage protection of Palestine's standing monuments.[49] Unlike Britain's involvement in India, Egypt and Sudan, Palestine presented a different kind of administration, dictated by the League of Nations Mandate Agreement. Before and during the War, the Foreign Office had managed British interests in "foreign countries" such as Egypt, Greece, Italy and the Ottoman Empire[50], and the India Office managed the British Raj in India, as well as Mesopotamia (Iraq) after the war until the British Mandate was firmly established there. With the Mandate for Palestine and Mesopotamia in hand, from 1922 an

44 See Kenyon 1920, 5. *How to Observe in Archaeology* (1920, 1929) contains a complete list of organizations represented on the AJC. The AJC is also referred to in documents as the Joint Archaeological Committee.

45 Thornton 2011a.

46 Hill 1929, 112–114.

47 The term "Whitehall", deriving from the street in London where many of the chief departments of the British government are located, is used to refer to the British government in general (see Burns 1921, 7).

48 See Fisher 2009, 2 and 24.

49 BSAJ. 3 Jan 1920. Minutes of Organising Committee. Minute Book 1: 1918–1960. Palestine Exploration Fund Archives.

50 Before the Great War the Ottoman Empire included "Turkey in Asia" incorporating what became Mandate Palestine, Transjordan and Iraq.

important change occurred: the Colonial Office, and specifically its Middle East Department (initially under Winston Churchill as Secretary of State for the Colonies), began overseeing the affairs of the British Mandates in Palestine, Transjordan and Iraq.[51] In Palestine (and later Transjordan), a Crown-appointed British High Commissioner was at the head of the administrative structure.[52]

All of these factors had an impact on archaeology. In practice, the AJC in London advised members of the Middle East Department on matters of the administration of archaeology and a representative of the Middle East Department, Gerald Clauson, sat on the Committee.[53] The High Commissioner of Palestine communicated through the Middle East Department on matters concerning archaeology (Fig. 1). Although the Palestine administration operated in a semi-autonomous fashion, the Colonial Office had an overarching view.[54] From their Downing Street office CO officials supported issues of importance to British interests, as will be seen from the evaluation of the role of the BSAJ in Palestine.

By 1920 in Jerusalem John Garstang was joint Director Department of Antiquities of Palestine and the British School of Archaeology in Jerusalem. The Palestine Exploration Fund and the British Academy had set up the BSAJ in 1918 and recruited Garstang. Frederic Kenyon was BSAJ President, and its Vice-Presidents were Edmund Allenby, a Field Marshal who had led British forces to occupy Jerusalem in 1917, and Palestine's High Commissioner Herbert Samuel. These organizational relationships illustrate the links between the emerging British administration in Palestine and archaeology.[55] Garstang was subsequently appointed Director of the Department of Antiquities in Palestine, responsible for managing archaeological sites throughout Palestine, which until 1923 included the land that became Transjordan.[56] As BSAJ Director he trained prospective archaeologists and carried out research agendas.[57] Garstang balanced the interests of the various local and foreign schools and societies of archaeology in the region, which had representatives on the International Archaeological Advisory Board (IAAB). IAAB members were chosen by the High Commissioner, with Garstang as Director of Antiquities as Chairman.[58] Although classified as "non-political", like the AJC in London

51 Kirk-Greene 1999; Goode 2007, 188; Mercer, Collins, and Harding 1921; Steiner 1969, 214; Thornton 2011a; Thornton 2012a.

52 Luke and Keith-Roach 1930, 207.

53 Thornton 2011a; Hill 1929, 12.

54 Thornton 2011a.

55 See BSAJ 1920; Gibson 1999; Thornton 2009a; Thornton 2011a.

56 Article 21 of the Mandate agreement outlined the management of archaeology in Palestine. The Mandate Agreement is printed in full as Appendix I in *The Handbook for Palestine and Transjordan* (1930).

57 The duties of the BSAJ director are clearly delineated in Myres, J. L. 7 November 1926. "Memorandum on the Status and Functions of the British School of Archaeology in Jerusalem". T161/1256. The material hereafter referenced T161/1256 is the "British School of Archaeology in Jerusalem – Grant" folder, containing Foreign and Colonial Office correspondence with the Treasury and minutes between Treasury officials concerning the BSAJ grant. T161/1256 is held at the National Archives in Kew.

58 Luke and Keith-Roach 1930, 87; Thornton 2011a.

advised civil servants in Whitehall the IAAB advised the Palestine Department of Antiquities on technical and general matters concerning archaeology. IAAB members were also consulted on granting permissions to excavate.[59] A representative of the Palestine Government's Public Works Department, Austen St Barbe Harrison, Chief Government Architect, also sat on this board.[60]

Garstang was also part of the Pro-Jerusalem Society (PJS), an organization created and sustained by Ronald Storrs, Governor of Jerusalem from 1917 to 1926 to protect and restore the historic center of Jerusalem.[61] In the PJS representatives from the numerous archaeological, religious and political communities in Jerusalem were drawn together. Although financed privately, the network of administrative officials involved in PJS activities informally cemented it within the governmental structure until its closure in 1926. It represented the thrust of the early Mandate administration's enthusiasm for the new post-war era in Palestine. The Department of Antiquities financially supported some PJS projects, and John Garstang was both a Council Member and (from his BSAJ address) a Subscriber.[62] Storrs, Garstang and representatives from the French and American Schools also joined other local scholars in the Palestine Oriental Society, which met four times a year for scholarly lectures on subjects concerning archaeology and local culture.[63]

In the early 1920s, the land east of the Jordan River, originally part of the Palestine Mandate, became the Kingdom of Transjordan. It was excluded from the terms of the Balfour Declaration relating to Jewish settlement.[64] Abdullah, son of Sherif Hussein who had been involved in the Arab Revolt, became King (Emir) of Transjordan in 1921. This new country had a separate Mandate from 1923 with a British Resident and Assistant Resident to be Britain's representatives. The High Commissioner for Palestine took on an additional role as High Commissioner for Transjordan, to retain general oversight of the Mandate.[65]

According to the terms of Article 3 of the Transjordan Mandate, all government departments were to have Transjordanian directors.[66] British officials were occasionally put in place to advise (and in the case of antiquities effectively to manage) departments

59 Anonymous 1920, 1922a; Thornton 2011a. – Anonymous 1922a. Rough Statement of progress of scheme for Excavating Mount Ophel. Israel Antiquities Authority Archive. IAA ATQ 1789 Box 4.

60 Thornton 2011a. – Harrison, A. 18 July 1927. Candidate's Separate Statement. Israel Antiquities Authority Archives: Harrison Legacy Box.

61 See Wharton 2008.

62 Anonymous 1919; Anonymous 1926; Ashbee 1921, xv and 97; Garstang 1922; Storrs 1949, 311; Thornton 2011a; Thornton 2012a.

63 POS 1920. – Papers from these lectures were later published in the *Journal of the Palestine Exploration Society*.

64 Salib 1993, 88.

65 Luke and Keith-Roach 1930, 421–422; Salib 1993, 83–88; Thornton 2011a; Thornton 2012a. – See Wilson 1987, Salib 1993 and Abū Nūwār 2006 for further details of Transjordan during the Mandate Era.

66 The Mandate Agreement for Transjordan is reprinted as Appendix II in *The Handbook for Palestine and Transjordan* (1930); Luke and Keith-Roach 1930, 463; Thornton 2011a; Thornton 2012a.

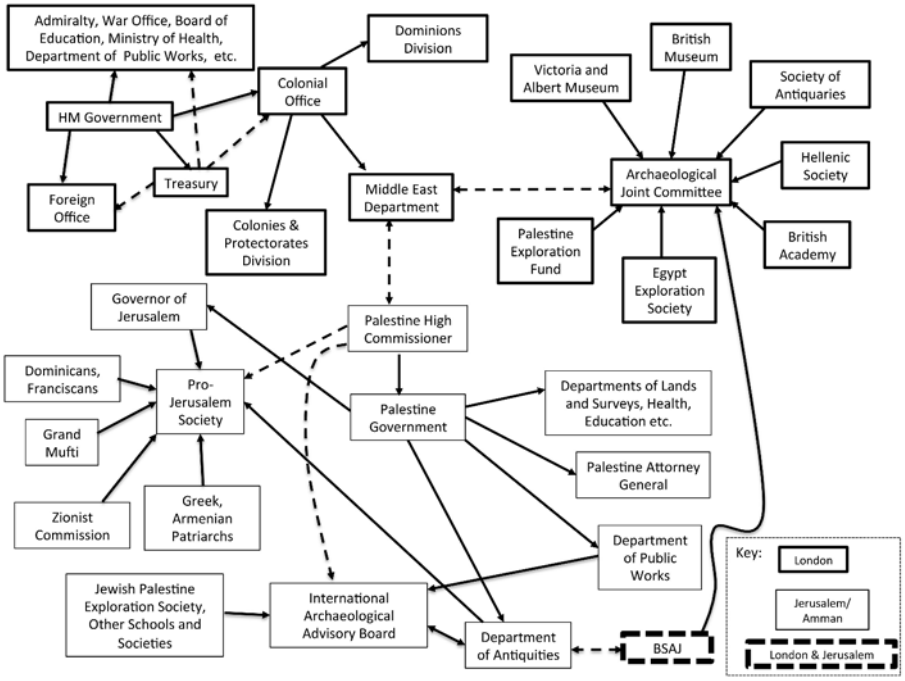

Fig. 1 The framework for archaeological administration in Mandate Palestine and Transjordan c. 1927. – Only a selection of members/organizations/departments are shown here. For a full list of the members of the Pro-Jerusalem Society, the Archaeological Joint Committee and lists of officials in the Middle East Department and the Palestine Administration departments see Ashbee 1921; Hill 1920; Hill 1929; Mercer, Gent, and Harding 1927.

(see Fig. 2). The new Transjordan Mandate included separate provision for antiquities.[67] From 1923, the Department of Antiquities was created under the nominal leadership of Riza Tewfik Bey, secretary to the Transjordanian Prime Minister.[68] Garstang at the Palestine Department of Antiquities initially took practical responsibility for Transjordan's archaeology by sending former BSAJ student George Horsfield to begin protecting Transjordan's standing monuments and antiquities through a small-scale program of preservation and restoration, instigated at Abdullah's request. Eventually Horsfield was assisted by an Englishman, Reginald G. Head, and Ali, who came from Jerash, a village and archaeological site north of Transjordan's capital Amman. Ali became a personal friend of Horsfield's.[69]

As in Palestine, the context of archaeology during this period in Transjordan was inherently a matter of politics. Examining archaeology's place within its political and economic context is critical to understanding its relationship within the administrative

67 Thornton 2011a; Thornton 2012a, 201.

68 Albright 1924, 3.

69 BSAJ 1924a, 77; Thornton 2009b; Thornton 2011a; Thornton 2012a, 201.

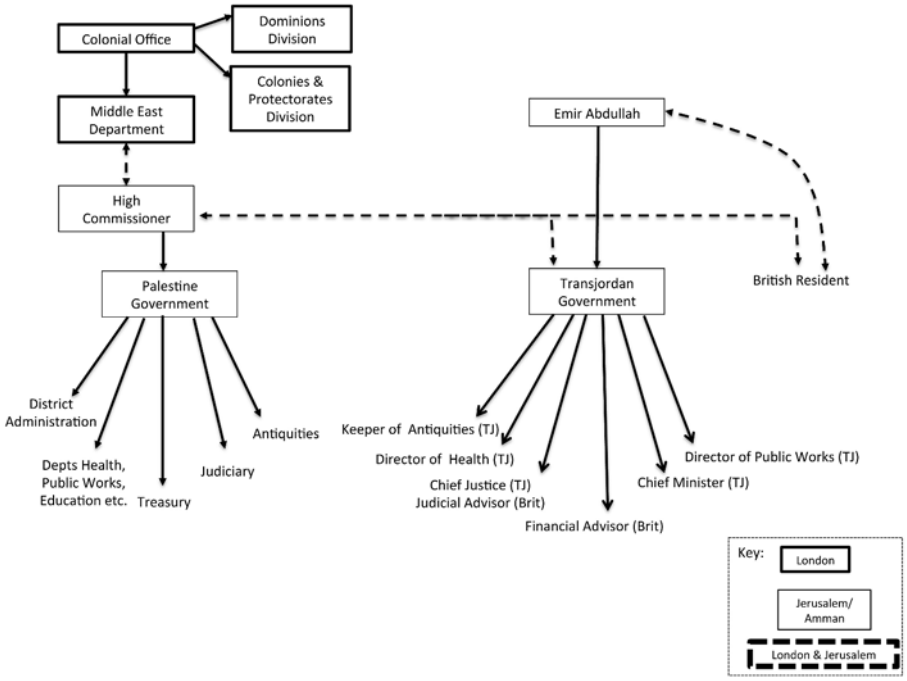

Fig. 2 Government administration during the Mandate in Palestine and Transjordan 1927 (e. g. Mercer, Gent, and Harding 1927, 505).

framework. As discussed in detail below, it is clear that although archaeology was peripheral within British governmental structure, being a small department in small, newly formed administrations within the larger remit of the Foreign and Colonial Offices, it was considered and consciously framed by these Whitehall departments as important to British prestige.

4 Center and periphery: The British School of Archaeology in Jerusalem

4.1 The BSAJ in London

> It is possible that the traveller will begin his journey at a point other than the capital. Inquiries should be made at the London head-quarters of the Schools concerning residents at such places who may be able to give advice to intending travellers.
> – *How to Observe in Archaeology*[70]

The British Schools of Archaeology were important centers for information exchange. They were created as hubs for scholars and travellers as well as a training facilities for giving practical field experience and research opportunities. By the Mandate period the British Schools at Athens (founded in 1886) and Rome (founded in 1901) were well established, and seen as such by the officials at H. M. Treasury, who had sanctioned annual £500 grants to both Schools since 1895 and 1905 respectively.[71] The Treasury file relating to the BSAJ's government grant reveals a debate within the British government in London that highlights the BSAJ's position as both a center and a periphery in the Mandate context.

Fisher's work on the Foreign Office's short-lived Committee on British Communities Abroad shows that in the wake of victory in the First World War the British government was attempting to revitalize British strength and international impact.[72] For the Foreign and Colonial Offices in particular the British Schools of Archaeology fed into this plan. The British School at Athens (BSA) was seen within Whitehall more broadly as a visible symbol of the potential for increasing what we would now call British 'soft power' in foreign countries, and encouraging nationalist feeling and imperial support among British expat communities, given the "public service" (possibly a euphamism for intelligence work) that certain BSA students and staff had undertaken during the First World War.[73] Consequently it was considered to be worthy of continued Government financial support.[74] The potential for enhanced diplomatic cultural relations through the British Schools' staff and students was particularly meaningful in light of

70 Hill 1920, 9.

71 Thornton 2011a. – On terms of grants to the British Schools at Athens and Rome, see Sperling, R., 19 Feb 1920, Letter to Secretary to the Treasury, T161/1256.

72 Fisher 2009.

73 Gill 2011: (Ch 13) discusses the wartime work of BSA staff and students.

74 E. g. [*Illegible*], 27 February 1920, Treasury minute, T161/1256; Myres, J. L. 7 November 1926, "Memorandum on the Status and Functions of the British School of Archaeology in Jerusalem", T161/1256. – For the concept of soft power see Nye 2008.

the Committee's research.[75] Their 1920 report stated that the British schools of archaeology contributed to British scholarly prestige, and that the British students who attended them had the opportunity to make their mark on both local and international levels. It concluded the section on archaeology by emphasizing that these contributions were valuable enough to merit Governmental financial support.[76] These qualities were consistently championed by the BSAJ's Council members and officials at the Foreign Office and the Colonial Office in London, and submitted as evidence to Treasury officials for the need for government support for the newly established BSAJ.[77]

As Macleod's study of the history of the Royal Society's Government grant shows, personalities and relationships within and between learned societies and the Treasury are key to understanding the management and financing of science at a governmental level.[78] In the case of archaeology, Treasury officials were willing to maintain the levels of support given to the British Schools at Athens and Rome which had by that time been established long enough to prove their value – especially considering their wartime contribution. However, they felt the viability of the newly formed BSAJ was unclear, and that its emphasis on Biblical archaeology was less valuable in terms of "importance" and subscriptions than the classical archaeology dominating BSA/BSR research projects.[79] In addition, as the BSAJ was situated in a newly defined *mandated* territory (under British administration but with duties to report to the international League of Nations), it was neither a foreign country nor a colony in the traditional sense.[80] While both the Foreign and Colonial Offices acknowledged the BSAJ's potential value for scientific prestige and diplomatic relations, and supported applications by BSAJ Organising Committee/Council members for a Government grant on the same terms as the British Schools at Athens and Rome, successive Foreign Office and Colonial Office officials (and through them BSAJ Council members Kenyon, D. G. Hogarth, and J. L. Myres) had to push to convince the decision makers at the Treasury of the new School's value and potential.[81]

75 See Fisher 2009. – Fisher suggests that the Committee on British Communities abroad could be considered the precursor to the British Council (Fisher 2009, 38–39).

76 HoC 1920, 10.

77 Myres, J. L. 7 November 1926. "Memorandum on the Status and Functions of the British School of Archaeology in Jerusalem", T161/1256; Amery, L. 21 April 1926, Letter to Treasury, T161/1256; Davies, H. E. 21 March 1922, Treasury minute; Shuckburgh, J. 19 November 1926, Letter to Secretary to Treasury, T161/1256.

78 MacLeod 1971.

79 E. g. Headlam, M. F. 20 January, 9 June 1926, Treasury minutes, T161/1256; W., A. 18 Feburary 1927. Letter to L. Amery [Draft]. T161/1256; W., A. 10 February 1927, Treasury minute "Government grant to BSAJ", T161/1256.

80 E. g. Graham, A. 5 March 1922, Treasury minute, T161/1256.

81 Oliphant, L. 13 March 1922, Letter to Treasury, T161/1256; Meiklejohn, R. S. 23 March 1922, Treasury minute, T161/1256; Grindle, G. 21 April 1926, Letter to Treasury, T161/1256; Myres, J. L. 7 November 1926, "Memorandum on the Status and Functions of the British School of Archaeology in Jerusalem" T161/1256; Shuckburgh, J. 21 February 1922, Letter to Treasury, T161/1256; Hogarth, D. G. n. d. Letter to W. Churchill, T161/1256; Hogarth, D. G. & Kenyon, F. G. 1 February 1922, Letter to W. Churchill, T161/1256.

In part, the framework for archaeology in Palestine encapsulated a certain ambiguity in the early Mandate period – John Garstang sat at the head of the Department of Antiquities of Palestine and the BSAJ. From the center of operations in Whitehall, Treasury officials felt that the Mandate government should take responsibility for financial support of the BSAJ in addition to the Department of Antiquities of Palestine.[82] However, under pressure from the Foreign Office, and, as Deputy Comptroller of Supply Services R. S. Meiklejohn put it, with the weight of BSAJ's "distinguished patronage" and "eminent" Council members, the Treasury conceded that as a *British* school training British students (rather than a Mandate school) the BSAJ merited a government grant. Finally, £200 was awarded to the School from the Treasury's Special Service Fund in 1922.[83]

The following year the Treasury sanctioned financial support through the Civil Service Estimates Class IV (Science, Education and Art) Scientific Investigations vote for a £500 per annum grant for three years. However, unlike the terms given to the British Schools at Athens and Rome, the funding was given on the condition that the BSAJ provide match funding through public subscription.[84] While much of the BSAJ's match funds were gathered in Britain through its office at the Palestine Exploration Fund, the financial accounts that the BSAJ produced to send to the Treasury also reveal the local network of BSAJ subscribers in Jerusalem. The nature of the funding agreement made it necessary for the School to cultivate 'local' support in both the UK and Palestine. Among its Jerusalem-based subscribers were members of the Palestine government – the High Commissioner Herbert Samuel (donating £E5), Ronald Storrs (subscribing £2.2.0), Norman Bentwich of the Judicial Department (subscribing £2.2.0), Herbert E. Bowman of the Education Department (subscribing £E2.5.0) and A. M. Hyamson of the Immigration Department (subscribing £E2) – as well as Dr John Strathearn from Jerusalem's Opthamalic Hospital (£E10.24.0) and Annie Elizabeth Landau, principal of the Evelina de Rothschild School for Girls (subscribing £2).[85]

The role of the Foreign and Colonial Offices cannot be underestimated when considering the history of the BSAJ. Foreign Office and Colonial Office officials were regarded as highly important figures to the BSAJ's Council members, and to all the British Schools abroad. BSAJ Council President (and former British School at Athens student) John Linton Myres' 1926 "Memorandum on the Status and Functions of the British

82 The complexities of funding the Palestine Mandate administration, and evidence of battles between the Foreign and Colonial Offices and the Treasury over the question of financing Mandate Palestine are discussed in B. J. Smith 1993.

83 Oliphant, L. 13 March 1922, Letter to Treasury, T161/1256; Meiklejohn, R. S. 23 March 1922, Treasury Minute T161/1256.

84 Davies, H. E. 21 March 1922, Treasury minute, T161/1256; Barstow, G. L. 6 April 1922, Letter to Undersecretary of State, Colonial Office, T161/1256.

85 BSAJ 1924b – The amounts given are in the abbreviated form of pre-decimal British currency – pounds (£), shillings (s) and pence (d) as originally listed in the BSAJ's published accounts. Amounts designated £E refer to pounds issued by the Bank of Egypt, used in Palestine at the time.

School of Archaeology in Jerusalem" acknowledged the Foreign Office's important role as middleman between the British Schools and the UK government, and its "sympathy" and "steady and most effective support" with the Schools' "projects and difficulties".[86]

This relationship becomes even clearer considering that Curzon's support for archaeological research in Palestine (and particularly for the British School of Archaeology in Jerusalem) continued until his death. The BSAJ Annual Report for 1925 began with the Council's "wish to put on record their sense of the loss which has been suffered by the death […] of its Vice-President, the Marquis Curzon of Kedleston". The relationship continued, though with the election of Secretary of State for the Colonies Leopold Amery in his place. The new Palestine High Commissioner Herbert Plumer joined Amery, former High Commissioner Herbert Samuel, Field Marshal Edmund Allenby and the Archbishop of Canterbury in the BSAJ Vice-Presidential team.[87]

4.2 The BSAJ in Jerusalem

> On arrival in the country of his choice [the traveller, A. Th.] is recommended to […] take an early opportunity of getting in touch with the local British Archaeological school […] where he will receive advice what to look for and where and how to look, and assistance in procuring suitable equipment. Thus the traveller who starts from Athens or Jerusalem should apply at the British School of Archaeology.[88]

By 1929, despite the cessation of the government grant from 1928 and a formal split between the BSAJ and the DAP, the BSAJ was firmly established as a center for scholarship and advice, and a forum for Jerusalem 'society' – a 1925 notice in the *Palestine Bulletin* (an English-language newspaper in Jerusalem), records in its "Social and Personal" column that the High Commissioner Herbert Samuel attended K. A. C. Cresswell's lecture on "Moslem Architecture" held at the School.[89] Examining Agnes Conway's letters and diary entries from the spring of 1929 showcases the BSAJ as the hub that its Council members had been assiduously promoting to Treasury officials. Conway's archive also indicates that as a newly arrived archaeologist, she was immediately introduced to the archaeological community in Palestine, the men and women attached to the various international schools, illustrating the value of the School for scholarly networking. She recorded in her diary that:

> I must say it's awfully nice to be met here, find everything arranged, + a programme complete + just to be taken in hand. [George Horsfield] is introducing

86 This typescript memorandum can be found in T161/1256.

87 BSAJ 1926.

88 Hill 1920, 8.

89 *Palestine Bulletin* 1925.

> me to all the schools + archaeologists, so that I can use their Libraries […] It really is wonderful coming back to Jerusalem after exactly 2 years in such a privileged position. I can't tell you how happy I am […][90]

At the BSAJ, she met John Crowfoot, Garstang's successor as Director, and his wife, Molly, who Conway wrote "knows everything about weaving".[91] She also met several BSAJ students who helped her prepare for the forthcoming excavation: Dorothy Garrod advised Conway on food supplies for Petra and Elinor Ewbank provided her with books on surveying techniques. Garrod, Ewbank and Mary Kitson-Clark, whom Conway met on her way to Jerusalem, were about to begin their own excavations at El-Wad Cave.[92] During her stay in Jerusalem Ewbank, Conway and Kitson Clark took a day trip to visit a Russian nuns' commune in Ain Karin (Ein Kerem).[93]

The Dominican scholar Pere Savignac of the French École Biblique was also introduced to Conway; he promised to join the Conway-Horsfield party for part of the excavation.[94] The relationship between the British and French Schools was close at the beginning of the BSAJ's history. The School's minute book shows that John Garstang initiated friendly relations with the French School, and George Horsfield, had spent some months studying at the École Biblique during his BSAJ training.[95] W. F. Albright, Director of the American School, was also introduced to Conway during her time in Jerusalem and she was taken through the School's Library.[96] Albright lectured to BSAJ students, and helped to solidify the close relationship between the British and American Schools.[97] Both Savignac and Albright worked with Horsfield to excavate or survey sites in Transjordan.[98]

Another person whom Conway met on arriving in Jerusalem was Horsfield's close friend Austen Harrison, the Chief Architect for the Palestine Government Department of Public Works, mentioned briefly in the preceding section. Harrison was already involved in designing and building the Palestine Archaeological Museum (now the Rockefeller Museum), which eventually provided offices for the Department of Antiquities

90 Conway, A. 1 March 1929, Letter to "Jinky and Baby Brother", University of Cambridge Archives: MSS Add 7676/R261-638/R435.

91 Conway, A. 28 February, 10 March 1929, Diary Entries, Cambridge University Library: MSS Add 7676/Z31. – Garstang resigned his positions at the British School and the Department of Antiquities in 1926. Molly Crowfoot's expertise cemented her role as an eminent textile archaeologist (Crowfoot 2004).

92 See Callander and P. J. Smith 2007. – Conway, A. 22 February, 3 and 10 March 1929, Diary Entries, Cambridge University Library: MSS Add 7676/Z31.

93 Conway, A. 16 March 1929, Diary Entry, Cambridge University Library: MSS Add 7676/Z31.

94 Conway, A. 7 March 1929, Letter to "Jinky and Baby Brother", Cambridge University Library: MSS Add 7676/R261-368/R436.

95 British School of Archaeology in Jerusalem, 19 February 1919, Minute Book 1: Palestine Exploration Fund Archives; Horsfield, G. c. 1924–1936, typed statement, "Mr Horsfield's File", Israel Antiquities Authority Archives: IAA ATQ 4088/Box 5.

96 Conway, A. 1 March 1929, Diary Entry, Cambridge University Library: MSS Add 7676/Z31.

97 British School of Archaeology in Jerusalem, 26 September 1921, Minute Book 1: Palestine Exploration Fund Archive; see also Gibson 1999.

98 E. g. Albright 1924; Bellamy 1988, 370.

as well as exhibition space for the collection of artefacts excavated in Palestine.[99] Harrison's close relationship to the archaeological community had begun just after the war in Athens, where Harrison was a visiting architect at the British School at Athens.[100] Harrison also drew plans and interpreted some of the BSAJ's early excavations; these plans and notes were published in the School's *Bulletin*.[101]

For Agnes Conway, George Horsfield brought everything together. She wrote in a letter that "Mr Horsfield is complete master of the situation here. Talks fluent Arabic, knows everybody + is perfectly calm + capable".[102] However, she was able to make her own contribution to the initial plans for the work through financing. Horsfield had minimal funding for his own excavations, so Conway's personal connection with the Mond family was particularly useful.[103] A family of industrialists and scientists, the Monds were interested in Palestine affairs and were family friends of the Conways.[104] Alfred Mond, a Liberal Member of Parliament, was President of Economic Board for Palestine.[105] He also gave money to the Pro-Jerusalem Society.[106] As the Minister of Public Works during the First World War Alfred Mond took a lead role in creating the Imperial War Museum, bringing in Martin Conway, Agnes Conway's father, as its first Director General, while Agnes Conway herself worked on the Women's Work Sub-Committee.[107]

Alfred Mond's brother Robert Mond was one of the BSAJ's most generous financial supporters; he had established a prehistoric research studentship at the BSAJ to which he contributed £500, along with making other regular donations and acting as the School's Honorary Treasurer.[108] Robert Mond was also a long-time supporter of John Garstang's work, having been a member of two of Garstang's "Excavation Committees" funding research in Asia Minor and Sudan before the First World War.[109] Henry Mond, Alfred Mond's son, contributed £500 to the Conway-Horsfield Petra expedition, with the expectation of a detailed report on the site's potential.[110] The networks involved in Agnes Conway's welcome to Jerusalem as a scholar and archaeologist bring together twin strands of archaeological research: British-based funding and the social-intellectual nexus of the BSAJ in Jerusalem. This nexus was itself comprised of London and Palestine based political and scholarly actors.

99 See Fawzī 2006. – Harrison, A. 1 February 1929. Letter to E. T. Richmond, Israel Antiquities Authority Archives: Harrison Legacy Box.

100 Harrison, A. 18 July 1927. Candidate Separate Statement. Israel Antiquities Authority Archives: Harrison Legacy Box.

101 E. g. BSAJ 1922, pl. I; Harrison 1925; Thornton 2011a.

102 Conway, A. 19 March 1929, Letter to "Jinky and Baby Brother", University of Cambridge Archives. MSS Add 7676/R261–638/R438a+b.

103 Thornton 2011a.

104 See Greenaway 2004.

105 See Anonymous 1922b.

106 Storrs 1949, 311.

107 See Evans 1966; Kavanagh 1994; Thornton 2011a.

108 Gibson 1999.

109 Thornton 2011a; Thornton 2013.

110 Conway, A. 5, 9 September 1928, Diary Entry, Cambridge University Library: MSS Add 7676/Z30; Conway, A. 7 February 1929, Diary Entry, Cambridge University Library: MSS Add 7676/Z31; Horsfield, G. 28 April 1929, Petra Excavation Diary, UCL Institute of Archaeology Archives: Horsfield Collection Box 8; Thornton 2011a.

5 Conclusion

The case studies presented here encapsulate both the ideal and the practical in the history of archaeology. Evaluating the terms of and networks behind the BSAJ's government grant highlights a debate in the heart of Whitehall about the role of British schools of archaeology and the political and intellectual value of archaeological research: whether it was part of British prestige or a "scientific luxury" during post-war economic austerity.[111] In the early years of the Mandate the Foreign and Colonial offices supported the BSAJ to enhance British prestige in a new British-led administration and develop a closer relationship with Britons overseas as part of a wider imperial agenda. While the Treasury reluctantly designated funding for a brief period, ultimately its officials sought to avoid committing British taxpayers' money to risky new ventures overseas in a new and untested political framework. By the late 1920s, Whitehall's support had been removed despite the best efforts of Foreign Office and Colonial Office officials and archaeologists, creating an even more pressing need for generating support through public subscription.

The local network on the ground in Jerusalem was well developed by the time Agnes Conway arrived in 1929. When the framework of administration as shown in Figures 1 and 2 are combined with the three relationship categories, a new and more nuanced interpretation of the 'archaeological network' appears. In Figs. 3 and 4, the blue transactional relationship lines show just how many people were contributing to archaeological research in Palestine and Transjordan, whether through training or funding, while red lines show how personal familial and friendship links were an important part of the development and financing of archaeological work there. Green organizational relationship lines illustrate the connections between the archaeological and the political administration, and highlight the role of the various Jerusalem and London based groups in bringing the archaeological and political worlds together, while the underlying framework (in Figs. 1 and 2) demonstrates just how 'peripheral' Departments of Antiquities were from the 'center' of politics in London, and even the center of politics within the Mandate administrations.

The links between Transjordan and Palestine are also revealed more clearly. For archaeology, these links were even closer when considering that George Horsfield, the British 'advisor'/Chief Inspector to the Transjordan Government on antiquities, was trained in Jerusalem and had personal and professional connections to the archaeological community there, shown in Fig. 3 with transactional, organizational and personal relationship lines. Evaluating evidence using these three broad relationship types enables a researcher to move beyond a segmented approach to history and archaeology.

111 E. g. McNeill, R. M. 13 December 1926, Treasury minute, T161/1256.

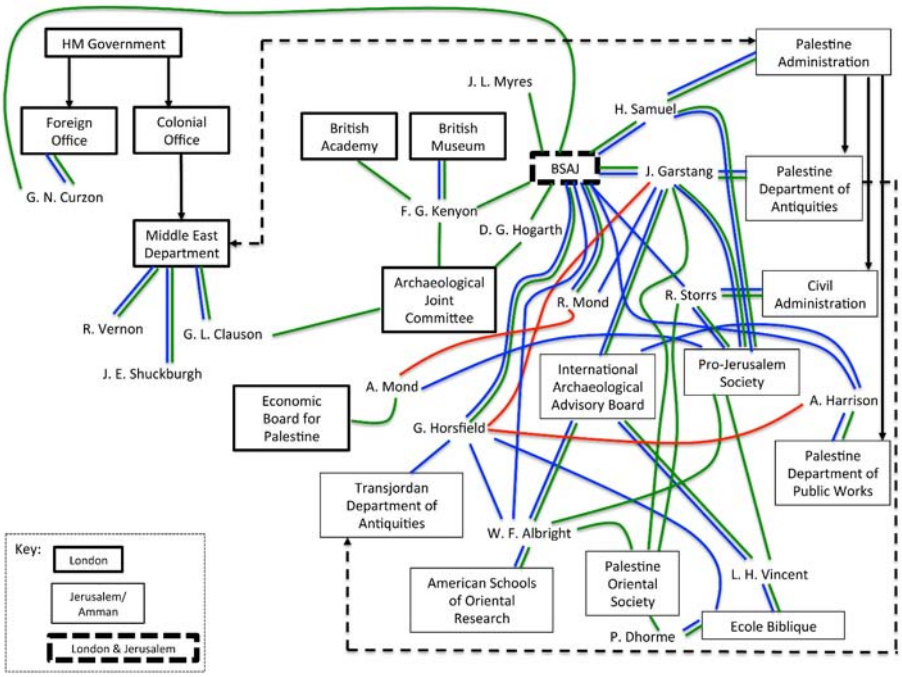

Fig. 3 The London-Jerusalem-Transjordan network with relationship links.

The relationship approach can create space for the contributions of 'outsiders' who are still regularly dismissed or excluded from the traditional history of archaeology narrative. It also serves to highlight areas where relationships might exist, indicating places for future research.

All the people discussed in this paper had some part to play in the development of the discipline; either through their work as archaeologists; through collective affiliation with or membership in training facilities, learned societies and committees; by providing financing, logistical support or skilled expertise; or through association with the 'political' side of archaeology, working for a government department or taking part in a government initiative. The history of archaeology should recognize and accept these members of the archaeological 'fringe' with the 'scientists' much more frequently; analyzing their collective activities will help us appreciate how archaeology developed. Political support, while rarely substantially financial, enabled much work to be done by 'qualified' archaeologists; personal connections, such as Agnes Conway's friendship with the Mond family, ensured that individual projects were able to come to fruition.[112]

112 Thornton 2011a.

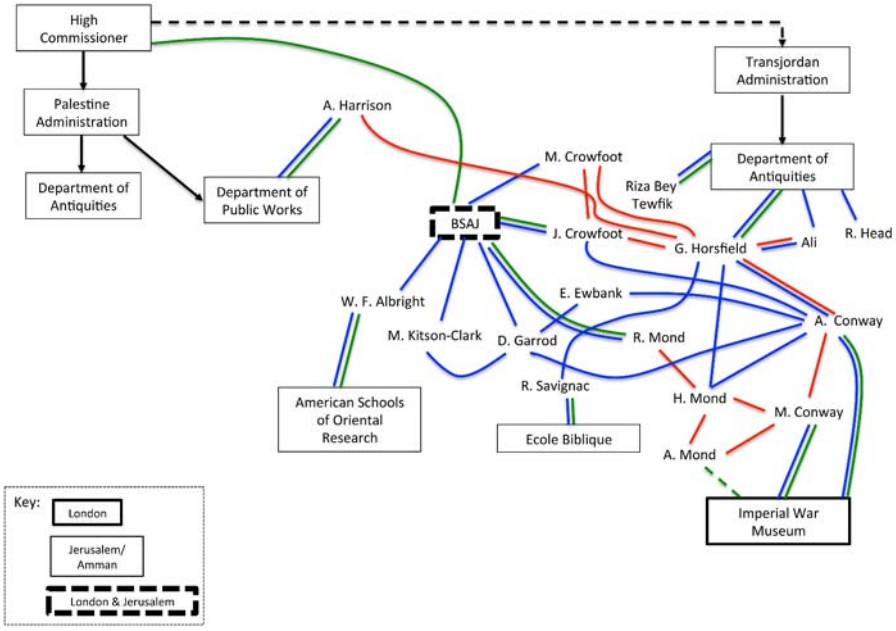

Fig. 4 The BSAJ's relationship links.

Analyzing the BSAJ's networks contributes to our understanding of how different government departments and administrations view and interact with archaeology. On a small scale, Agnes Conway's experience highlights Jerusalem as a social and intellectual center for Mandates, and an important base for archaeological work in both Palestine and Transjordan. The BSAJ's London and Jerusalem networks on a wider scale reveal a transnational impact despite the School's 'new' status. These networks worked to maintain Britain's scholarly presence in Jerusalem during the Mandate period; their legacy ensures that the BSAJ continues to exist today.

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The Long Revolution of Radiocarbon as Seen through the History of Swiss Lake-Dwelling Research

Summary

This paper reassesses the implementation of radiocarbon dating in archaeology based on the technique's development while researching ancient lake dwellings in Switzerland between 1950 and 1970. The aim is to explain archaeologists'initial failure to accept the results obtained by this method. Two key issues are thereby the core focus of this analysis. The first concerns the disciplinary context that influenced the reception of 14C dating among prehistorians. The second deals with methodological discussions concerning 14C dating and dendrochronology, being radiocarbon dating's most related chronological tool. While dendrochronology and 14C were first complementary in the 14C calibration process since the 1960s, it was then quickly realized that dendrochronology produced more detailed temporal data due to the good preservation conditions of wooden structures at Swiss lake dwellings and thus competed with 14C results. In fact, this competition had to do with the two differing methodologies of data acquisition and time measurement.

Keywords: History of science; 14C; dendrochronology; Swiss lake-dwelling research.

In diesem Artikel soll die Anwendung der Radiokarbondatierung in der Archäologie neu untersucht werden, basierend auf ihrer Entwicklung während der Erforschung der Schweizer Seeufersiedlungen zwischen 1950 und 1970. Ziel ist es zu erklären, warum die Archäologen die durch diese Methode gewonnenen Resultate zunächst nicht akzeptieren konnten. Zwei Schlüsselfragen leiten die Untersuchung: Die erste fragt nach dem disziplinären Kontext, der die Rezeption von 14C unter Prähistorikern beeinflusste. Die zweite bezieht die methodologischen Aushandlungen über C14 und der damit verbundenen Methode der Dendrochronologie mit ein. Während sich C14 und Dendrochronologie seit den 1960er Jahren im Kalibrationsprozess ergänzten, lieferte die Dendrochronologie darüber hinaus Einzeldaten, die gerade im Bereich der Pfahlbauten mit ihrer guten Holzerhaltung der C14-Datierung rasch Konkurrenz machten. Tatsächlich hing diese Konkurrenz mit den unterschiedlichen Arten der Datengewinnung und Zeitmessung zu tun.

Keywords: Wissenschaftsgeschichte; 14C; Dendrochronologie; Schweizer Pfahlbauforschung.

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1 Introduction

American researchers developed the 14C dating method about 1950. In the last decades of the twentieth century, methodological handbooks and historical accounts of archaeology promoted this as a highly valuable tool – particularly with regard to the revision of Neolithic and Bronze Age chronologies in Europe and to the understanding of cultural change during these periods.[1]

However, this literature has tended to characterize the development of the method as linear in its trajectory and constant in its effects. For example, Colin Renfrew has reinforced this view by focusing primarily on the heuristic impact of the method on the development of archaeological thought.[2] This leading British archaeologist and early proponent of the 14C dating method used the word "revolution" to describe the invention, arguing that the scientific community experienced a paradigm shift with regard to the interpretation of cultural change once it had embraced the reliability of 14C.[3] Needless to say, this description of the archaeological community's reception of the method is partial – it represents the point of view of one archaeological radiocarbonist.

The historiography of radiocarbon dating has tended to focus on the success of the method as well as on its heuristic significance for the development of archaeology, emphasizing revolutionary moments of methodological innovation. Moreover, due to their common interest in promoting a linear and progressive narrative of archaeological practice, the authors of these publications draw a stark dividing line between archaeology before and after radiocarbon.[4] As Renfrew puts it:

> Sixty years ago, it was in general not possible to date archaeological finds with precision unless these could be related to one of the historical calendars, whether Egyptian, or Chinese or indeed Maya. Vast parts of the globe lacked any secure chronology. Dating was nowhere possible before about 3000 BC. Now a secure chronology is available everywhere, so long as organic materials are available for dating.[5]

1 Renfrew 1973; Stöckli 1986, 13.

2 Renfrew 1973.

3 Renfrew 1973; Renfrew was not the first one to use this term. In 1952, O.G.S. Crawford, the editorialist of the British journal *Antiquity*, talks about a "revolutionary discovery" (Crawford 1952, 177).

4 E. g. Evin and Oberlin 1998; Renfrew 1973; Renfrew 2009.

5 Renfrew 2009, 122.

This historiography also suggests that, once they appeared, the results produced by this method were quickly accepted without dispute. Such a one-sided historiography has many shortcomings. First, though this literature generally emphasizes the origin and development of radiocarbon in laboratories and the results contributed by this tool to the study of prehistory, it has not sufficiently documented the processes by which the prehistorians adopted this tool during the second half of the twentieth century. Second, the socio-political context surrounding the production of this method in the 1950s has not been accounted for, despite the decisive role it played in the rapid pace of development and diffusion.[6] Third, the disciplinary historiography has misrepresented the long lapse of time between the invention of the method and the recognition of its results by members of the archaeological community. In other words, this historiography does not give a satisfying description of the relationship between the tool and its potential users – in this case archaeologists. Finally, the integration of dendrochronology into the calibration of 14C is generally treated as a mere technical detail, which obscures the important role this second method played in the gradual acceptance of radiocarbon dating by archaeologists.

Based on an analysis of the development and reception of 14C and dendrochronology in Swiss lake-dwelling research,[7] we seek to explain the gap between archaeologists' recognition of the methodological innovation and their acceptance of results thereby obtained. After 1854, when the first lake-dwelling settlements were discovered by the antiquarian Ferdinand Keller, such settlements quickly became a very popular domain of research.[8] Given the wet atmosphere of these areas, organic remains – including those of wood constructions, seeds and plants – were very well preserved, which encouraged naturalists and archaeologists to collaborate. Since the 1950s, the excellent conservation of such organic remains has enabled 14C and dendrochronology to be used in parallel. Focusing on the period between the 1950s and the 1970s, our analysis hinges on two key issues. The first concerns the disciplinary matrix – between the humanities and the natural and exact sciences – that influenced the reception of 14C among prehistorians. During this period, Swiss prehistorians tried to stabilize their discipline and to improve its standing among the sciences. Collaborations with natural and exact sciences were seen by some prehistorians as a mean of reinforcing their own discipline. The seductive power of the 14C method was particularly strong due to its origin. Alliances between prehistory and nuclear physics were also highly regarded by the National Science Foundation. The second issue involves the methodological negotiations between 14C and dendrochronology.

6 There are some exceptions, however, which mostly concern the history of the method in the United States. Marlowe 1980; Marlowe 1999; Nash 1999; Nash 2000. Regarding Europe and Germany, also see Billamboz 2004.

7 Delley (in press).

8 Kaeser 2004.

We pursue two avenues of analysis in what follows: one structural, and the other biographical. Regarding structure, we will situate the development of the method in pre-existing political and social contexts (i. e. the pacification of nuclear research programs after WW II), and related, though external, technical innovations (i. e. dendrochronology for the calibration of 14C) that played a decisive role for the stabilization of 14C in the field of archaeology. In particular, we will tackle the structural features of the allied disciplines (nuclear physics and botany) involved in the development and application of the method in archaeology, as well as the epistemic impact of dendrochronology's ability to inscribe probabilistic time upon a "real-year" calendar. The biographical point of view aims at shedding light on the fate of these two methods in the daily practice of archaeology. The ways in which archaeologists reacted to this innovation differed depending on their personal epistemological orientations and research backgrounds.

2 Diabolizing Milojčić and making dissidents invisible

According to the disciplinary historiography, the entire archaeological community quickly accepted radiocarbon as a decisive tool. Only one European prehistorian is supposed to have resisted: the German archaeologist Vladimir Milojčić. A professor of prehistory at the University of Heidelberg, Milojčić published a book in 1949 on the chronology of central and south-western Europe,[9] which he had established using the common archaeological method of cross-dating. Like many of his contemporaries, Milojčić combined a diffusionist perspective on the question of culture change with the study of artefact assemblages in closed contexts such as tombs. If these contained objects imported from Egypt, Crete or Greece – cultures that used texts and calendars before the rest of Europe – this provided archaeologists with a means for building absolute chronologies. But even early radiocarbon evidence called these archaeological chronologies into question, and with the advent of calibrated dates in the mid-1960s, this interpretative system was further eroded.[10] If Milojčić had criticized the new method with good reason, his skepticism can't be interpreted merely as an ideological rejection of a high chronology for the Neolithic and the early Bronze Age in Europe. In rejecting this possibility, Milojčić was seen by radiocarbonists as someone who either didn't want to put forth the effort to understand the details of radiocarbon dating, or as a narrow-minded traditionalist who refused to engage with specialists from other disciplines.[11] His opposition, however, stemmed from different issues, among them, the power dynamics between archaeology (assigned to the humanities) and the natural and exact sciences. He developed his objection in an article published in *Germania* where he clearly

9 Milojčič 1949.

10 See Ferguson, Huber, and Suess 1966.

11 Schwabedissen and Münnich 1958.

expressed his opposition to the monopolization of crucial chronological questions in archaeology by the radiocarbon method.[12] Even if Milojčić was the most visible representative of the opposition to the 14C method in archaeology,[13] it appears that many of his contemporaries were also skeptical, though they were less vocal or simply declined to state their position. The unreliability of the results obtained by the 14C method in those years was a primary cause of such resistance. Another point was certainly, as Milojčić pointed out, the fact that archaeologists already had their own dating methods; as long as the new approach didn't deliver reliable results, i. e. results in accordance with archaeological chronologies, there was no need to give too much weight to the 14C dates. Nevertheless, as mentioned above, archaeologists did take notice of this innovation. Moreover, the seductive power of 14C, as well as of the natural and exact sciences, remained decisive for archaeologists.

3 The relationships between the sciences around and after the 1950s

The establishment of the 14C dating method in America was facilitated by nuclear research infrastructures and the competencies of scientists involved with the military-industrial complex. The context of the Manhattan project, which led to the fabrication of the atomic bomb, was especially significant in this regard.[14] Beginning in the late 1940s, many scientists specializing in nuclear physics found new research opportunities in the development of non-military applications – for example, in medicine, agronomy, energy production, and isotopic dating methods. In most industrialized countries, governments subsequently invested in these domains, a move that is exemplified by the political program "Atoms for Peace", launched in mid-1950s America. In Switzerland, politics and science were similarly linked in the case of the National Science Foundation, where a fund was specifically created in 1958 for financing basic and applied research in the nuclear domain. From 1945 until this date, nuclear research had been overseen by a Commission for atomic science, which depended directly on the Swiss government for financial and scientific support.[15] Such massive investments furthered and contributed to the diversification of several disciplines – archaeology, geology, climatology, and botany among them – and led, notably, to the 1957 creation of a radiocarbon laboratory in Berne, entirely financed by the National Science Foundation. Fully integrated with the knowledge-production regime which was implemented during the

12 Milojčič 1957.
13 Milojčič 1957; Milojčič 1958; Milojčič 1959; Milojčič 1964.
14 Marlowe 1980; Marlowe 1999.
15 Joye-Cagnard 2010, 118.

Cold War in the framework of nuclear pacification programs, "a grouping of institutions, beliefs, practices, politic and economic regulations which delimitates the mode of being sciences",[16] the 14C dating method has, since its origin, had significant social and cultural authority among the sciences.

State administration and control beginning in the middle of the twentieth century changed the relationships between the humanities and the natural and exact sciences in a way never witnessed before. Indeed, most industrialized nations developed government-supported institutions responsible for administrating scientific research between the interwar period and the 1950s. The structuring effects of such institutions – the *Centre national de la recherche scientifique*, the *Deutsche Forschungsgemeinschaft*, the National Science Foundation in Switzerland and Belgium, among other examples – were important. Concretely, while these institutions were created to support science, especially in the domain of basic research,[17] they also defined priorities and norms that were intended to accommodate new expectations regarding the relationship between science and society.[18] In addition, they incorporated new categories of actors (administrators and policy makers, for example), and this modified the position of scientists and the place of science in competitions among nations. In such competitions, roles for the humanities and sciences clearly emerged. For a discipline like archaeology, alliances with well-established and authoritative sciences, such as physics, helped increase both the authority of the discipline and its visibility among scientists and the general public.[19]

Besides promising to yield knowledge in the domain of prehistory, collaborations between archaeologists and physicists were also thought to be a way of increasing the scientific status of results at a time in which procedural reproducibility and quantitative methods were so important. The establishment of a 14C laboratory in Bern in 1957 reveals such interests: In this case, the prehistorian Hans-Georg Bandi (1920-) initiated alliances with physicists for precisely these reasons. Together with Max Welten, a botanist with similar interests in 14C dating, and the nuclear physicist Hans Oeschger, Bandi created a 14C laboratory at Bern's Institute of Physics.[20] What is more, from 1957 on, they managed to obtain the support of the National Science Foundation, which encouraged archaeologists in the use of this method. Through 14C dating, archaeology thus benefitted directly from the powerful position physics, and in particular nuclear physics, occupied among the sciences after World War II.

Archaeologists also characterized the use of natural scientific evidence in prehistoric research in terms of its modernity, despite the fact that such evidence had been in

16 Pestre 2003, 35. – Translation by the author.

17 Fleury and Joye 2002.

18 Concerning the influence of the Swiss National Science Foundation on the development of Swiss archaeology, see Delley 2013.

19 Bourdieu 2001, 134.

20 Archives of the Swiss National Science Foundation, Bern. Application no. 962 (H.-G. Bandi and M. Welten), 16.5.1956. Division I; Application A 42, Intermediate report 1.4. 1959-31.3.1960, 11.4.1960. Division II; Application A 228, 7.6.1962. Division II.

circulation since the 1860s.[21] In the field of wetland archaeology, the 1954 centennial anniversary of the discovery of the lake-dwellings presented a good opportunity for touting the newfound scientific quality of this field of research. Walter Guyan (1911–1999), who excavated an important lake-dwelling settlement at Thayngen-Weier and was in charge of editing *Das Pfahlbauproblem* (The Lake-Dwelling Problem), the book published in honour of the commemoration, clarified in the introduction to the volume that the authors "tried to discuss the problem taking into account in particular the progresses of the methods and of the '*Hilfsmitteln*'" – namely those tools stemming from the natural sciences.[22] Emil Vogt (1906–1974), curator at the Swiss national Museum and professor of prehistory at the University of Zurich, was heavily involved in the publication of *Das Pfahlbauproblem*,[23] in which the first 14C dates concerning Swiss prehistory were published. Vogt deliberately emphasized in a letter to the book's editor that "this volume is *not* a commemorative volume, but a collection of more scientific works on the lake-dwelling problem".[24] This generation of prehistorians underlined the modernity of their research by drawing attention to the use of "scientific tools" derived from the natural and exact sciences. Presenting prehistory as a multidisciplinary field of research was all the more important when such a definition was one of the priorities articulated by the new scientific administration. Depicting prehistory as a unified element within modern science as a whole became part of the rhetoric chosen by members of the National Science Foundation to exemplify a new way of practising scientific research:

> In recent decades, a new 'style' of research has been affirming itself in many areas of science. Major projects can no longer even "get off the ground" without cooperative work from veritable groups of scientists: a result of ever increasing specialization. The business of an excavation, for example, was in the last century the work of someone such as Schliemann, assisted by his wife and a few faithful handlers of the pick and shovel; today it is necessary that the archaeologist and the prehistorian collaborate with the physicist (in physical procedures to determine dates), with the botanist (in pollen analysis), with the specialist in dendrochronology (in the determination of annual layers in the trunks of trees), with the parasitologist (in the determination of the internal parasites of the inhabitants), with the entomologist-archaeologist (in the determination of insects for prior periods), with the palaeontologist and with the chemist.[25]

The scientific tenor of these excavations was also thought to provide a guarantee of objectivity. It became necessary for these scholars to emphasize field observations and empiri-

21 Kaeser 2011.
22 Guyan 1955. – Translation by the author.
23 Vogt 1955.
24 Letter Vogt to Guyan, 14.7.1954, Correspondence Vogt. Swiss National Museum Zurich. – Translation by the author.
25 Von Muralt 1963, 13. Thanks is due to Yan Overfield Shaw for translating the quote.

cal data collection, rather than interpretation. This helped them to establish boundaries between their own research practices, which they defined as modern and scientific, and those of previous generations of antiquarians and amateurs, reaching back to the nineteenth century. This supposedly more objective approach, focusing on concrete facts, was on display in a documentary entitled *Lake Dwelling Research in Switzerland*.[26] This film was ostensibly produced for the same commemorative purposes as the 1955 volume entitled *Das Pfahlbauproblem*. Unlike the book, however, it was intended for a wide public including amateurs as well as specialists. The documentary produced by the archaeologist Hans-Georg Bandi devoted considerable space to issues of methodology and procedures, and was filmed in such a way as to emphasize the scientific nature of Swiss archaeology. Steps taken by archaeologists and naturalists in the film were depicted as inerrant and systematic, while interpretive discussions of their findings are almost entirely absent, leaving even more place for empirical observations. The promotion of objective practices made it possible to counterbalance the subjective dimension of the debate regarding the position of the lake-dwelling villages, a debate which has taken place between German and Swiss archaeologists since the 1920s.[27]

Regarding the institutionalisation of 14C, the creation of a laboratory in Bern – which benefited, as we have shown, from the Swiss government's massive financial investment in nuclear research – anticipated what would become a common implementation of the method in archaeology. While the method was developed between 1949 and 1955, for a significant number of Swiss archaeologists its utility and necessity was still not yet obvious at the end of the 1950s. The expense of radiocarbon dating was a significant obstacle for archaeologists. However, with support from the National Science Foundation, they were able to reap the benefits of free dating – given that the laboratory itself was financed by the same institution. In this way, the National Science Foundation indirectly promoted the use of 14C amongst archaeologists. Nevertheless, there were relatively few demands for archaeological determinations between 1957 and the 1970s, as compared with requests for botanical and geological samples.

In sum, if the pragmatic and positivist rhetoric of Swiss archaeology could be fulfilled by scientific procedures imported from other domains – physics, geology, botany – practitioners continued to interpret 14C results with caution. In truth, the results obtained by the method were imprecise and inconsistent; laboratories in this case do not appear to have mastered the situation, despite claims to the contrary.

26 Bandi 1960.

27 See Rückert 1998, 87–88; Kaeser 2004, 107–108.

4 Concepts and categories are discipline-bound

There were other important reasons why the collaboration between different parties involved in the development and application of the method faltered. First, physicists and archaeologists did not agree on the meaning of a reliable method, and, by extension, a reliable date. While the ability to reproduce measurements on different equipment was a primary concern for physicists and chemists, from the point of view of archaeologists, the result of the measurement had no value in itself; rather it only took on meaning in an archaeological context. Unlike physical scientists, who believed that a date could be published as soon as it had been obtained in independent laboratory contexts, archaeologists maintained that the value of a result could only be established within the context of archaeological observation – involving stratigraphy, cultural-historical comparisons, and typology, for instance. A 14C date would be judged true or false only on the basis of its correspondence to results from these other domains, which were the tools archaeologists had traditionally used to establish their chronologies. These two different conceptions of what constituted acceptable methodology were not readily compatible, and this misunderstanding had far-reaching disciplinary ramifications that bring us back to the complexity of collaborations between natural sciences, exact sciences, and the humanities.

Second, as already mentioned, archaeologists had long presumed 14C dates to be true only in cases where these dates could be corroborated by archaeological chronologies. If 14C dates called these traditional chronologies into question, the dates tended to be ignored and were not included among archaeological results. Until the 1970s, when the first calibration curve was produced, archaeological chronologies were the primary means for verifying the reliability of radiocarbon dating in the absence of historical calendars – among laboratory researchers as well as archaeologists. This explains why, for three decades following the first tentative use of radiocarbon dating, prehistorians published essentially uncalibrated radiocarbon dates which more or less corresponded to archaeological chronologies, without estimating equivalencies in calibrated, i. e. calendar years. Such calibrated results nevertheless became available in the mid-1960s,[28] but they indicated that the European Neolithic was much older and had lasted much longer than archaeologists had been able to establish on the basis of traditional methods alone. In 1970, measurement of Neolithic piles at Auvernier-La Saunerie confirmed a new high chronology of the European Neolithic,[29] but despite Swiss prehistorians'

28 Ferguson, Huber, and Suess 1966.

29 Suess and Strahm 1970. – The results published by Ferguson et al. in 1966 were based on the measurement of the Neolithic piles of Thayngen-Weier and Burgäschisee, two Middle Neolithic sites (Pfyn and Cortaillod cultures) of Switzerland. The results obtained were 3700 and 3760 ± 40 BC, which means 1000 years older than the uncalibrated 14C dates (Ferguson, Huber, and Suess 1966, 1177). In 1970, the Neolithic piles of Auvernier-La Saunerie measured using the 14C method and subsequently calibrated were assigned to Late Neolithic levels. The

familiarity with questions of dendrochronology, references and commentaries regarding these new results were rare. To understand this wait-and-see attitude, we must first consider the fact that the community of prehistorians didn't believe in such high dates – which means a middle Neolithic beginning around 3600 BC instead of 2600 BC – and preferred to ignore them.[30] Indeed, Swiss prehistorians were waiting for these new dates to be confirmed by the continuous oak dendrochronological referential curve. The German botanist Bruno Huber had started to build this referential curve in the 1940s[31] based on measurements of oak wood samples taken from historic and prehistoric buildings in Southern Germany and in Switzerland. Many Swiss archaeologists who were involved with the excavation of lake-dwelling settlements from the 1950s onwards took part in this project. This referential curve, on which each oak dendrochronological curve produced for prehistoric settlements had to be correlated in order to date the settlements in real years, would not be established until the middle of the 1980s.[32] Until this date, Swiss archaeologists maintained that new 14C chronologies had to be taken with precaution. So there was a clear discrepancy between, on the one hand, the development of the method and its implementation, and, on the other hand, the full-fledged acceptance of its results, signalled by their integration into archaeological reasoning.

5 A genuine interest in naturalist methods, but doubts about 14C: Emil Vogt

The tentative attitude archaeologists showed towards 14C results did not derive from a refusal to collaborate with other disciplines. The case of Emil Vogt is a good example. Vogt debated the question of method, and, given his position as curator at the Swiss National Museum and professor at the University of Zurich, his views had considerable influence. In the context of lake-dwelling excavations, Vogt did not hesitate to assert his point of view when excavators failed to adhere to procedures he had mandated concerning the surface of the excavations, the documentation of discoveries, and especially the drawing of archaeological remains *in situ*. Like German prehistorians active in the domain of *Moorarchäologie* (archaeology of marshes) – Gustav Schwantes,[33] Hans Reinerth[34] and Hermann Schwabedissen[35] – Vogt believed in the potential of palynology, botanics and (since the 1950s) dendrochronology as means of furthering knowledge about lake-dwelling settlements. Vogt was especially interested in the question of the

result obtained was 2400 BC instead of 2000 in uncalibrated years.

30 Drack 1969; Stöckli 1986, 13.

31 Huber 1941; Huber and Jazewitsch 1958.

32 Becker, Billamboz, and Egger 1985.

33 Schwantes 1939.

34 Reinerth 1940.

35 Schwabedissen 1949.

positioning of the lake-dwelling settlements, which he situated on the shore and not in the lakes.

The first 14C dates obtained in Copenhagen on the basis of Swiss prehistoric research, came from the settlement of Egolzwil 3, a site Vogt identified as the most ancient Neolithic settlement in Switzerland following excavations there at the beginning of the 1950s. The impulse to date samples from Egolzwil 3 didn't come from Vogt himself, however, but from the Danish sedimentologist and botanist Jens Troels-Smith, a member of the *Moorlaboratorium* at the National Museum of Copenhagen. Troels-Smith, who had been collaborating since the beginning of the 1950s with Vogt and other Swiss archaeologists who were part of his circle – especially Josef Speck, Walter Guyan, and Hans-Georg Bandi – was interested in Egolzwil 3 due to its presumed high antiquity. With the 14C determinations he sought to establish whether the Swiss Neolithic was older or younger than the Danish Neolithic. Troels-Smith had already studied botanical evidence coming from these two areas, and had also established palynological calendars for Switzerland and Denmark. 14C measurements would now date such climatic and environmental events absolutely, and further, would determine when neolithization, as a cultural and social event, occured in both of these areas.[36]

While Vogt admired Troels-Smith's methods of observation compared to the approaches of other Swiss botanists, which he found a bit outdated[37] – he nevertheless remained cautious about the results provided by 14C determinations. Regarding the first results obtained at Egolzwil, Vogt said:

> Your first radiocarbon dates contain a very large margin of error. I wouldn't dare to calculate an average from these two results. In itself, the date of 2600 would fit well, whereas the date of 3200 looks too old. I agree with you when you say that it is too early for concluding anything from these two measurements.[38]

Hence, it is no surprise that when Hans-Georg Bandi asked Vogt to furnish some archaeological wood samples from the Swiss National Museum in 1954 – his aim being to calibrate new equipment at the laboratory in Bern – Vogt answered that recent communications published on the method were far from satisfactory. He mentioned in particular the dating of the site known as Zug-Sumpf, which Frederik Zeuner derived at the University of London, as well as the results obtained for Egolzwil 3 and Thayngen-Weier.[39] It is important to note that Vogt's position was not unique. Reviewing the 14C dates of Egolzwil 3, the prehistorian Marc-Rodolphe Sauter also emphasized that the 14C dates

36 Troels-Smith 1955; Troels-Smith 1956.

37 Letter Vogt to Guyan, 12.5.1952. Correspondence Vogt. Swiss National Museum Zurich.

38 Letter Vogt to Troels-Smith, 5.3.1953. Correspondence Vogt. Swiss National Museum Zurich. – Translation by the author.

39 Letter Vogt to Bandi, 21.5.1955. Correspondence Vogt. Swiss National Museum Zurich.

didn't fit well with the shallower chronology most prehistorians had adopted: Sauter believed that many cross-checks needed to be done in order to satisfy archaeological and naturalist requirements.[40] Thus, despite his profound interest in applying the natural sciences to prehistory, on the questions of chronologies, Vogt didn't expect much of the 14C method. Like many contemporary archaeologists, his epistemological orientation when it came to dating methods was oriented towards typology and cross-dating. For Vogt, results brought by physicists should first confirm the chronologies established by archaeologists before their contribution to the business of archaeology could be evaluated properly.

6 Reconciling two conceptions of time

Alongside archaeological chronologies, dendrochronology became a second safeguard for archaeologists using 14C methods – both in the context of lake-dwelling research and in archaeology more generally. As a method that was external to archaeology, dendrochronology revealed 14C inconsistencies. Moreover, in the 1960s this method became the most expedient way to transform 14C dates into real calendar years and has remained so ever since. The symbolic capital of dendrochronology was thus reinforced, just as its potential to generate highly precise dates had already been recognized.

In the eyes of archaeologists, dendrochronology and 14C didn't have the same heuristic potential. Since the mid-1960s, dendrochronology had been central to the interpretation of lake-dwelling villages – it contributed to the production of detailed relative chronologies of settlements, the restitution of different phases of construction, and the analysis of phases of abandonment and occupation, to name some examples – whereas radiocarbon had primarily been a means of obtaining an average dating of the different phases of occupation witnessed by a given a settlement.[41] And while archaeologists didn't expect precise results from the radiocarbon method, they were also aware of its inaccuracy: "We will convince physicists and also some archaeologists that for once archaeological pieces of evidence about relative continuities are more evident than their measurements."[42] In Feldmeilen-Vordefeld, Twann and Auvernier-La Saunerie archaeologists clearly exposed these problems by comparing the results of dendrochronology with those of 14C.[43] The critical benefit of dendrochronology, as compared with 14C hence became greater and greater in a wetland context. For archaeologists such as Alasdair Whittle and Jean-Paul Demoule, 14C gives a false impression of continuity within

40 Sauter 1955, 152.
41 Furger 1980; Gallay 1965; Schwab 1989.
42 Winiger 1976, 55. – Translation by the author.
43 Winiger 1976; Furger 1980; Schifferdecker et al. 1989, 25.

a site occupation and between different cultural phases due to the margin of error accompanying each date.[44]

Nevertheless, the relationship between 14C and dendrochronology was not a struggle between two absolute dating methods. The disagreement between these two methods had rather to do with the differing conceptions of time they reproduced. While archaeologists faced difficulties with the interpretation of 14C time, which was cast probabilistically, dendrochronological and archaeological time could easily be compared, given that the unity of time – one tree ring produced annually – corresponded directly to the rhythm of an annual calendar. This was true despite the fact that archaeologists did not immediately gain access to an absolute dating method. Indeed, until the end of the 1970s, dendrochronology only yielded *relative* dating means.

One of the specific qualities of the method that was quickly appreciated by archaeologists was that it could, in a best-case scenario, measure time within an error margin of one year. This meant that archaeologists could measure the durations recorded in prehistoric materials – piles and timbers used in building and renovating prehistoric villages, as well as diverse artefacts made of wood – to an accuracy of a few years, which was uncommon for this kind of research. The many different construction and renovation phases witnessed by these settlements could thereby be revealed. Moreover, given the high precision of the dating process, the analysis of wood could provide archaeologists with a relative calendar of the occupations and abandonments of the sites that could be compared with environmental data (e. g. climatic changes, lake levels) and also with archaeological calendars (typology, artefact importations, cultural changes, and the like). The ability to establish, within a short period of time, regular phases of occupation and abandonment along the lakeshores caused by high water periods constituted a decisive conceptual change in the interpretation of the lake-dwelling settlements.

Furthermore, dendrochronology motivated archaeologists to explore several of the cultural, social and historical choices made by prehistoric people more fully than the radiocarbon method. The analysis of wood provided insight into the priorities of lake-dwelling builders with regard to the age, size and species of the trees that were chosen. By regrouping pieces of wood according to felling year and examining the ways in which trees grew, dendrochronologists could classify trees that came from the same zones in tandem. The repetition of such observations, correlated with current botanical observations in the forests, confirmed that prehistoric people controlled and managed their forests, which in turn pointed to territorial organization in timber cultivation. This may well have led to inter-settlement organization, which made it possible to study prehistoric occupation in a large zone such as Auvernier Bay, excavated between 1969 and 1975. The cultural dimension of dendrochronological time was that much more significant in the 1960s and 1970s, when the study of relationships between man and his natural

44 Whittle 1988; Demoule 1995.

environment became a renewed source of preoccupation within contemporary society as well.

7 When wood produces effects

Toward the end of the 1950s, the popularity of dendrochronology in lake-dwelling research was on the rise. However, in the context of non-wetland archaeology, where wood was rarely preserved, dendrochronology had not been at issue. Archaeologists in non-wetland environments thus had much higher expectations for the 14C method. In fact, increasing disappointment was also more strongly felt in this milieu, especially when the results delivered by the 14C method failed to match archaeological assumptions. However, as mentioned before, archaeologists who dared to take a clear position on the method were few, and rather than unleash a critical explosion in print, they protested via a "silent mutiny".[45] This phenomenon can be seen indirectly from the large number of articles published by radiocarbonists during the second half of the 1950s (written either by physical scientists or by archaeological devotees) intended to convince skeptics to make use of the 14C method, despite some methodological difficulties laboratory workers were still trying to understand.

From that time on, the role of dendrochronology became decisive for the stabilization of the 14C method. When physicists at the end of the 1950s discovered that the amount of 14C had not been constant in the atmosphere over time, it became obvious that all the dates obtained by this method were in need of correction. Dendrochronology appeared to be the best solution. Samples of prehistoric woods from living *sequoia gigantea*, living and fossilized *pinus aristata* from California, and prehistoric oak piles from Swiss lake-dwelling settlements were sent to 14C laboratories in Europe (Heidelberg, Copenhagen, and Groningen) and America (e. g. La Jolla, where a special program on 14C calibration was started at the end of the 1950s). Such "trading zones",[46] in which collaborations among archaeologists, dendrochronologists and physicists intensified over time, provided radiocarbonists with an "artificial reality"[47] – a calibration curve by which calendar dates eventually could be obtained. This new curve yielded results in a time that could finally be understood by archaeologists in real solar years. In other words, prehistoric wood was an intermediary: through processes of calibration, it helped translate probabilistic time into a historical or calendar time accessible to archaeologists.

However, the contributions of dendrochronology were not limited to the calibration process. Dendrochronology also afforded archaeologists an opportunity to recon-

45 Callon 1986, 201.
46 Galison 1996.
47 Galison 1996, 142–151.

cile themselves to the 14C method, which experienced many set-backs in these years and produced inconsistent results that archaeologists did not take seriously. While physicists tried to suggest the method was under control[48] and required only minor adjustments, some prehistorians, who upheld the reliability of the method, tried to harmonize two worlds separated by an epistemological boundary. The role these archaeologists played in such transformation processes corresponds to Latour's translator.[49] Among them were Harm Tjalling Waterbolk (1924-), who trained as an archaeologist and a botanist in Holland and spent his mature career at the Biologisch-Archaeologisch Instituut in Groningen, and Hanjürgen Müller-Beck (1927-), who trained at the University of Tübingen as a specialist on the Palaeolithic with a strong naturalist orientation. Both emphasized in the 1960s that archaeologists had to have reliable results in order to trust the method. But in light of the fact that significant work had yet to be done before the method could produce reliable results, it was deemed important to keep archaeologists informed about it and to try to convince them, despite the situation, that 14C could be useful in the field of archaeology. To achieve this, Waterbolk and Müller-Beck used very concrete case studies to demonstrate the potential of the method.[50] Such examples contrasted with the theoretical ones physicists used.

Müller-Beck's demonstration regarding the 14C results obtained from the important lake-dwelling settlement at Burgäschisee is telling.[51] Müller-Beck tried to reconcile these results with other methods commonly used in prehistory (typological, botanical, and stratigraphic) in addition to dendrochronological evidence. Marshalling these various tools, Müller-Beck tried to relativise the importance of absolute dating in archaeology by turning the discussion towards the questions of duration – which he understood to fit better with archaeological observation[52] – rather than fixing the debate on the question of the precision of the 14C results, as was usually the case. At stake in his demonstration were not precise dates, but rather reliable durations that were not competing with archaeological chronologies. In other words, Müller-Beck tried to familiarize archaeologists with dendrochronological time, in order to steer their expectations towards the 14C method. Despite this interesting perspective, Müller-Beck's way of conceiving time remained mostly limited to the context of lake-dwelling research, where dendrochronology was always more closely aligned with the archaeological approach, and especially with the interpretation of the settlements. Given the subsequent development of dendrochronology in this context, it continued to be easier to relativize 14C here than in dry-land archaeological contexts.

48 Broecker and Kulp 1956; Barker 1958.

49 Latour 2005, 37. – As translators, these intermediates tried to convince different actors – archaeologists who were not convinced by the method – in order to rally them behind the common cause of stabilizing the 14C dating method in the field of archaeology.

50 Waterbolk 1960; Müller-Beck 1961.

51 Müller-Beck, Oeschger, and Schwarz 1959.

52 On this question see also Olivier 2001.

8 Conclusion

Before the 1970s, 14C dating did not satisfy the archaeological community. Even though the German archaeologist Vladimir Milojčić has usually been thought to be the only figure to resist the method, many sceptical archaeologists remained silent and are thus absent from disciplinary histories of the method. Based on the analysis of the reception of 14C within Swiss lake-dwelling research, this paper has aimed to shed light on why 14C continued to seduce members of the archaeological community despite obvious problems. In the 1950s, new expectations defined by the norms and priorities of scientific administrators were increasingly appropriated by archaeologists, who took over the promotion of the sciences in prehistory. But the situation was still more complex. If the power dynamics between the humanities, natural sciences, and exact sciences encouraged some archaeologists to highlight their collaborations with other research domains beginning in the 1950s, they remained tentative when it came to 14C results. In fact, these results could call traditional archaeological chronologies into question. Dendrochronology, however, produced accurate durations rather than absolute dates, and thus was not in direct competition with archaeological chronologies. The dendrochronological approach to temporality grew increasingly popular among prehistorians working in the field of wetland archaeology. Distinct from the kind of results archaeological chronologies could produce, dendrochronology also opened up new dimensions in the interpretation of archaeological time, both from a material *and* cultural perspective. Furthermore, through the calibration process, dendrochronology came to play a decisive role in the recognition and stabilization of 14C in the context of archaeology. Its power to translate radiocarbon calendars into calendar years has progressively helped archaeologists reconcile themselves to the 14C method.

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Objects of Knowledge in Modern Settlement Archaeology. The Case of the Iron Age *Fürstensitze* ('Princely Residences')

Summary

This paper attempts to analyze modern settlement archaeology as a kind of 'experimental system' that by technical means generates new 'objects of knowledge'. The productivity of such a perspective can be demonstrated by looking more closely at the development of modern settlement archaeology in Germany during the late nineteenth and early twentieth century. The objects of knowledge that constitute this field of research were not present from the beginning, but developed only gradually out of field archaeological practice. During this 'experimental' process on-site observations were combined with insights from more or less distant contexts, often in a quite unsystematic manner. Among the more complex objects of knowledge generated by modern settlement archaeology is the so-called Fürstensitz, or princely residence, of Central European Iron Age research.

Keywords: Pre- and protohistory; settlement archaeology; Iron Age; history of archaeology; history of science; experimental systems; objects of knowledge.

In diesem Beitrag wird die moderne Siedlungsarchäologie als eine Art ‚Experimentalsystem' analysiert, das durch technische Mittel neue ‚Wissensobjekte' generiert. Die Produktivität einer solchen Perspektive wird durch die genauere Betrachtung der Entwicklung der modernen Siedlungsarchäologie in Deutschland im späten 19. und frühen 20. Jahrhundert veranschaulicht. Die Wissensobjekte, die dieses Forschungsfeld konstituieren existierten nicht von Anfang an, sondern entwickelten sich sukzessive aus der feldarchäologischen Praxis. Während dieses ‚experimentellen' Prozesses wurden Feldbeobachtungen mit Beobachtungen aus mehr oder weniger entfernten Kontexten mitunter in einer ziemlich unsystematischen Art und Weise miteinander verknüpft. Eines der komplexeren Wissensobjekten, das die moderne Siedlungsarchäologie geschaffen hat, ist der so genannte ‚Fürstensitz' der mitteleuropäischen Eisenzeitforschung.

Keywords: Ur- und Frühgeschichte; Siedlungsarchäologie; Eisenzeit; Archäologiegeschichte; Wissenschaftsgeschichte; Experimentalsysteme; Wissensobjekte.

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1 Introduction

One of the more complex objects of knowledge generated by modern settlement archaeology is the so-called *Fürstensitz* (princely residence) of Central European Iron Age research. This concept, which is still used today to describe a special form of concentration of political and economic power in early Iron Age Central Europe, was explicitly formulated by Wolfgang Kimmig (1910–2001) in the late 1960s in reference to his own fieldwork on the Early Iron Age hill-fort called Heuneburg near Hundersingen in the Upper Danube region in Southern Germany.[1] Today *Fürstensitze* are either seen as a historical reality beyond any reasonable doubt,[2] or as a theoretical construct that has to be carefully checked against the available archaeological evidence – and possibly modified or even abandoned.[3] It is no secret that my own preferences rest on the side of the latter position.[4] My objections are less directed towards the model itself, as presented in the 1960s by Kimmig, than towards its uncritical application in the decades that followed. Fifty years ago, Kimmig's model clearly stimulated Iron Age research, but in the course of time the concept has become more and more of a burden for the development of appropriate ideas concerning social and cultural developments in the middle of the 1st millennium B.C.

Such reflections are of no immediate relevance to this paper and therefore need not be substantiated here. Instead, I will try to consider the problem here mainly from the perspective of an external observer. Thus, I discuss the *Fürstensitz* neither as a historical reality, nor as a model which requires further verification. Instead, in the context of this paper, *Fürstensitz* is primarily meant to designate an "epistemic object"[5] produced by (prehistoric) archaeologists to help bring the available evidence into a form that allows insights into the (political) structure of Iron Age societies of the Northern Alpine region.

Consequently, my reflections will focus on the 1950 and 1960s, when the 'epistemic object' *Fürstensitz* was coined. But my primary concern is not to re-evaluate this definition in the sense of a legitimization or a critique. By historicizing and contextualizing the *Fürstensitz* concept I hope to reveal a special epistemic constellation characteristic of prehistoric research in the middle of the twentieth century.

2 Epistemic objects in prehistoric archaeology

In the sciences, epistemic objects are means to create reliable, new knowledge concerning the structure of the world in which we live. They are able to accomplish this task only

1 Kimmig 1969.
2 E. g. Biel 2007.
3 E. g. Eggert 1989.
4 E. g. Veit 2000.
5 Rheinberger 2001.

in combination with 'technical objects' (particle accelerators, for example). The latter's task is to function reliably and flawlessly.[6] Technical objects also play a significant role in the fabrication of archaeological knowledge. This is especially obvious with regard to the multitude of archaeological field techniques, but it also applies to many processes in the field of archaeological find analysis, such as the seriation of find complexes or the cartographic representation of spatial distributions, that are primarily of a technical nature. These techniques should not be seen as isolated scientific tools, but as more or less closely related to the 'epistemic objects' or 'objects of knowledge' under examination. In the context of settlement archaeology for example 'the culture layer', 'the post hole', 'the pit house' and 'the dwelling pit' may be regarded as typical objects of knowledge. These objects were not present from the beginning, but developed only gradually out of archaeological fieldwork undertaken in the late nineteenth and early twentieth century on a relatively small number of key sites.[7]

Apart from such elementary objects of knowledge, prehistoric archaeology, and in particular settlement archaeology, has created much more complex objects of knowledge. A good example is 'pile dwelling', an object of antiquarian research in the second half of the nineteenth century. Its discovery (or rather 'construction') is strongly associated with the work of the Swiss historian and antiquarian Ferdinand Keller (1800–1881). Keller based his conclusions concerning prehistoric relics found together with impressive fields of wooden posts at the shores of many Swiss lakes on experience gained while exploring Swiss and English prehistoric dry land sites.[8]

Thus, the pile dwelling phenomenon ultimately came into existence in a process that could be characterized as 'experimental'. But this process clearly was not limited to on-site observations. Rather the information gathered 'in the field' was combined in an imaginative way with observations from more or less distant contexts. The unsystematic method repeated itself, as observations in foreign contexts used as analogies were not themselves investigated thoroughly; instead, seemingly relevant 'facts' were taken from general education. In this sense field archaeology should not be regarded as a closed experimental system that produces results only through highly controlled processes conducted onsite. To the contrary, systems of this kind are not only open to external influences, but would not work without such external input.

Processes similar to those at work in pile dwellings in the nineteenth century can be detected in early research on prehistoric fortifications (*Burgenforschung*). Both fields of research contributed considerably to the development of modern settlement archaeology, and hence to the discovery that discolorations in the soil (representing post holes

6 Mehrtens 2008, 37; see also Rheinberger 2001.

7 As e. g. the Bronze Age settlements *Römerschanze* near Potsdam and Berlin-Buch or the early Neolithic site of Köln-Lindenthal.

8 Cf. Trachsel 2004.

or pits) could be used to reconstruct the outline of buildings and even of whole settlements. In this context it is necessary to mention the excavations of Carl Schuchhardt (1859–1943) in the Roman legionary camp at Haltern in Westphalia as well as in the prehistoric 'castles' of northern Germany.[9] These were among the first attempts at reconstructing architectural structures built without stones.

From this point it was only a small step to the large-scale excavations carried out in the first half of the twentieth century, for example by Werner Buttler (1907–1940) and Waldemar Haberey (1901–1985) at Köln-Lindenthal[10] or by Gerhard Bersu (1889–1964) on the Goldberg hilltop near Nördlingen.[11] These archaeologists exerted a strong influence on more recent settlement archaeology, not only in Germany (Kimmig and other archaeologists of the next generation took part in the Goldberg excavation and gained their first field experience here) but for example also on the British Isles, where Bersu involuntary spent several years during World War II.[12]

3 The *Fürstensitz*-concept as a complex object of knowledge

The development of even more complex objects of knowledge like the *Fürstensitz* or as well the so called *Herrenhof* – 'chief's farmstead', a large enclosed complex – of later prehistory becomes intelligible only in such a context. Their 'creation' is to be seen in the context of large, state-financed excavation projects in post-war Germany, such as the Heuneburg excavation in south Germany[13] or the excavation at the Feddersen Wierde near Cuxhaven,[14] which offered an opportunity for refining such initially vague objects of knowledge as the *Fürstensitz* or the *Herrenhof* by means of systematic excavation and documentation of larger parts of selected, well preserved sites. They relied from a technical point of view on the experience of an older generation of excavators, who had passed down their knowledge to a younger generation of archaeologists, mostly during fieldwork.

In the case of the Heuneburg, the term *Fürstensitz* first occurs in the subtitle of a report on the 1950s excavations directed by Kurt Bittel (1907–1991) from Tübingen University and Adolf Rieth (1902–1984) from the Tübingen unit of the State Heritage Management Program.[15] Both referred to older publications from the 1870s by Eduard Paulus ('the Younger', 1837–1907), who had persisted in viewing the mound of the Heuneburg hill-fort as related to some adjacent tumuli. These tumuli – due to the rich grave finds discovered during unsystematic excavation in one of these burial mounds –

9 On Schuchhardt see: Grünert 1987.
10 Buttler and Haberey 1936.
11 Cf. Parzinger 1998.
12 Evans 1989; Krämer 2001.
13 Kimmig 1968; Kimmig 1969; Kimmig 1983.
14 Haarnagel 1979; cf. Burmeister and Wendowski-Schünemann 2006.
15 Bittel and Rieth 1951.

had been labeled the 'Hundersinger Fürstengrabhügel,' prince's burial mounds of Hundersingen.[16] Yet, only in 1921 was Walter Veeck (1886–1941) able to prove by means of small-scale excavations that a tentative connection existed between these tumuli and the hilltop-site.

It was not until 1948 that the first systematic topographic and archaeological survey of the site, with its surrounding ramparts and ditches was initiated. The results of the 1950s excavations have been summarized by Bittel and Rieth in a small booklet which appeared in 1951 (Fig. 1). Here the late nineteenth century argumentation that used the funerary evidence to give meaning to the adjacent settlement site has been reversed for the first time. Bittel and Rieth claim that their excavation of the defensive structures of the site (Fig. 2) has, for the first time, justified labelling Heuneburg a *Fürstensitz* of the Early Iron Age. Furthermore, the neighborhood of the large mounds and princely burials seemed to fit into this picture.[17] The authors also mention the absence of undisturbed, peaceful development within the settlement, since the excavations revealed at least three successive defensive systems of differing structure as well as signs of destruction and fire. Finally, Bittel and Rieth sketch out a working program for the years to come that was supposed to solve open questions by means of systematic archaeological fieldwork.

In 1951, Wolfgang Kimmig and Wolfgang Dehn took over as directors of the Heuneburg project. In the decades that followed it was Kimmig, who had followed Bittel as head of the Institute of Pre- and Protohistory at Tübingen University, who primarily carried on the research and popularized the Heuneburg within archaeological circles and beyond (Fig. 3). In diverse publications he elaborated on the conclusions of Bittel and Rieth. A short publication from 1955 exhibits a certain terminological change, with Kimmig replacing the term *Fürstensitz* with the term *Adelssitz* ('noble seat'), emphasizing the aristocratic structure of the supposed ruling elite of the Heuneburg. Kimmig claimed that the political structure as described in Caesar's report on the Gallic war (58–52 BC) may well have been in existence half a millennium earlier.[18] Although this argumentation was at that time widely accepted among his colleagues, Kimmig's terminological proposal, which he renewed in his programmatic paper of 1969, did not prove successful in the long run.

In 1968 – after 14 years of systematic fieldwork on the Heuneburg – Kimmig published a first synthesis in the form of a guide addressed to the wider public. Here he presented the Heuneburg as a citadel for the nobility quite different from medieval castles (Fig. 4). Far from emphasizing the uniqueness of this site, Kimmig argued that citadels

16 For details see Schweizer 2006, 82–85, with references.

17 Bittel and Rieth 1951, 53.

18 Kimmig 1955, 301.

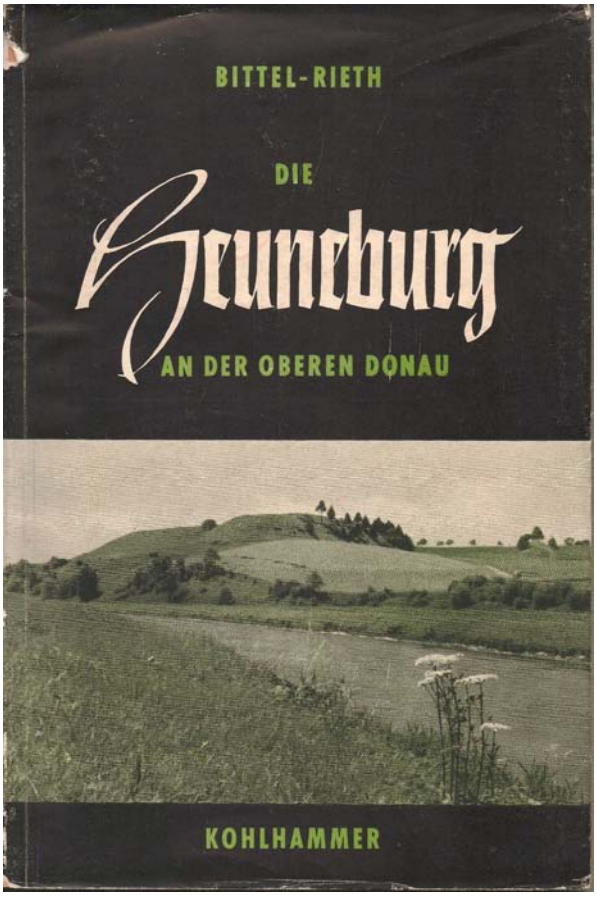

Fig. 1 Cover of the booklet "Die Heuneburg an der oberen Donau" of 1951.

of this kind may have existed at a distance of 15 to 20 km. Only later did Kimmig attribute a much more exclusive character to his *Fürstensitze*, which is made clear by the distribution map he added to a number of his publications.[19] This map (Fig. 5) was reproduced with minor modifications by many scholars in the decades that followed.

But let me come back to the guide from 1968, the interpretative parts of which remained practically unchanged in a new edition published in 1983. Here Kimmig gives a vivid description of how he imagined Early Iron Age society in south-western Germany. He confronts the reader with a ruling dynasty, vassals and slaves, as well as with farmers, craftsmen and priests. In the center of Kimmig's ideas stands a kind of charismatic leader, ruling the Heuneburg-region with 'patriarchal calm':

19 Kimmig 1983, 9; Kimmig 1990.

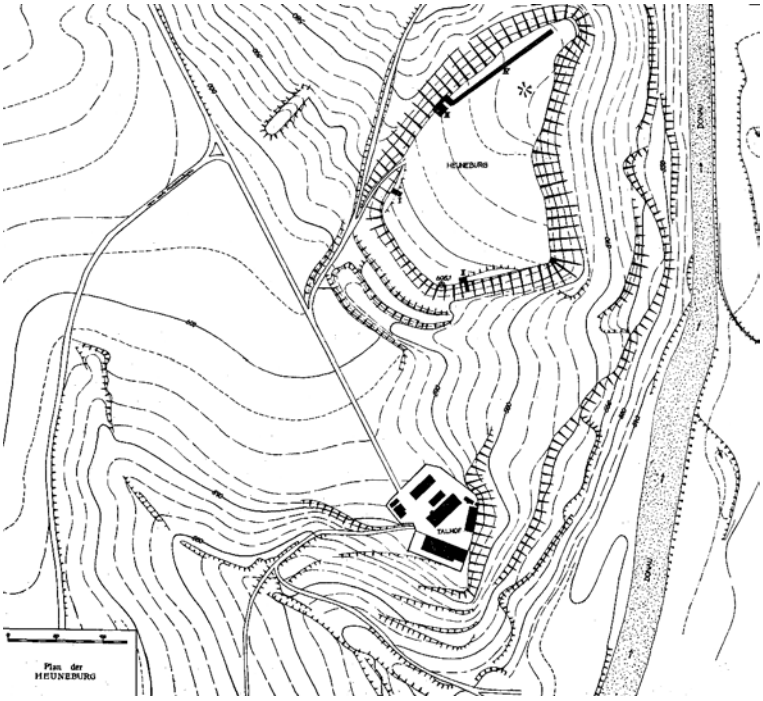

Fig. 2 Topographical situation of the Heuneburg-hillfort after Kurt Bittel and Adolf Rieth in 1951.

> We may think of this man as a dynamic ruler, who had at his disposal the means to rebuild the settlement mound and thereby to create a power center of his own dynasty. This man obviously was conscious about his descent, since he placed emphasis on being buried in the middle of his people.[20]

Kimmig concludes that, although the "name and dynasty of the citadel's noblemen will be wiped out forever [...] fourteen years of patient and planned research were enough to generate a historic landscape from what had been a terra incognita."[21] Here it becomes clear that Kimmig's reasoning is informed by an implicit analogy between the planned layout of the Heuneburg settlement and the systematic, long-term research on the site. This places Kimmig and his fellow excavation directors residing in Hohentübingen castle, where the Institute of Pre- and Protohistory of Tübingen University has been housed since the early 1920s,[22] in a certain sense at the same level as the ruling elite of the Early Iron Age. In both cases a kind of 'dynastic structure' is visible (although in academic circles descent normally lacks a biotic element). I will not go into the details of such an

20 Kimmig 1968, 100. Translation by the author.
21 Kimmig 1968, 119.
22 Veit 2006.

Fig. 3 Wolfgang Kimmig (1910–2001) in 1955 at his office at the Institute for Pre- und Protohistory of Tübingen university, located in Hohentübingen castle.

ethno-psychoanalytic perspective within the history of archaeology,[23] since the information given so far is sufficient to illustrate the dominant role of historical imagination in Kimmig's work. He sketches out a historical scenario, but eventually leaves it to others to check the 'facts' carefully against the archaeological evidence.

Indeed, many problems concerning the structure of Iron Age society seem to have been solved before excavation had even begun. This may be illustrated by the following citation: "We still don't know to what extent the inner area of the Heuneburg was covered with buildings. However, the existence of a market run not only by local farmers is most likely."[24] Kimmig was convinced that foreign traders also offered goods and luxuries at this market. Given the rudimentary state of the typographic and comparative analysis of the materials discovered during the excavations on the Heuneburg before 1968, it is astonishing to be confronted with such a detailed reconstruction. Apart from Riek's publication of his pre-war excavations in the Hohmichele burial mound in 1962 in the new monograph series *Heuneburgstudien*, publications on material from the Heuneburg did not appear until the 1970s and 1980s starting with Günter Mansfeld's 1974 work on the fibulas.

23 For the theoretical background of such an approach cf. Erdheim 1982.

24 Kimmig 1968, 122.

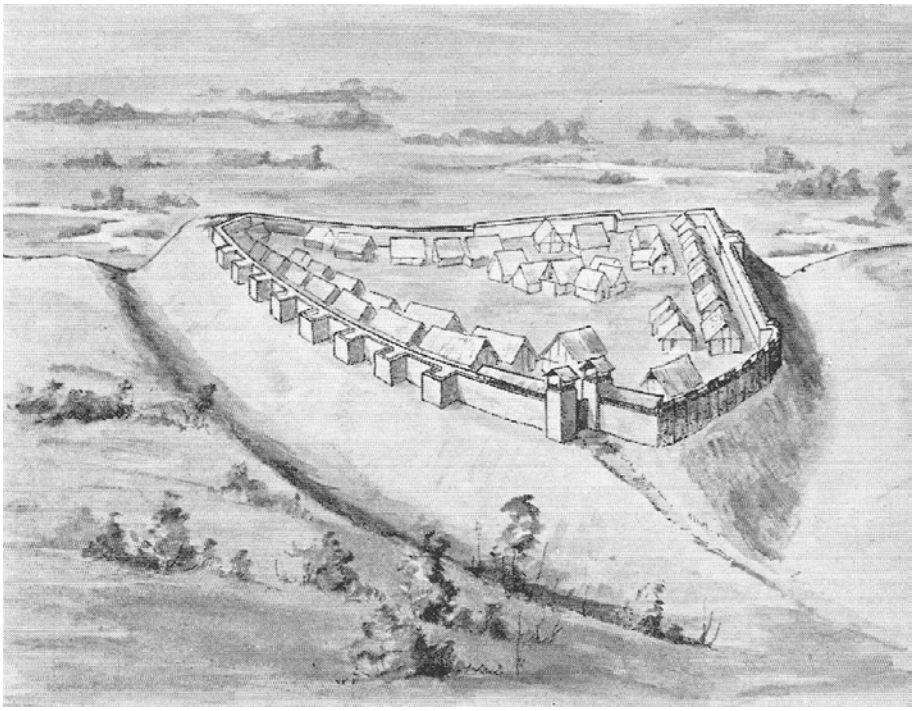

Fig. 4 Reconstruction of the *Heuneburg* with fortifications and buildings by Wolfgang Kimmig in 1968.

The stratigraphic sequence and the architectural evidence were finally published between 1989 and 1996 by Egon Gersbach,[25] who joined the project in 1963 and was responsible for all organizational and technical aspects of the excavation. From this time on a clear division of work becomes apparent. While Gersbach organized and supervised the more practical activities on the site, Kimmig concentrated his activities on integrative and comparative work. His interest was less focused on details than on the large lines of a historical interpretation. And quite similarly to his supposed *Fürst* with his far reaching contacts, Kimmig cared about the international recognition of the Heuneburg project. In this context it proved particularly useful for him to have been in contact with French prehistorians for decades.[26] In France, similar archaeological situations of hillforts surrounded by large burial mounds became apparent. In particular, the protohistoric monuments around the Mont Lassois in Burgundy seemed to reproduce the pattern discovered in the region of the Heuneburg.[27]

25 Gersbach 1989; Gersbach 1995; Gersbach 1996.

26 His contacts can be traced back to his activities in the office responsible for the protection of cultural heritage in France during the German occupation (see Olivier 2004).

27 Cf. Brun and Chaume 1997.

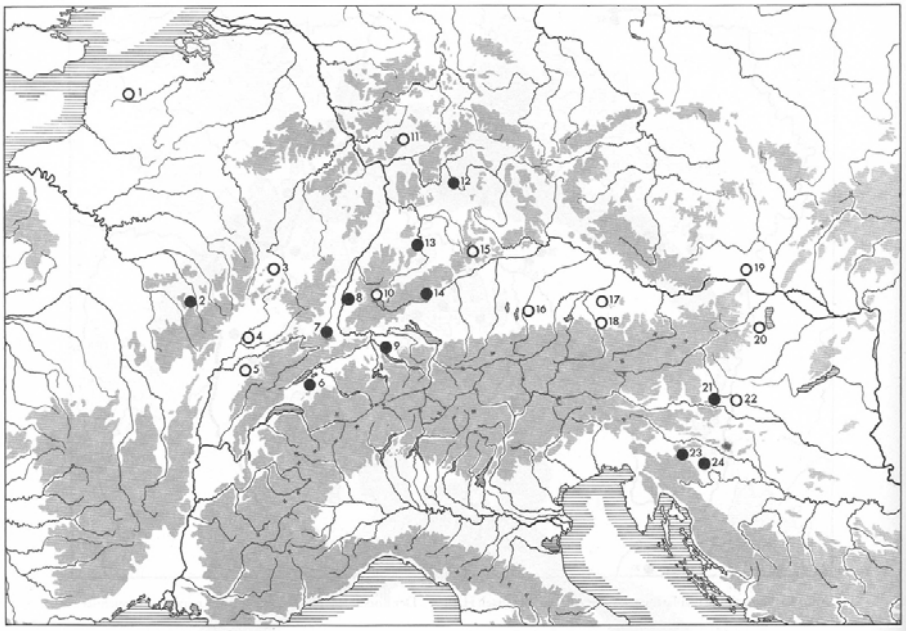

Abb. 5 Späthallstatt/frühlatènezeitliche »Fürsten«- bzw. Herrensitze in der Zone nördlich und südlich des Alpenbogens. – 1 Kemmelberg, comm. Kemmel, Westflandern. – 2 Mont Lassois, comm. de Vix bei Châtillon-sur-Seine, Côte d'Or. – 3 Sion, comm. Saxon-Sion, Meurthe-et-Moselle. – 4 Gray, Haute-Saône. – 5 Camp-du-Château bei Salins-les-Bains, Jura. – 6 Châtillon-sur-Glane bei Fribourg. – 7 Britzgyberg bei Illfurth, Haut-Rhin. – 8 Münsterberg von Breisach, Kr. Breisgau-Hochschwarzwald, Baden-Württemberg. – 9 Uetliberg bei Zürich. – 10 Kapf bei Villingen im Schwarzwald, Baden-Württemberg. – 11 Glauberg bei Glauburg, Wetteraukreis, Hessen. – 12 Marienberg von Würzburg, Unterfranken, Bayern. – 13 Hohenasperg bei Asperg, Kr. Ludwigsburg, Baden-Württemberg. – 14 Heuneburg a.d. oberen Donau, Gem. Herbertingen-Hundersingen, Kr. Sigmaringen, Baden-Württemberg. – 15 Ipf bei Bopfingen, Ostalbkreis. – 16 Kyberg bei Oberhaching, Kr. München. – 17 Hellbrunner Berg bei Hellbrunn, Land Salzburg. 18 Helpfau-Uttendorf, Oberösterreich. – 19 Michelberg bei Stockerau, Niederösterreich. – 20 Ödenburg-Sopron, Westungarn. – 21 Burgstallkogel bei Klein-Klein im Sulmtal, Kärnten. – 22 Radkersburg, Steiermark. – 23 Ringwall Vir bei Sticna (Sittich), Unterkrain, Slowenien. – 24 Novo Mesto (Rudolfswert), Unterkrain, Slowenien. – Gefüllte Kreise weisen auf Herrensitze mit einem Wahrscheinlichkeitsgrad über 50%, offene Kreise auf solche mit einem Wahrscheinlichkeitsgrad unter 50%.

Fig. 5 Spatial distribution of the *Fürstensitze* according to Wolfgang Kimmig in 1990.

Even though Kimmig strongly emphasized the important role of large systematic excavations in answering historical questions on an interregional scale, his procedure at first sight seems to deviate considerably from an 'experimental system' that regularly produces knowledge basically by technical means. Nevertheless, Kimmig's relatively simple concept of the *Fürsten-* or *Adelssitz* as outlined in his paper from 1969,[28] effectively stimulated empirical research well beyond the Heuneburg area in the decades that followed.

For the Heuneburg itself, examples of this more experimental approach can still be found around 1970. A paradigmatic case is Jörg Biel's thesis on prehistoric hilltop-

28 Significant features were the division of the settled area between acropolis and suburbium, the existence of rich burials under tumuli *(Fürstengräber)* in the vicinity of the *Fürstensitz* and the presence of imported goods, especially from the Mediterranean, inside the settlement and in the lavish tombs.

settlements in south-western Germany.[29] Even though this study was published nearly twenty years after Kimmig's paradigmatic paper, it was written in Tübingen at the high-time of the Heuneburg excavations (Biel presented his thesis to the faculty in 1972). Against this background it is not surprising that a chapter on Early Iron Age hilltop settlements is the most important part of the book. Here Biel tries to demonstrate the spatial efficacy of the *Fürstensitz* Heuneburg by means of a differentiated chronological analysis of the archaeological evidence from various hilltop sites in the vicinity of the Heuneburg.

Biel bases his argumentation on a distinction between four types of hilltop sites in the Early Iron Age[30], the third of which he labelled "Fürstensitz":[31] Type I comprises very small settlements located on hilltop positions, but lacking fortifications (this type is of minor importance for subsequent argumentation); Type II comprises real hilltop settlements in extreme positions, that is to say sites far away from the communication routes, and partly at high elevations. According to Biel's analysis these locations are chronologically restricted to the early 'Hallstatt D' phase from around 650 BC;[32] Type III comprises fortified settlements in favorable positions close to communication routes like the Heuneburg. For this category the term *Fürstensitz* is reserved; Type IV designates real hilltop sites which have fortifications, but which – unlike type II settlements – typically existed over the whole time span under investigation.[33] This last type according to Biel is to be found only beyond the territories of the *Fürstensitze*. In Biel's view it seems possible that these smaller hilltop settlements had a lasting existence when they lay outside the sphere of influence of a *Fürstensitz* (which according to Biel may have had a radius of up to 50 km). This seems to be confirmed by the spatial distribution of the sites presented by Biel on a small map (Fig. 6).

New research from the last few decades in this region has invalidated the conclusions Biel drew from this representation.[34] What seems important to me in the context of the arguments presented above is that by making things visible, Biel – in a limited sense – brought the *Fürstensitz* into being as an object of knowledge. By producing chronological charts as well as distribution maps, Biel made the *Fürstensitz* a reality even on a nonverbal level. In any case its character changed from that of an evocative historical term borrowed from historical studies to a 'real' object of knowledge that at least in part had been constructed experimentally.

Biel's arguments certainly might have been much stronger had he not attempted to formulate a particular historical interpretation of his observations at a very early stage in his analysis; he may indeed have stopped a promising 'experimental' process too early. In

29 Biel 1987.

30 The Early Iron Age includes the prehistoric phases 'Hallstatt C' and 'Hallstatt D', from around 800 BC to the middle of the 5th century BC.

31 Biel 1987, 145–150.

32 Middle of 7th to middle of 5th century BC.

33 That is, in archaeological terms, from early Hallstatt D to Latène A/B: 7th to 4th century BC.

34 Cf. also Biel 2007.

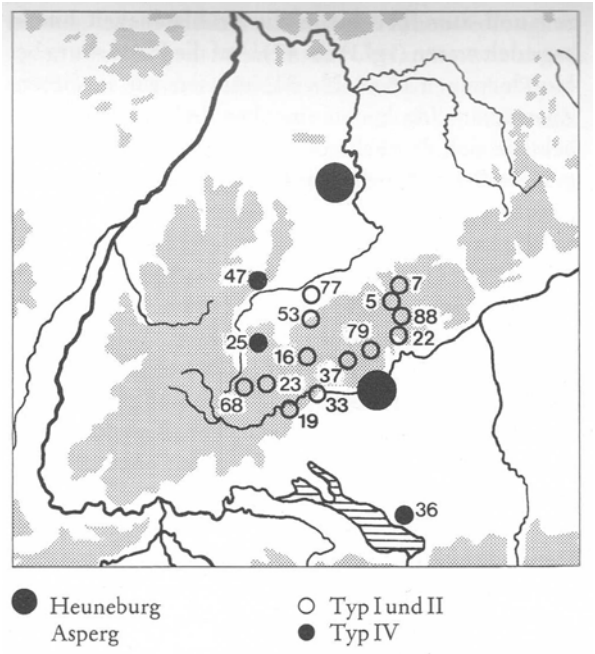

Fig. 6 Chronological and spatial distribution of Hallstatt-period hilltop sites in southern Württemberg (southwestern Germany) according to Jörg Biel in 1987.

any case it seems possible to imagine an archaeology with a much stronger emphasis on such practices of pattern recognition independent of concepts borrowed from (written) history. In this context mention should be made of more recent approaches that try to replace such evocative terms as *Fürstensitz* with more neutral terms such as 'central place' or 'complex center.'[35] On the other hand, the historical narratives which result from such approaches appear pale und lifeless to many scholars even today.[36]

According to Wolfgang Ernst there is a fundamental difference between 'cold' archaeological fieldwork and 'hot' historical imagination:

> An abyss separates both practices. […] It is the practice of historians to write a plausible history from fragmentary textual evidence in archaeology. Exactly here lies the difference between the archaeological field […] and the archive-fictions of the historians.[37]

Later on, Ernst continues:

35 Cf. Gringmuth-Dallmer 1996.

36 Cf. Biel 2007; Kolb 2007.

37 Ernst 2004, 237. – Translation by the author. Originally: „Eine Kluft trennt die beiden Praktiken […]. Es ist die Praxis der Historiker, aus fragmentarischen Textbefunden in Archiven plausible Geschichte zu schreiben. Genau hier unterscheidet sich das archäologische Feld (im doppelten Sinne) von den Archiv-Fiktionen der Historiker."

> Excavators 'on the site' deal with material rather than verbal contexts, in collaboration with *technicians* rather than scholars specialized on texts. The scientific scope of work of prehistorians cannot be fixed with the historian's vocabulary.[38]

Applying this distinction to the example presented in this paper, we may be inclined to look at Kimmig as representing the part of historical imagination and Biel as representing the part of archaeological fieldwork. A closer look shows that the situation is more complicated. As I have already tried to demonstrate, we certainly find elements of 'cold' archaeological reasoning in the publications of Kimmig, as well as strong elements of 'hot' historical imagination in Biel's contributions. And as far as I can see, such a dichotomy is implicit to all archaeologies.

4 Conclusion: Settlement archaeology as an experimental system?

The thesis behind this paper is that historiographical concepts developed within modern history of science, like 'experimental system', 'epistemic object' or 'object of knowledge', are perfectly applicable to prehistoric archaeology. The *Fürstensitz* discourse within Iron Age research has been used as an illustration. At a very early stage in Kimmig's research, a certain configuration of data became visible, which – according to his expectations of how Iron Age society worked – forced him to see the archaeological evidence as having a special 'form' or *'Gestalt'.* This *Gestalt* he labeled *Adelssitz,* and he formulated at the same time some rather vague criteria for identifying such higher-order settlements in the archaeological record. The task of making this specific *Gestalt* (for which a majority of scholars preferred to use the term *Fürstensitz*) visible in the archaeological evidence, Kimmig largely left to others. This was achieved by field work combined with attempts to present the emerging spatial pattern by means of maps, chronological charts and other kinds of illustrations (representing 'technical objects'). Along the way, the nature of the epistemic object called *Fürstensitz* permanently changed. Vague and blurred at the beginning it underwent a process of stabilization that was combined with a process of 'black boxing'.[39] This means that the experimental system at work not only produced new knowledge, but at the same time even erased the complex process by which it came into existence.[40] Nowadays we have the means to gain a deeper understanding of these processes that ultimately gave rise to prehistoric archaeology as it is practiced today. To

38 Ernst 2004, 247–248. – Translation by the author. Originally: „Ausgräber befassen sich vor Ort eher mit materiellen den verbalen Kon/texten, im Team mit Technikern eher denn mit Textgelehrten. Das wissenschaftliche Arbeitsfeld der Prähistoriker kann nicht mit dem Vokabular der Historiker fixiert werden."

39 Cf. Latour 1999.

40 Cf. Stoff 2008, 50.

accomplish this aim it will be necessary to conduct more detailed analyses emphasizing the particularities of archaeological field practice and diverse techniques used by archaeologists in subsequent phases of archaeology's development. These studies will clearly be able to demonstrate the inadequacy of the famous term 'science of the spade', to which many archaeologists still proudly refer today.

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Illustration credits

1 Bittel and Rieth 1951, book cover. **2** Bittel and Rieth 1951, Plan 1. **3** Fischer, Kimmig, and Planck 2002, 17. **4** Kimmig 1968, Abb. 25. **5** Kimmig 1990, Abb. 5. Courtesy Römisch-Germanisches Zentralmuseum Mainz. **6** Biel 1987, Abb. 42. Courtesy Landesamt für Denkmalpflege im Regierungspräsidium Stuttgart.

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The Colonial Archaeological Hero Reconsidered. Post-Colonial Perspectives on the 'Discovery' of the Prehistoric Past of Indonesia

Summary

Taking as its starting point a collection of (auto)biographical narratives on the academic careers of Dutch prehistorians Van Stein Callenfels, Van Heekeren and Van der Hoop, this paper discusses the phenomenon of the 'colonial archaeologist as hero' from both a historical and a theoretical (post-colonial) perspective. We thus reconsider those colonial archaeologists who, according to traditional histories of archaeology, 'discovered' the prehistoric past of Indonesia during the 1920s and 1930s. We do this in order to gain a better understanding of the colonial dimension of research into the prehistory of the Dutch East Indies and the way it continues to affect the archaeology of post-colonial Indonesia. We focus on the dynamic social and cultural contexts within which the archaeological research was developed and conclude that the creation of knowledge of the prehistoric past included various forms of indigenous involvement.

Keywords: Post-colonialism; history of archaeology; prehistory; Dutch East Indies; Indonesia; biographies; legacies of colonialism.

Dieser Aufsatz behandelt das Phänomen des ‚kolonialen Archäologen als Helden' aus historischer und postkolonialer Perspektive. Im Zentrum stehen (auto-)biographische Erzählungen über die akademischen Laufbahnen der niederländischen Prähistoriker Van Stein Callenfels, Van Heekeren und Van der Hoop. Wir erörtern die Rolle der drei kolonialen Archäologen, die nach herkömmlicher Geschichtsschreibung in den 1920er und 1930er Jahren die prähistorische Vergangenheit Indonesiens ‚entdeckten'. Unser Ziel ist ein besseres Verständnis der kolonialen Dimension der Vorgeschichtsforschung in Niederländisch-Ostindien und deren Auswirkungen auf die Archäologie Indonesiens in postkolonialer Zeit. Wir fokussieren auf die kulturellen und sich dynamisch verändernden Kontexte und Praktiken, in welchen die archäologische Forschung stattfand und zeigen, dass bei der Herstellung archäologischen Wissens verschiedene Formen indigener Beteiligung wichtig waren.

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Keywords: Post-Kolonialismus; Geschichte der Archäologie; Prähistorische Archäologie; Niederländisch-Ostindien; Indonesien; Biografien; Erbe des Kolonialismus.

In this article we reconsider the apparently fixed image of 'the colonial archaeologist' from a historical and a theoretical (post-colonial) perspective. We focus on the (auto)biographical narratives of a group of colonial archaeologists who worked in the Dutch East Indies and carried out research into the so-called 'prehistoric' past in the first half of the twentieth century. Thereby, we examine the way these narratives functioned as authoritative prescriptions for understanding and dealing with Indonesian prehistory. By analysing the transformations of the status and significance of these colonial archaeologists and their academic work across regime changes in colonial and post-colonial Indonesia, we hope to provide balanced insights into the cultural and socio-political ramifications and the continued effects of colonial-era prehistoric research.

Our focus on 'colonial archaeologists' is motivated by the realization that, in traditional histories of colonial archaeology, in which new discoveries are the main impetus for a progressive understanding of the early past, the personality of the archaeologist is often regarded as pivotal. We are told time and time again that the colonial archaeologist obtained his leading position in the research field solely on the basis of his talents and an all-absorbing vocation, whereas other relevant factors are obscured.[1] As a rule, this colonial archaeologist is a male adventurer who travels to exotic places to look for traces of the material culture of the past. Women do not generally play a significant role in these quests.[2] Often a loyal indigenous guide comes to his aid, and eventually the archaeologist becomes acquainted with other local people as well. Despite these contacts, the life-stories of colonial archaeologists generally confirm the colonial 'gaze' on the prehistoric past of colonial societies as lost worlds waiting to be discovered by westerners. The colonial archaeologist might be an eccentric, but he ultimately turns from social outsider to public hero, thanks to his discoveries.[3] To the present day, the image of the colonial archaeologist remains fixed and strong and apparently unaffected by decolonization;[4] in contemporary popular culture, Indiana Jones represents him par

1 Cf. Abir-Am 1982, 285.

2 For the contemporary gendered dimensions of archaeological fieldwork, see: Moser 2007. Cf. McClintock 1995, 1–17.

3 For a classic example of this perspective, see Ceram 1949.

4 See, for example, the exhibition *Das Grosse Spiel. Archäologie und Politik* (The Great Game. Archaeology and Politics) of 2010 in the *Ruhr Museum Essen*. Although the exhibition recognizes the connections between archaeology and imperial politics around the turn of the nineteenth and twentieth centuries, it merely reproduces archaeological hero stories and the exotic fascination with 'the other'. One of the main questions of this exhibition is: "Who were the pioneers that discovered and excavated the archaeological sites and monuments?" ("Wer waren die Pioniere, die archäologische Stätten und Monumente entdeckten und untersuchten?"). As a result,

excellence. Referring to this phenomenon, Neil A. Silberman speaks of the fable of the archaeologist as hero and of a basic pattern of adventure that is endlessly repeated in the histories of archaeological discoveries.[5] As a result, the heroic narrative pattern has deeply influenced the public understanding of archaeological work in general.[6]

In the Dutch East Indies of the 1920s and 1930s, as elsewhere in Asia, prehistory and the researcher that 'discovered' it had begun to fascinate diverse audiences. The collecting of stone tools was a booming activity, practiced by professionals and amateurs alike. For those researchers involved, the notion of human evolution did not pose the problem it had for many in the nineteenth century in the context of a creationist worldview. As a result, newly acquired knowledge of the prehistoric past was considered highly modern. The fact that the information gathered in this new field of knowledge was ordered as a story of linear (although in Asia sometimes impeded) advancement, only added to its modern status. In the Dutch East Indies, prehistorians concluded that, during Palaeolithic, Mesolithic and Neolithic times, there had been many connections with and migrations between different parts of Asia; famous excavations included those along the north-east coast of Sumatra (where shell mounds or kitchen middens were excavated) and those at the Guwa Lawa Bat Cave near Madiun.[7] Researchers also believed that, in some parts of the colony, the prehistoric past still continued into the present, a view they gathered from the continuing use of stone axes and the absence of a tradition of writing. Consequently, archaeological knowledge was essentially connected with colonial hierarchies of a social and cultural nature.

At the same time, however, 'archaeological' sites were the objects of many other parallel processes of appropriation. At a local level, people living nearby, for example engaged, or re-engaged with such sites; but it was only from the 1900s onwards that state-supported institutions intervened more seriously, situating sites in the national domain.[8] In addition, in the context of the dissemination of knowledge about these sites, a generation of young nationalists from Java and Sumatra also became fascinated by images of the old Hindu and Buddhist empires that had ruled the archipelago ages ago and that now were encountered in the maps, research-proceedings and conservation projects of colonial archaeology. These reconstructions of the great 'Indonesian' empires

indigenous local or national perspectives on, and appropriations of archaeological sites and objects are hardly touched upon. See brochure *Das Grosse Spiel*, Ruhr Museum Essen 12-2/13-6 2010; cf. Trümpler 2008a, 16; Trümpler 2008b, 105–113; Bernbeck 2011.

5 Silberman thus referred to nineteenth-century European archaeologists such as Giovanni B. Belzoni, Austen H. Layard and Heinrich Schliemann, who published autobiographical or travel accounts as part of their excavation reports, cf. Belzoni 1820; Layard 1849; Schliemann 1881.

6 Silberman 1996, 251–252.

7 Bernet Kempers 1982, 19–22; Tanudirjo 1995, 68–70; von Heine-Geldern 1945; Soejono 1969.

8 For local perspectives on archaeological sites in the nineteenth century, see: Bloembergen and Eickhoff 2013a.

of the past fuelled their dream of a great national Indonesian future.[9] Likewise, the excavations of a prehistoric past stimulated a national self-awareness; after decolonization, prehistory would, as a result, become an integral part of Indonesia's national past.[10]

In order to get beyond the colonial gaze – for example, the exclusive focus on the heroic colonial archaeologist and his discoveries – post-colonial historians have, since the 1970s, been developing concepts and strategies to identify, criticize and deconstruct the so-called colonial discourse.[11] They understand this discourse as a system of statements within which the supposed centrality and modernity of Europe is related to the supposed inferiority of the colonized races and societies. As a result, the imperial power perceives itself as having a duty to advance the civilization of the colony through force, trade, administration, and cultural and moral improvement.[12] Post-colonial theory explicitly invokes discussions on topics such as suppression, resistance, representation, difference, gender, place and the sacred, as well as responses to European imperial master discourses, like history, philosophy and linguistics.[13] What interests post-colonial historians and social scientists is the relation between knowledge and power and the development of cultural representations of colonial society that made colonialism self-evident – to both the colonizers and the colonized. Relevant questions in this context are: to what extent, why and how were these images internalized? To what extent, how and why did these images persist after political decolonization? And how can we recover 'agency' and 'history' for the subjected 'others' in colonial regimes? Pointing to the relationship between imperialism and history writing, Gyan Prakash raises the question of

> how the history of colonialism and colonialism's disciplining of history can be shaken loose from the domination of categories and ideas it produced.[14]

Acknowledging the importance of academic self-reflection, Dipesh Chakrabarti, in his famous essay "Provincializing Europe",[15] presented a number of suggestions for achiev-

9 Reid 1979. Cf. Bloembergen and Eickhoff 2011.

10 See the work of the Indonesian Minister of Education, Muhammad Yamin, who argued, with classical archaeological and prehistoric findings (rock paintings in South Sumatra) as proof, that the white-red colors of the Indonesian national flag were already key to a unified Indonesian people in prehistoric times (Yamin 1953).

11 Bruce Trigger is an early example of an archaeologist who tried to define the phenomenon of colonial archaeology. He did this in 1984 by stressing the relationship between the nature of archaeological research and the social milieu in which it is practiced. Colonial archaeology, according to Trigger, served, wherever practiced, primarily to denigrate native societies and peoples by trying to demonstrate that they lacked the initiative to develop on their own. The assumption that the culture of these subjected 'others' had been static since prehistoric times justified the European colonial project, cf. Trigger 1984; cf. Trigger 1989, 110–147. For a recent overview of the history of post-colonial studies, see: Roque and Wagner 2011, 6–13. For archaeologists inspired by post-colonial studies, see: Lydon and Rizvi 2010. Cf. González-Ruibal 2010 and Gosden 2012.

12 Ashcroft, Griffiths, and Tiffin 2007, 36–38.

13 Ashcroft, Griffiths, and Tiffin 2006.

14 Prakash 1995, 4–5.

15 Chakrabarty 2008.

ing this goal. He argues that the categories of European thought (including the concept of historicizing) are simultaneously both indispensable and inadequate when writing about the non-European world. He therefore stresses the importance of questioning the structure of a chosen narrative and of making it heterogeneous by including multiple perspectives, ambivalences and contradictions, and by "translating across cultural and other semiotic systems".[16] Other scholars advocate a more complex understanding of the colonial past as a global phenomenon.[17] Cooper, for example, emphasizes that empires entailed diverse networks and power structures that established circuits along which personnel, commodities and ideas moved and that, as a consequence, created multiple hierarchies in both the metropolis and in colonial society. Simultaneously, so he stresses, circuits were at work that escaped the control of the colonial state. Empires reproduced difference, but dealt with structures from within that "complicate the relationship of ruler and ruled, of inside and outsider," as well.[18]

Inspired by Chakrabarti and Cooper, in this paper we consider colonial archaeological activities, not only as part of a colonial regime of truth and power, but also as an aspect of a dynamic field of diverse involvements, exchanges and interferences – including indigenous ones.[19] In our project on the history of archaeology and heritage formation in colonial and post-colonial Indonesia, we decided to make so-called archaeological sites our central focus and question what kind of encounters and interventions took place there and under what constraints.[20] What position does the "colonial archaeologist" have in this constellation? What role do authority, force and violence play? What kind of dissemination and appropriation of site-related objects, documentation and images can we trace, from a local to a global level? This approach helps us to get beyond an exclusive focus on colonial discourses and, following Lynn Meskell, to recognize that archaeology essentially deals with the diverse ways in which meanings and identities are attributed and negotiated.[21] It furthermore enables us, following Margarita Díaz-Andreu, to focus on players other than the colonial archaeologists, who, according to Díaz-Andreu, "by getting involved, (…) challenged the rules of the game".[22]

Thus, when dealing with the prehistoric archaeology of colonial and post-colonial Indonesia, our site-based approach enables us to avoid an exclusive fixation on the relationship between archaeology and (colonial) state formation[23], and the related phenomenon described by Susan Legêne and Henk Schulte Nordholdt as 'colonial determinism'. They stress – with good reason, as we will show – that archaeological sites in

16 Chakrabarty 2008, 17, 43 and 45–46.
17 Roque and Wagner 2011, 5. Cf. Raben 2013.
18 Cooper 2005, 48–53.
19 Cf. Roque and Wagner 2011, 17–23; Stoler 2011, 35–66; Stoler 2009.
20 For this project see: http://ghhpw.com/sbs.php (visited on 07/07/2015).
21 Meskell 1998.
22 Díaz-Andreu 2007, 6–10, 239–244 and 402–409.
23 For a historical analysis of the phenomenon of colonial archaeology, with a strong focus on its relationship with the state cf. Anderson 1991, 155–185; Cohn 1996, 76–105.

contemporary Indonesia are not necessarily defined by, nor today necessarily a representation of, colonialism; this is the case, even if colonial relations have played a crucial role in transforming them into archaeological sites.[24]

In order to understand when, how and to what extent Dutch pre-historians working in the colony transformed into archaeological heroes, and for whom, we will follow a three-level enquiry in this article. First, we will focus on three publications of a more or less biographical or autobiographical character, each narrating the life story of one of three selected prehistorians: Pieter Vincent van Stein Callenfels, Hendrik Robert van Heekeren and Abraham Nicolaas Jan Thomassen à Thuessink van der Hoop. Taken together, these narratives offer a first introduction to the diverse prehistoric activities that were carried out in the first half of the twentieth century and the way they were perceived by a wider, Dutch-speaking audience. These publications concern the popular biography *Iwan de Verschikkelijke. Leven en werken van Dr. P.V. van Stein Callenfels* (Ivan the Terrible. Life and Work of Dr. P.V. van Stein Callenfels),[25] the autobiographical travelogue *De onderste steen boven. Belevenissen van een globetrotter* (Uncovering the Truth. The Adventures of a Globetrotter), written by van Heekeren,[26] and the obituary of A.N.J.Th.à Th. van der Hoop, written by the Dutch archaeologist (and former director of the Colonial Archaeological Service) August Bernet Kempers, and published in 1969 in a Dutch academic journal.[27]

How did these hero stories represent the discovery of the prehistoric past of the Dutch East Indies and how did they transform this into a story in which Dutchmen took the lead, while women and explorers of other nationalities were marginalized? Secondly, via an alternative focus on the encounters and practices in which colonial archaeology was embedded at both excavation and museum sites, we will explore how the production of knowledge of the prehistoric past entailed various forms of indigenous involvement. How do the three narratives of the selected life stories relate to these alternative involvements? After all, the biographies were published in Dutch, and in the Netherlands, and were therefore difficult to access for most Indonesians. Thirdly, we focus on the effect of decolonization on the image of archaeologists and the representations of the prehistoric past in Indonesia. Our interpretations are based on observations during research trips to Indonesia in 2010 and 2011, and on discussions we had with Indonesian colleagues. To what extent do the traditional hero narratives shape the Indonesian understanding of the prehistoric past, or prehistoric studies as practiced nowadays in important academic and heritage institutions in Indonesia?

24 Legêne and Schulte Nordholt 2015, 8.
25 Swanenburg 1951.
26 Van Heekeren 1969.
27 Bernet Kempers 1969.

1 Three 'heroes' of prehistoric archaeology in the Dutch East Indies

In the biographical narratives of the three 'heroes' of prehistoric archaeology which we discuss in this section, the basic pattern of adventure is undeniably present. The stories are closely intertwined and although the selected prehistorians, of course, had very different personalities, their stories are, to a large extent, comparable, as our protagonists all worked in the same institutions and dealt with the same concepts of the past.

The author of the popular biography of Van Stein Callenfels, B.D. Swanenburg, was a close friend of his central character.[28] Swanenburg aims to describe the life of the archaeologist, who was born in Maastricht in 1883 and died in Colombo (Ceylon/Sri Lanka) in 1938. The book is essentially a collection of anecdotes, but at the same time it contains precise descriptions of some of Van Stein Callenfels' excavations. We read how Van Stein Callenfels (Fig. 1) went to the Dutch East Indies in 1904 and embarked upon a career as a civil servant at the center of the Dutch colonial administration, in Buitenzorg (Bogor). This career choice was, however, not a great success, and for a few years he went 'native', in the sense that he lived in a Javanese village without a regular income. It was, according to his biographer, during this time that Van Stein Callenfels became familiar with the culture of the Javanese people. Following this episode, he was employed on a coffee plantation and was able to initiate his private research into antiquities. Soon afterwards, Van Stein Callenfels, by now a self-made archaeologist, was transformed into a public figure who, notwithstanding his eccentricity and unconventionality, was supported by the colonial government. In 1915, he became an employee of the Archaeological Service *(Oudheidkundige Dienst)* of the Dutch East Indies (established in 1913) and started conducting research into the so-called Hindu-Javanese past. Van Stein Callenfels went back to the Netherlands and wrote his doctoral thesis in Leiden in the years 1921 to 1924.[29] After this, he returned to the Dutch East Indies, where he focused more and more exclusively on prehistory. He became an internationally recognized specialist, who played an active role in organizing the dialogue with prehistorians working in other parts of Asia. It was on his initiative that the Congress of Prehistorians of the Far East, founded in 1932, began to meet every three years.[30] By the time of his death, he was a renowned, even legendary, archaeologist. His gigantic stature and unorthodox behaviour might have turned him into the colonial cult figure nicknamed "Ivan the Terrible"; nonetheless, he did possess diplomatic skills. He took part in international academic networks and was often asked by the colonial authorities to show official

28 The author introduces himself as a friend halfway through the book, cf. Swanenburg 1951, 110–111.

29 Van Stein Callenfels 1925.

30 Hanoi 1932, Manila 1935, Singapore 1938. – To initiate this congress was decided for in 1929 in Bandung, during the Fourth Pacific Science Congress, where, owing to the persistence of Van Stein Callenfels, a meeting of prehistorians was organized; cf. Soejono 1969, 75; von Heine-Geldern 1945, 157.

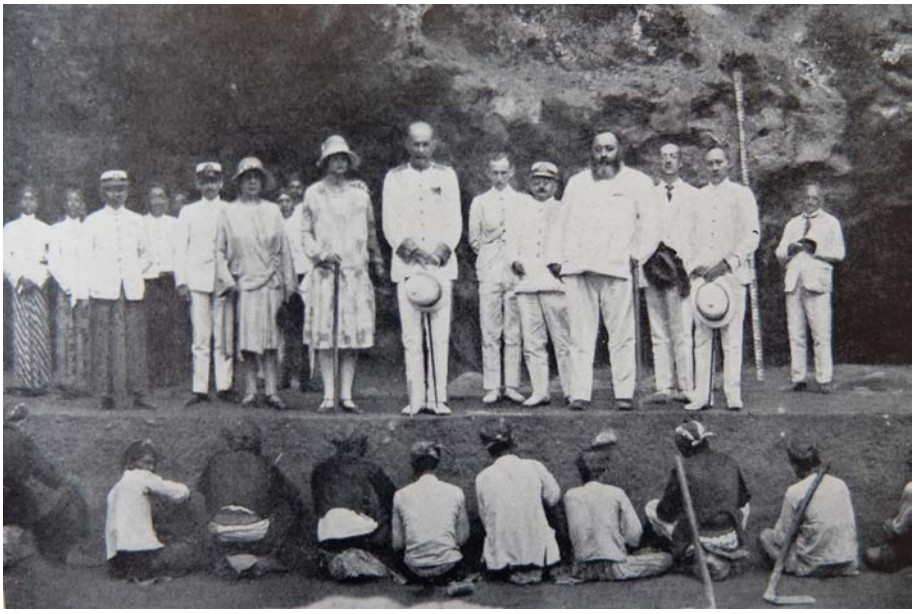

Fig. 1 Colonial archaeologist Van Stein Callenfels (with beard and white tropical suit) with workmen and visitors at the Guwa Lawa excavation.

guests round on archaeological sites.[31] Although the book was published in 1951, it does neither mention the Indonesian revolution nor the Indonesian war of decolonization against the Dutch Empire. As such, it is a pure and, in a certain way, nostalgic celebration of the heyday of colonial archaeological activity in the Dutch East Indies.

"Uncovering the truth. The adventures of a globetrotter"[32] (Fig. 2) is an archaeological autobiography, written in 1969 by Hendrik Robert van Heekeren, who was born in Semarang in the Dutch East Indies in 1902 and died in 1974. In this book he describes how he – a tobacco planter and (volcanic) mountaineer – started conducting research into the prehistoric past during the 1930s.[33] The autobiography has a decidedly different character from the biography of Van Stein Callenfels, as it has a different timescope. Van Heekeren discusses his pre-wartime archaeological activities in the Dutch East Indies and his private archaeological 'excavations' during the Japanese occupation and the Pacific War, when, as a forced labourer in the construction of the Burma railroad in 1943, he secretly collected prehistoric flints; from there he moves on to his archaeological work

31 Jaquet 1989; cf. http://www.historici.nl/Onderzoek/Projecten/BWN/lemmata/bwn3/steincallenfels (visited on 07/07/2015).

32 *De onderste steen boven. Belevenissen van een globetrotter*, Van Heekeren 1969.

33 Van Heekeren had published his first book about his 'tropical' travels and volcano climbing activities in the Dutch East Indies under the pseudonym Hybride, see Hybride 1940.

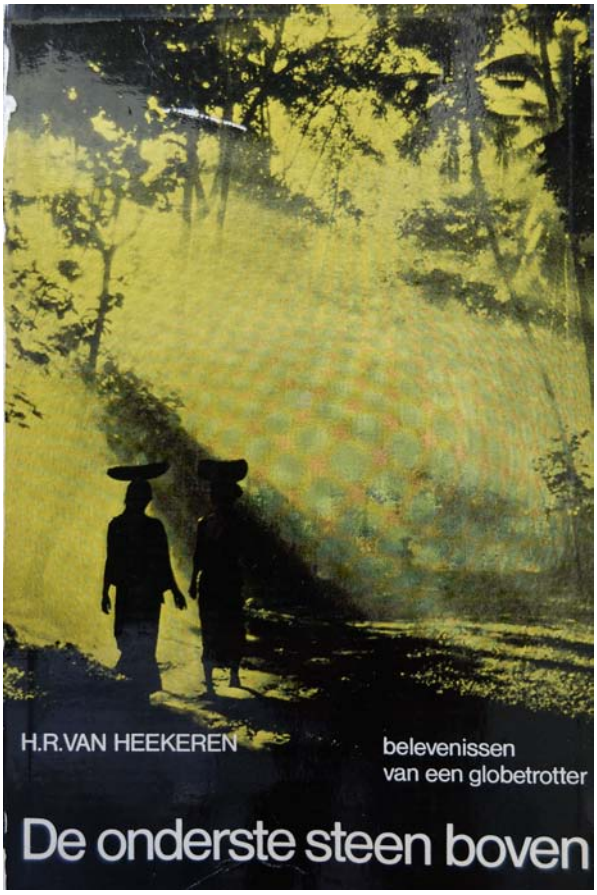

Fig. 2 Cover of the autobiographical travelogue of Van Heekeren.

in Indonesia after independence. From 1946 to 1956, Van Heekeren worked as an official prehistorian for two successive Archaeological Services – the Dutch colonial Service (from 1946 to 1949) and the Indonesian Archaeological Service (the *Djawatan Purbakala*, later *Dinas Purbakala*), which was established in 1946. During the chaotic years of the Indonesian Revolution which followed the unilateral proclamation of the Indonesian Republic by its president Soekarno and its vice-president Mohammed Hatta on 17 August 1945 and which lasted until 1949, two archaeological services operated simultaneously; one in Batavia (today's Jakarta), headed by the Dutch, and one in Yogyakarta, headed by the Indonesians. In this period, Van Heekeren was based in Batavia and worked as a curator at the Museum of the Batavian Society of Arts and Sciences (today's *Museum Nasional*). He returned to the Netherlands in 1956 and subsequently conducted prehistoric research around the world (in Tanzania, the Netherlands Antilles and Thailand, among other places).

The author of the obituary of A.N.J.Th. à Th. van der Hoop, the archaeologist August Bernet Kempers, had, as librarian of the Batavian Society, been a close colleague of Van der Hoop in Batavia. The obituary appeared in 1969 in the *Bijdragen tot de Taal-, Land- en Volkenkunde*, a scholarly journal of another learned society based in the Netherlands, which, after decolonization, continued its research, focusing on the linguistics, anthropology, and history of Southeast Asia, and more specifically of Indonesia.[34] Van der Hoop, who was born in Arnhem in 1893 and died in The Hague in 1969, initially found fame in the Netherlands as an 'hero of aviation'. In 1924 he was a member of the first crew to fly from Schiphol/Amsterdam to Batavia.[35] Van der Hoop and his crew received a hero's welcome on their return to the Netherlands. He was honoured in many ways: he was depicted on a Dutch 75 cent air stamp commemorating this flight; a memorial stone was unveiled in the city of Medan (Sumatra). In 1931, Van der Hoop went back to the Dutch East Indies to collect archaeological information on the stone culture of the Pasemah region in Sumatra for his PhD thesis at Utrecht University.[36] In 1934, as Dr Van der Hoop, he started working as curator for the Museum in Batavia. He became active in many scholarly fields (prehistory, ethnology, Hindu Javanese history, colonial history) and – being a civil servant – also got involved with modernizing the museums in the Dutch East Indies. When the Japanese occupied the Dutch East Indies in March 1942, Van der Hoop was initially allowed to remain in office at the museum, whereas most of the other Dutch colonials were interned. During this period he collaborated with his Japanese superiors, such as C. Koda of the Japanese military administration, who was in charge of the direction of the museum and who generally considered the museum and its collection to be important; eventually, in April 1943, Van der Hoop was himself interned. He was released in 1945 and repatriated to the Netherlands in 1946. In 1947, he returned to Indonesia in order to work for the Archaeological Service based in Batavia. In 1950, political developments made him decide to return to the Netherlands for good. By that time, he was 57 years old. His life as an archaeologist was finished, but in the Netherlands he remained active in several cultural fields. For instance, he got involved with the dissemination of knowledge about the Dutch colonial culture of the early modern period.[37]

The first thing that is apparent when analyzing these narratives about the three prehistorians is that it was self-evident that the prehistoric past was there; it only had to be discovered. Wherever the archaeologist traveled in the Indonesian archipelago, the early

34 Bernet Kempers 1969. Bernet Kempers, a specialist in Hindu-Javanese archaeology, was a close colleague of Van der Hoop in Batavia for many years. In the obituary he is, therefore, able to describe Van der Hoops' academic career in conjunction with more private stories and personal impressions. For Bernet Kempers cf. Soekmono 1994.

35 Van der Hoop 1925.

36 Van der Hoop 1932. – He received his PhD in 1932.

37 For Van der Hoop and the "Stichting Cultuurgeschiedenis Nederlanders Overzee" (Foundation for Cultural History of Dutchmen Overseas), cf. Lunsingh Scheurleer 1987.

past always seemed to have left its traces. With regards to Van Stein Callenfels and his early (primarily Hindu-Javanese) archaeological research on Java's past, we read about the rhythm of travel and the related archaeological activities:

> And so he travelled […] time and again […], in order to inspect, to excavate, to decipher, to discover […], to replace […], to restore […], to retrieve, and to do everything he considered important for Java's archaeological treasures, to which he gave his heart.[38]

When reading Van der Hoop's dissertation, it becomes clear that for him it was "the road" that organized his work. It not only functioned as a grid within which to arrange his observations, but also as a model to offer his readers a way to verify these observations. In his first chapter, called "The Palembang-Pageralam road" he writes:

> In describing the megalithic remains in South Sumatra, we will take the order of the sequence in which a traveller, traversing the country by the main roads, would meet with such remains.[39]

For Van Heekeren, travel, adventure and archaeological research were closely connected.[40] In his later years, he speaks of a certain restlessness that made him travel. At the same time, he considers travelling to be the inescapable fate of the prehistorian.[41] The modern age allowed him to travel by airplane and motorboat, but he also used canoes and rafts, or just walked into the jungle.[42] He also recollects that, when he was appointed to the Archaeological Service in 1946, a colleague from London, Prof. Frederick Zeuner, wrote to him: "You are working in an almost completely unexplored area and may well make great discoveries."[43] It is this self-image and self-fashioning as adventurous male explorers that eventually enabled colonial archaeologists to become public heroes. However, at the same time, as competitive colleagues, they sometimes critically evaluated each other in this respect. Van Heekeren, for example, was annoyed by the way Van Stein Callenfels made a grand entrance to the harbor of Makassar, like a celebrity.[44] On the other hand, the biography of Van Stein Callenfels also stresses that he was a lonely

38 Swanenburg 1951, 56. – Translation by the authors. Originally: "En zo reisde Callenfels […], keer op keer […] om te controleren, te inspecteren, te ontgraven, te ontcijferen, te ontdekken, om […] te herplaatsen, om […] te herstellen, om […] te achterhalen, en om verder alles te doen, wat hij in het belang achtte van Java's archeologische schatten waaraan hij zijn hart verpand had."

39 Van der Hoop 1932.

40 In his autobiography he mentions the adventure books of Karl May and Gustave Aimard he read when he was a child (Van Heekeren 1969, 2). Van Stein Callenfels' favorite adventure books were: "The three musketeers" and "The count of Monte Cristo", both by Alexandre Dumas. Cf. Swanenburg 1951, 6.

41 Van Heekeren 1969, 143.

42 Van Heekeren 1969, 176–177. It is with great pride that he quotes someone describing him as a man with an "indomitable scientific spirit" (Van Heekeren 1969, 145).

43 Van Heekeren 1969, 106.

44 Soejono 1975, 108.

person. His death from a heart attack in a hotel room in Colombo resembles the end of a tragic hero.[45] The image of the archaeological hero was also echoed in later representations, in literature, as well as film. According to Swanenburg, Van Stein Callenfels in all probability inspired Sir Arthur Conan Doyle when he was creating the figure of the aggressive, dominating "Professor Challenger" in his novel *The Lost World*, which was published in 1922, given the fact that the two of them met in 1913 at the *Galle Face Hotel* in Singapore.[46] Van Heekeren's account of finding prehistoric axes while performing forced labour on the Burma railroad and the fact that a Japanese guard forced him to throw them away made it into the script of the film *The Bridge on the River Kwai*. Yet, these scenes were ultimately cut from the movie.[47]

For Van Heekeren, who, after decolonisation, continued to carry out research into Indonesian prehistory, often at the invitation of, and in collaboration with, Indonesian colleagues, the ideal of 'post-colonial friendship', especially between a *guru* (teacher) and his former students, was an important aspect of being an archaeological hero. In his travelogue, he recounts that young Indonesian students see him as such a hero; but, stressing his own unpretentiousness, he adds: "in the Far East, it is easy to become a legendary figure."[48] For him, this friendship was, de facto, an essential condition for continuing his research in Indonesia. This notion of friendship thus obscured the complex interdependencies and role-reversals that were at work in the exchange between this now former colonial archaeologist and his now post-colonial Indonesian archaeological colleagues. During his return travels to Indonesia, Van Heekeren felt welcome, but he was also aware of his new subordinate position, as he warned against colonial nostalgia. With regard to the colonial period, he concluded that, although some Indonesian people refer to it as "normal" times, nobody really wants these times to return.[49] However, at one point in his autobiography, he himself cannot suppress his nostalgia when speaking of the old generation of colonial archaeologists. He mentions Dubois, Von Koenigswald, Oppenoorth, Van Stein Callenfels and Van der Hoop in this respect as "Dutchmen" who made their mark, even though their research tradition *in the Netherlands* had come to an end.[50] This list is a clear example of the way that the 'discovery' of Indonesian prehistory was turned into a story in which Dutchmen took the lead, whereas the role of Indonesians, women and explorers of other nationalities was obscured.[51]

45 Swanenburg 1951, 177 and 266.
46 Swanenburg 1951, 46.
47 The role was played by the anthropologist Karl Heider, cf. Soejono 1975, 109. For the discovery and history of the stones cf. Van Heekeren 1969, 51–57, 67, 145 and 165.
48 Van Heekeren 1969, 181.
49 Van Heekeren 1969, 181.
50 Van Heekeren 1969, 177. Cf. Bernet Kempers 1982.
51 For some examples of this gendered national marginalization, see the work of Lenore Selenka, who organized an expedition to Trinil in 1907–1908, the eccentric cousins Paul and Fritz Sarasin from Switzerland who 'discovered' the so-called Taolian culture of Southwest Sulawesi at the beginning of the twentieth century or the explorer W. Rothpletz, who, due to his 'neutral' Swiss nationality, was able to work during the Japanese occupation. See: Selenka and Blanckenhorn 1911; P.

Van der Hoop, who started his career as an aviation hero, did not manage to attain archaeological hero status in the same way as Van Stein Callenfels or Van Heekeren. His museological and bureaucratic activities in the mid- and late 1930s may not have left much room for achieving this status. The fact that he could continue his work in the museum of the Batavian Society under the Japanese occupation – thus allowing the Japanese to show their commitment to the Asian dimension of the collection – might, in the eyes of Dutch colonials, have made him a collaborator. It seems however, that they used this term exclusively to denounce Indonesian nationalists like Soekarno, who saw the Japanese occupation as a first step towards the national liberation of Indonesia. Furthermore, Bernet Kempers describes Van der Hoop in his obituary as a cynical person who kept people at a distance, while he glosses over his homosexuality as his "being different".[52] Being an unmarried gay man must have made Van der Hoop's social position vulnerable, especially in the late 1930s, when, in the Dutch East Indies, homosexuals, including those in higher social echelons, were prosecuted.[53] This vulnerability is also highlighted in the anecdote in which it is related that Van der Hoop no longer felt at home in the Indonesia of the 1950s, as he was forced to live in the garage of his former villa.[54] His cynical worldview with regard to decolonization had already risen to the surface when the aviation monument in Medan commemorating his flight to Batavia in 1924 was restored and inaugurated for a second time in the late 1940s, having been destroyed during the Japanese occupation. Van der Hoop supposedly remarked that he was probably the first person ever to witness the inauguration of the same monument to himself twice.[55]

2 'Reconsidering' the prehistorians of the Dutch East Indies

Without the specific cultural and socio-political contexts of the colonial society in which they operated, Van Stein Callenfels, Van Heekeren and Van der Hoop would never have been able to make their archaeological discoveries and would never have become archaeological heroes. In order to develop a balanced understanding of these contexts, beyond an exclusive focus on the colonial discourse, we concentrate in this section on the encounters and interventions that took place at the archaeological sites which our protagonists selected to carry out their research. Starting with the (auto)biographical narratives and using additional archival material, we aim to analyze the interaction in these encounters between, on the one hand, the colonial/academic regime of truth and power and, on the other hand, the words, visions and agency of indigenous people.

Sarasin and F. Sarasin 1905; Rothpletz 1951, 77–126.

52 Bernet Kempers 1969, 402.

53 Bloembergen 2011.

54 Bernet Kempers 1969, 424.

55 Bernet Kempers 1969, 406.

Reconstructing the involvement of indigenous people from the evidence in the selected texts is not an easy task. In the obituary of Van der Hoop, Bernet Kempers does not mention Indonesian participants as having any role. The narratives on Van Stein Callenfels and Van Heekeren are different in this respect. A conservative colonial worldview is manifested throughout *Ivan the Terrible*. Van Stein Callenfels is repeatedly quoted as saying that there are "toeans" (masters) and "koelies" (coolies), thus confirming the traditional hierarchy of colonial society.[56] One of many anecdotes in the book corresponds directly with this notion of colonial hierarchy: when Van Stein Callenfels was being transported by sedan chair, from his position above them, he loudly compared his carriers to wheels.[57] The biographer also stresses that Van Stein Callenfels strongly disliked the ethical polities of the first decades of the twentieth century (the Dutch version of the civilizing mission of the European colonial powers).[58] He criticized the modern colonial style of the 1920s and 1930s, which he simply described as playing tennis and going to swimming pools. Van Stein Callenfels himself was a hard worker and famous for drinking many bottles of beer, smoking heavily and talking loudly when attending parties at the colonial club. It was this kind of behaviour in public that resulted in his nickname "Ivan the Terrible". But this attitude did not make him any less popular among colonials in the Dutch-Indies: on the contrary, Van Stein Callenfels became what might be described as a colonial cult figure. Paradoxically, his anti-modern lifestyle, at the same time, created less distance between himself and the Javanese.[59] Apparently, at least according to Swanenburg, he was a popular figure among the Javanese people, who, given his weight of 150 kilos and his height of 1.92 meters, perceived him as being an incarnation of the wayang figure of Koembakarna (a giant with a massive appetite and a good character) from the *Ramayana* epic.[60]

As mentioned earlier in this text, in his autobiography Van Heekeren stresses his friendship and contacts with his Indonesian colleagues who worked at the *Archaeological Service* in the 1950s. At that time, a few of them were being educated with the ultimate aim of taking over this service from the Dutch.[61] The description of his first return trip to Indonesia in 1968 tells us how complex these relations had become. Although his main aim was to prepare the new edition of his *The Stone Age of Indonesia* (first published in 1957[62]), he was also interested in the development of post-colonial Indonesian society. He witnessed, for example, the effects of the anti-communist violence of 1965 and embarrassed his hosts by asking questions about these events.[63] Van Heekeren, however, does not reflect on the colonial nature of his work in the colonial past. It is quite clear

56 Swanenburg 1951, 83 and 250.
57 Swanenburg 1951, 150.
58 Swanenburg 1951, 83. For Ethical Politics, see: Locher-Scholten 1981; Bloembergen and Raben 2009.
59 Compare with Drieënhuizen 2012, 316.
60 Swanenburg 1951, 43.
61 Van Heekeren 1969, 178–179; cf. Bloembergen and Eickhoff 2011, 421 and 425–426.
62 Van Heekeren 1957.
63 Van Heekeren 1957, 185 and 192.

that Van Heekeren enjoyed a warm welcome from his former colleagues, like R.P. Soejono, one of the prominent first generation, post-independence prehistorians of Indonesia. But it is also obvious that feelings of friendship helped to hide the inconveniences caused by the new post-colonial role-reversals and interdependencies. Still, he saw his own return primarily as moral support for Indonesia. The colonial bias that had once made him self-evidently 'the teacher' and Indonesians people unable to cope on their own obviously still permeated Van Heekeren's worldview.

From the early nineteenth century onwards, archaeological sites in the Dutch East Indies were visited for various reasons by travellers, including indigenous people, from the colony and from abroad.[64] In addition to, in some cases, merely living close to the sites, indigenous people were also able to fulfil diverse roles or positions in the excavations which took place during the colonial period, serving for instance as foremen, workmen or informants. In Van Stein Callenfels' book we find, for example, some references to a foreman called Moenaf, who is praised for his devotion to his excavation work (Fig. 3).[65] Whereas the workmen remain unmentioned (although they do sometimes appear in illustrations), local people living close to the site do play a role in the book. We learn that they sometimes reburied archaeological findings because they feared "soesah" (problems).[66] They play an indirect role as well, as Van Stein Callenfels' wider knowledge of Javanese culture enabled him to recognize figures from the wayang stories in temple reliefs.[67] Both Van Stein Callenfels and Van Heekeren do, however, sometimes speak of the people as still living – or almost living – in the Stone Age.[68] Nevertheless, when Van Heekeren describes the culture of indigenous people he meets during his many travels, he does not perceive their culture as static. On the contrary, he focuses on the processes of modernization.[69]

When exploring what actually happened on location during an archaeological excavation, the archives of the Batavian Society offer some important research opportunities.[70] In the archive, we find additional information on "meneer Moenaf" (Mister

64 Bloembergen and Eickhoff 2013a; Bloembergen and Eickhoff 2013b.

65 Swanenburg 1951, 224 and 229.

66 Swanenburg 1951, 83.

67 Swanenburg 1951, 98–101.

68 Swanenburg 1951, 143 and 188; Van Heekeren 1969, 178–179.

69 Van Heekeren 1969, 99.

70 The whereabouts and state of the archives of the Dutch Colonial Archaeological Service, split up and made inaccessible after several movements and reorganizations of the Indonesian Archaeological Service in the 1950s, 1960s and early 1970s, are unclear. Some say they have disappeared. We are still searching, but we have so far been unable to trace them, partly because, what is known to be left of them (the glass negatives collection) is inaccessible. Interviews by Marieke Bloembergen with Ekowati Sundari (head of the Archaeological Department at the Museum Nasional, Jakarta 12-12-2012 and 5-12-2010); with Junus Arbi and Saifal Majahid (Department of Culture and Tourism – under which the Archaeological Service resorted until 2012 – Jakarta, 2-2-2011), and with the senior archaeologist Mundardjito (Emeritus Professor at the University of Indonesia, Jakarta, 25-6-2012).

Fig. 3 Foreman, probably 'Meneer' Moenaf, at work.

Moenaf). In a letter from 1935, Van der Hoop recommends him to a plantation owner who wanted to start excavation work after discovering prehistoric flints:

> I can send you Mister Moenaf of the Archaeological Service. He knows Van Stein Callenfels' research methods. Once you have told him where to excavate, he can work independently and we can be confident that everything will go fine.[71]

In 1932, Van Stein Callenfels even asked the Batavian Society not to fire Moenaf, although the economic crisis made severe budget cuts unavoidable.[72]

From correspondence like this, we are able to ascertain that colonial archaeology was, in fact, not a project executed by colonial scholars alone – it never had been. Of course, there were barriers: the letters of Anna Jacoba Resink-Wilkens, a collector of Javanese antiquities based in Yogyakarta, teach us how Indonesian workman were, against their will, excluded from archaeological knowledge by Van der Hoop, and how Van der

71 Van der Hoop to A. Dünnwald (Mojokerto), 10-1-1935. KBG DIR No. 1059, ANRI, Jakarta. – Translation by the authors. Originally: "U kunt de beschikking krijgen over den Heer Moenaf van den Oudheidkundigen Dienst, die geheel door Dr. V. St C. gevolgde methode van ontgraving op de hoogte is. Wanneer u hem aangeeft, waar er gegraven moet worden, kan hij verder zelfstandig werken en wij hebben de zekerheid dat het goed gaat."

72 Van Stein Callenfels to C.C.F.M, 28-6-1932. Le Roux, KBG DIR No 1051, ANRI Jakarta.

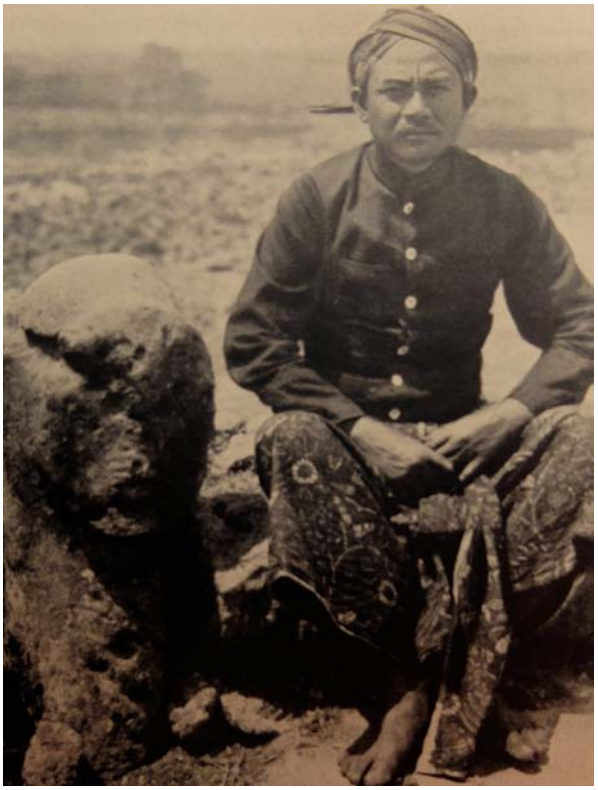

Fig. 4 Unknown Indonesian, probably a workman, at the Gunung Kidul excavation.

Hoop discounted local sources, in this case the Javanese court chronicles (*Babad*), kept in the library of the sultan's palace in Yogyakarta. The letters show us that Resink-Wilkens tried to transgress these same 'colonial' barriers. In 1934, she wrote Van der Hoop a slightly indignant letter:

> One of the coolies who worked for you at the Goenoeng Kidoel excavation visited me and asked me if I could explain to him and his fellow coolies what the excavation had been about.[73]

Interestingly enough, in his later publication, Van der Hoop did include 'local' perspectives on the stone box graves he had excavated. He mentions that the graves are still there because local people are afraid to re-use the stones. One family who did so was later stricken with physical and mental illness.[74]

73 Resink-Wilkens to Van der Hoop, 15-1-1934. KBG DIR No. 1058, ANRI, Jakarta. – Translation by the authors. Originally: "Ik deel u dit mede na een bezoek dat ik gehad heb van een van de koelies die U bij de opgraving in Goenoeng Kidoel geholpen heeft, en die mij kwam vragen of ik hem en zijn medehelpers kan inlichten wat de bedoeling was

A year later, in a letter to Van der Hoop, Resink referred to a conversation with Sultan Hamengkubuwono VIII from Yogyakarta regarding the same excavation. She reports that the sultan has developed his own interpretation of the excavation site, on the basis of one of the *babad* kept in the library of the palace, the *Babad Giyanti*. He relates the excavation to a historical battle that took place in 1756, in which the principalities of Surakarta and Yogyakarta and the Dutch East India Company (VOC) were involved, as described in this *babad*.[75] However, Van der Hoop is not convinced and, in his reply to the letter, he recounts that he has already discussed this topic with the sultan. He continues in a denigrating manner: "His Highness has the tendency to connect everything with 'saja poenja boekoe geschiedenis'" – 'I own history books'.[76]

These incidents clearly indicate that colonial archaeological knowledge was not solely confined to the ambit of a colonial regime of truth and power, as the coolies developed an interest in it during excavation works and later even tried to renegotiate their subordinate position as workers. Moreover, the knowledge was partly incorporated into – and maybe even contested by – the coexisting knowledge systems of the Javanese elite.

3 Colonial archaeological 'legacies' in contemporary Indonesia?

This section is dedicated to the question to what extent the traditional hero narratives shape the Indonesian understanding of the prehistoric past as it is practiced today in important academic and heritage institutions in Indonesia. We try to answer it on the basis of some observations we made during our research trips to Indonesia in 2010 and 2011 and by referring to the discussions we had during those trips with Indonesian colleagues. We will, furthermore, base our conclusions on a comparison of catalogues of the prehistoric collection – now kept in the *Museum Nasional* in Jakarta – dating from colonial and post-colonial times.

The *Museum Nasional*, which houses important and famous archaeological and ethnographical collections from Indonesia, is generally seen as an important tool for nation building.[77] In this museum, there is a special prehistoric section that was notably once the responsibility of Van Stein Callenfels, Van Heekeren and Van der Hoop,

van die ontgraving". For Resink-Wilkens, cf. Drieënhuizen 2012, 227–291 and 314–319.

74 For the excavation, cf. Van der Hoop 1935, 85 and 90.

75 Resink to Van der Hoop, 9-4-1935. KBG DIR No. 1060, ANRI, Jakarta.

76 Van der Hoop to Resink, KBG DIR No. 1060, ANRI, Jakarta. – Translation by the authors. Originally: "Z.H. heeft altijd de neiging de dingen in relatie te brengen met 'saja poenja boekoe geschiedenis'".

77 Anderson 1991, 178–185; McGregor 2004, 26.

although the collection itself, as such, began to be assembled in the mid-nineteenth century.[78] The museum is older still. It goes back to the foundation in 1778 of the Batavian Society of Arts and Sciences. In 1779, this society started to collect objects donated by the society's members, and to put them on public display. The history of the society is strongly connected to the history of the Dutch empire and colonial and post-colonial Indonesia. During the British Interregnum (1811–1814), the Lieutenant-Governor of Java, Thomas Stamford B. Raffles, was, for example, appointed president of the Society. It then witnessed a "reanimation", as one of Raffles' biographers would later write.[79] In the course of the nineteenth century, the society continued to collect archaeological, ethnographical and anthropological objects and it was able to show acquisitions from those regions of the archipelago which had newly been brought under Dutch colonial rule, often by the use of violence. As a result, the collections grew considerably. The museum also acquired objects relating to prehistory from Europe and the Netherlands. In 1868 a new building at the Koningsplein (Royal Square) was inaugurated; it remains in use to this very day.[80] The museum gained a new role in the context of the ethical policies of the first decades of the twentieth century. As a result, the ideal of guarding and preserving local traditions – that were defined, collected and displayed in an essentialist way by the museum curators – became more and more important to the museum.[81]

During the Japanese occupation of the Dutch East Indies from 1942 to 1945, the Dutch staff of the museum were interned. In the period of Indonesian revolution and colonial warfare that followed the Japanese capitulation on 15 August 1945, the archipelago came to consist of areas occupied by the Indonesian Republic and areas that were still under Dutch colonial rule. First the museum was in Indonesian hands and then it passed into Dutch hands again. After the Dutch recognized Indonesian independence in December 1949, the museum officially became Indonesian, with a staff consisting mainly of Indonesians. The Batavian Society was renamed *Lembaga Kebudayaan Indonesia* (the Indonesian Culture Council) in 1950. In 1962, it was transferred to the Indonesian government, after which the museum became known as *Museum Pusat* (Central Museum). In 1979, it was officially renamed *Museum Nasional*.[82] In 2007, the museum was enlarged considerably when a new wing was opened, consisting of spacious exhibition rooms and new offices; the façade of this new building is a precise copy of the 1868 building.[83]

In the displays of the museum – which we studied in 2010 and 2011 – the history of the museum itself is hardly touched upon. In the new building, some information plates

78 For an early account of the history of this specific collection, cf. Van der Hoop 1941, XI-XIV.

79 Boulger 1897, 177. For the "reanimation" cf. Groot 2009.

80 Groot 2009.

81 McGregor 2004, 25.

82 McGregor 2004; http://en.wikipedia.org/wiki/National_Museum_of_Indonesia (visited on 07/07/2015).

83 Sitowati and Miksic 2006, 37–72 and 287–289.

mention that parts of the collection were acquired in colonial times during military campaigns, and there is a bust of Raffles in the colonial furniture department of the old building.[84] There are also portraits on display of Eugène Dubois, the discoverer of so-called Java Man, and of E.W. van Orsoy de Flines, the museum curator who donated his collection of Chinese porcelain to the museum in 1932.[85] The history of collecting is not a topic touched upon in the prehistoric section at all, whereas in the catalogue *Icons of Art* from 2006 this history is referred to only briefly. The information plates for the prehistoric displays do not mention that it was Van Stein Callenfels who donated his private collection of prehistoric flints to the museum in 1933; they also refrain from mentioning that he, in the capacity of curator, put together the first display in the same year.[86] However, behind the scenes, the legacy of Van Stein Callenfels was kept alive for a long time. A portrait of him, painted by the curator Dadang Undensja in 1971 from a photograph, used to furnish the old museum office. In the portrait, Van Stein Callenfels is praised as "Perintis penggalian arkeologi secara sistematis di Indonesia", 'the pioneer of systematic archaeological excavations in Indonesia.' The painting was removed when the office was converted into an exhibition room. When the staff recently moved to offices in the new wing, the painting did not become part of the new interior and was put into storage.[87] For Van Heekeren, the situation is different. The information plates on the classification of bronze axes do refer to his (post-colonial) archaeological work; the plates also mention that the classification was eventually "accomplished by R. P. Soejono".[88]

Part of the display seems to follow the original arrangements, as employed by Van Stein Callenfels and his successor, Van der Hoop, exactly; in the showcase on the Neolithic period, a prehistoric *kapak longlon* – oval (or round) stone axe – is put next to a comparable axe from contemporary Papua. The information plate explains that "the tool is still in use in the hinterland of Papua."[89] Analogous to this, in 1938, Van der Hoop spoke of a "Papoea-neolithicum", that only ended when the Europeans arrived and introduced the use of iron. The stone axes were, according to Van der Hoop, still in use, although iron axes had gained in popularity.[90] At first sight, the contemporary display in the *Museum Nasional* seems to be an example and continuation of a colonial practice described by Ian J. McNiven and Lynette Russel; they observed how indigenous people

84 Sitowati and Miksic 2006, 51. This bust (a copy of the original kept at the Royal Asiatic Society in London) was a gift of the Malayan branch of the Royal Asiatic Society on the occasion of the 150th anniversary of the Batavian Society in 1929 and meant to emphasize Dutch-English friendship. See: "De Raffles Herdenking", *De Indische Courant*, 24-12-1929; "De buste van Raffles naar het museum", *Het nieuws van den dag voor Nederlandsch-Indië*, 10-12-1929.

85 Sitowati and Miksic 2006, 60.

86 Sitowati and Miksic 2006, 61; Van der Hoop 1941, XI.

87 Interview with Ibu Ekowati, by Marieke Bloembergen, 15th December 2010, Jakarta.

88 Van Heekeren 1958.

89 Compare with Van Stein Callenfels 1934; Van der Hoop 1948, 18–19; Van der Hoop 1941, 166–167.

90 Van der Hoop 1938.

had been turned into "living fossils" through the labelling of certain groups in society as "primitive", lacking history or development.[91] Against this background, the question is raised to what extent our observation confirms Katharine E. McGregor's conclusion about the *Museum Nasional.* She states that the Indonesians inherited it from the Dutch and thereby perpetuated, even today, some parts of the colonial "agenda", such as "a discourse about primitiveness", static representations of ethnicity and the connected hierarchy of cultures.[92]

Interestingly, for the contemporary Indonesian members of the museum staff responsible for the display, the problem is non-existent, as the concept 'primitive' is not viewed as being antithetical to modernity. During an interview, curator Ni Lu Putu Chandra Dewi stresses that the display visualizes the prehistoric base of some contemporary social and cultural phenomena in Indonesia, such as, for example, the use of bark cloths or the making of pottery:

> Some aspects of prehistory are still alive in society. They are the "base" of religion, ancestral cult, and technology: stone and metal [...]. They show the visitor that objects similar to those that were made in prehistory are still in use. The Papua society is a living tradition, in the sense that they include traditions in the modern era.[93]

Following the Indonesian archaeologist Daud Tanudirjo, we can relate this stance to the nationalist conviction that the indigenous Indonesian population in the (prehistoric) past developed their own culture without any external influences. This is the so-called "local genius proposition", which Dutch and British archaeologists developed in colonial times, but which the first generation of post-colonial Indonesian successors strategically reformulated. Indonesian archaeologists were keen to demonstrate that indigenious prehistoric cultural traits were still dominant in the later Hindu-Buddhist and Islamic cultures, thereby relativizing the dominant thesis of foreign (Indian) influences being manifest in Indonesia's Hindu-Buddhist past civilizations.[94] It is a revealing example of the phenomenon whereby the objects and displays in a post-colonial museum can be interpreted and re-interpreted in many ways simultaneously.

91 They regard this as a "product of nineteenth-century social evolutionism", cf. McNiven and Russel 2005, 51. In the contemporary Western world the concept 'primitivism' is, indeed, often regarded as a perpetuation of the colonial discourse. Offering a different perspective, the anthropologist Nicolas Thomas pleads that it be regarded as "a historically situated expression" which, on that account, can serve first and foremost to make the limits of the colonial perspectives visible. Thomas 1994, 10 and 170–195.

92 McGregor 2004, 26.

93 Interview with Ibu Ekowati, Ni Lu Putu Chandra Dewi and Dhyanti Soekarno, by Martijn Eickhoff, February 4, 2011, Jakarta.

94 Ayatrohaedi 1986; Tanudirjo 1995, 71. For the 'local genius' proposition that stressed the connection between the prehistoric (megalithic) and later Hindu-Javanese cultures in Indonesia cf. von Heine-Geldern 1945, 152–153. Still influential to the 'Indianizing' thesis is the work of George Coedès, cf. Coedès 1968.

According to the historian J.M. Mackenzie, who studied the "mutation" of colonial museums into national museums, these multi-levelled appropriations also existed before decolonization.[95] In the archive of the Batavian Society we, indeed, find clues that support this observation. In 1935, for example, a Japanese prince and hundreds of Japanese sailors visited the museum in Batavia.[96] And a year later, a spokesman of the Islamic Muhammadiyah organization, that year celebrating its 25th anniversary, announced a visit to the museum by about 2000 of its members.[97] We can only speculate about what precisely the Japanese prince and sailors or the members of the Muhammadiyah organization were looking for in a museum that primarily honoured the Hindu-Javanese past and the ethnic diversity of the Dutch East Indies. But, in the context of both the rise of Japanese pan-Asianist thinking, and, at a local level, of nationalist consciousness among Indonesians, this was a place where such visitors could and may have looked for, respectively, a Greater Asian or an Indonesian spirit: thereby the visitors may have 'externalized' the colonial worldview that was undisputedly present in the museum, as well.[98]

A comparison of the various catalogues of the prehistoric department of the Museum of the Batavian Society published in the years 1934–1955 and written by the three prehistorians who take center stage in this paper might give us some clues as to how the museum decolonized in the early post-colonial era. How were colonial legacies, with regard to the prehistoric collections, defined and dealt with? The first catalogue on prehistory (in Dutch) appeared in 1934 and was written by Van Stein Callenfels;[99] revised editions of this catalogue by Van der Hoop appeared in 1939 and 1948; an Indonesian language edition appeared in 1941. It was not until1955, six years after the Dutch left Indonesia, that another catalogue of the museum's prehistoric objects was produced. Van Heekeren wrote this catalogue, entitled "Prehistoric life in Indonesia", in English.[100]

In the first catalogue the traditionally western-based narrative on prehistoric man is given a clear colonial basis. After an introduction on prehistory in general, the first chapter of the catalogue is called "The Palaeolithicum outside East Asia"; considering the fact that this chapter actually starts with a description of Palaeolithic cultures in France, it could just as well have been called "The Palaeolithicum in Europe". But writing "outside East Asia" when dealing with prehistoric objects from Europe – including Dutch ones – can be regarded as a way of connecting the western narrative about prehistoric man to a colony-centred point of view. Mackenzie recently wrote of this phenomenon:

95 Mackenzie 2009, 265–277.

96 Van der Hoop to Van Stein Callenfels, 3-4-1935. KBG DIR No. 1016, ANRI, Jakarta. Cf. "Het Japansche Eskader", *Soerabaijasch Handelsblad*, 3-4-1935.

97 President of the 'Comite van Ontvangst Congres Moehammadijah Ke 25' to the executive committee of the Museum in Batavia, 7-7-1936. KBG DIR No. 1066.

98 Recent studies on Japanese pan-Asianism include: Aydin 2007; Duara 2010; Mark 2006; Katzenstein and Shiraishi 1997; Saaler 2002; Saaler and Koschmann 2007.

99 Van Stein Callenfels 1934.

100 Van Heekeren 1955, 6.

"Museums in imperial territories represented the western view on the world, but were inevitably differently focused from those in Europe."[101] In catalogues published in subsequent years, this colony-centred Western perspective is maintained. However, in 1955, six years after the Dutch left Indonesia, when a catalogue of prehistoric objects was once again produced, a clear change of outlook is presented. It leads to, first, a decolonization and, then, a nationalization of the prehistoric past.[102] After a chapter on prehistory in general, there are four chapters on "Prehistoric Indonesia". The author, Van Heekeren, explains in his introduction that he removed all European (including Dutch) objects from the display in the museum. There was no "space" available – and they were "really out of place" there, or so he states. The world in which people who had a base in colonial society could feel connected to Europe or the Netherlands in the Museum of the Batavian Society – via prehistoric objects – had vanished.

From this we are able to deduce that, in the context of decolonization, the archaeological knowledge itself was considered neutral, whereas the focus – the connection with Europe and the Dutch "motherland" – had to change. Another example is the miniature version of Van der Hoop's Fokker F-VII airplane which, in colonial times, hung above a huge three-dimensional map of the Dutch East Indies in the Batavian Museum. Back then, it represented a Dutch colonial-hegemonic bird's-eye view of the colony. Nowadays, this model is part of the display on the history of transport – Van der Hoop's name and his famous flight in 1924 are mentioned on the information plate, but the object itself primarily illustrates just one of many possible ways that personnel and commodities once traveled through Indonesia.

What happened – against this background – to the image of the colonial archaeologists, who embodied the colonial connections par excellence? Did the herostories continue to function as authoritative prescriptions for understanding and dealing with Indonesian prehistory? When looking at present-day academic and heritage institutions, many "traces" of colonial archaeologists can be found. Nurhadi Rangkuti, director of the Balai Arkeologi Palembang (the archaeological research center of Palembang), says, for example, during a 2010 interview, with regard to Van der Hoop and his dissertation (of 1932) on the megaliths in the Pasemah-area of Sumatra:[103]

> He is our important reference. He documented, using photographs, and gave clear descriptions. Since Van der Hoop, there has not been a publication to match the one he produced. When he photographed sites, the features are clear. It is good documentation, taken from many angles. […] For us,Pasemah and Van der Hoop are one. If we talk of Pasemah, our minds think of Van der Hoop.

101 Van der Hoop 1948; cf. Mackenzie 2009, 5.
102 Van Heekeren 1955, 6.
103 Van der Hoop 1932.

> He is the pioneer; it is comprehensive.[104]

Leading Indonesian archaeologists from the *Universitas Gadja Mada* (UGM) in Yogyakarta, which was founded in 1949, likewise regard the colonial archaeologists as their "founding fathers".[105] The relationship between the individual archaeologist and them is often defined in terms of family relations. In an interview in 2010, Inayati Adrisijanti M. Romli, who started her archaeology studies at UGM in 1963, explains:

> I have been educated by generation number one. Their teachers were the Dutch. I knew Bernet Kempers and Van Heekeren. Mr. van Heekeren wanted the first generation to call him "oom" (uncle). To me, Van Heekeren said: "You can call me 'opa' (grandfather)". After 1965, when I was writing my "scriptie" (thesis), I met "opa Bob" in Jakarta. Later he visited Yogya. Yeah, it felt like family. He was the teacher of my teacher.[106]

Timbul Haryono, who started his studies at the UGM in 1964, explains in 2011 that he belonged to "the second Indonesian generation":

> Archaeology in Indonesia was "Nederland" orientated. Later it became more directed towards Australia and the US. The Dutch created an awareness amongst the people that enabled them to study archaeology. Our first knowledge of the temples was given to us by the Dutch.[107]

Following this line of reasoning, R. P. Soejono, who worked at the *Universitas Indonesia* in Jakarta and at the *Pusat Penelitian dan Pengembangan Arkeologi Nasional* (Arkenas, the National Archaeological Research Institute)[108], states during an interview in 2010:

> They [the Dutch colonial archaeologists, M. B. and M. E.] knew that Indonesia had a great past. We continued on that track.[109]

104 Interview with Nurhadi Rangkuti by Martijn Eickhoff, March 22, 2010, Palembang.

105 Discussion after the authors' presentation of the paper "The colonial archaeologist" during the conference "Sites, Bodies, Stories" at the UGM Yogyakarta, August 8, 2009.

106 Interview with Inayati Adrisijanti M. Romli, by Marieke Bloembergen and Martijn Eickhoff, 22th January 2010, Yogyakarta. For a clear example of this perspective, see the obituary of Bernet Kempers, written by Soekmono. According to Soekmono, Bernet Kempers was "de pionier van de universitairestudie van de Indonesische archeologie en de vader van het archeologisch werk in Indonesië" (he was the pioneer of academic research and the father of archaeological work in Indonesia). Cf. Soekmono 1994, 274.

107 Interview with Pak Timbul, by Marieke Bloembergen and Martijn Eickhoff, 21th January 2011, Yogyakarta.

108 The Arkenas arose from the division, in 1975, of the former Dinas Purbakala into two distinct institutes, one focusing on research (Arkenas), the other on preservation politics. Cf. De Groot 2009, 6.

109 "Ze [de koloniale archeologen, M. B. and M. E.] wisten dat Indonesië een groot land was geweest. We gaan dat voortzetten." Interview with R.P. Soejono by Martijn Eickhoff, 25 February 2010, Jakarta.

Moreover, the Indonesian archaeologists that took over the archaeological work from the Dutch, were, as in colonial times, public figures. But, as is illustrated by the biographical sketches in *Soejono's Festschrift* from 2006,[110] or by the Indonesian Wikipedia page on Soekmono, who is considered to be the Indonesian founder of Indonesian archaeology, instead of stressing the adventurous aspects of archaeological work, as the colonial archaeological heroes used to do, emphasis is now placed on professional academic background – including a national research infrastructure and an international research network.[111] When contemporary Indonesian archaeologists refer explicitly to the colonial archaeologist, it is their academic output they honour and not their colonial, adventurous lifestyle. The colonial archaeologists might have been founding fathers, but the real professionalizing of the prehistoric archaeology of Indonesia has only taken place in the post-colonial era, thanks to the support of the benevolent Indonesian state, so seems to be the hidden message.

4 Concluding remarks: Post-colonial Indonesia and the 'gift' of the colonial archaeologists

If we analyze the (auto)biographical narratives selected for this paper to uncover the way colonial archaeologists 'discovered' the prehistoric past of the Dutch East Indies, we can certainly gain some insight into the multiple cultural and social ramifications of prehistoric research in colonial times. Together with publications and archival sources, these narratives are able to reveal that the creation of knowledge of the prehistoric past entailed indigenous contributions. However, the processes for gaining access to and authority within this field, and for obtaining credit for this knowledge were still shaped by colonial circumstances. As a result, the general public, who were familiar with the archaeological hero stories, considered the colonial archaeologists themselves to have discovered the early prehistoric past of the Dutch East Indies, almost like lone travellers. The hero stories of the three men – taken together – made sure that the discovery of the prehistoric past became a Dutch success story in which Dutchmen had taken the lead.

Yet, archaeology not only reproduced colonial hierarchies, but was also part of alternative, potentially overlapping 'worlds of identification' and knowledge systems. As the diverse visitors to the museum of the Batavian Society or the letters of Resink to Van der Hoop show, the status of the archaeologist, archaeological knowledge and archaeological displays was, as a result, appropriated and questioned from diverse perspectives. Colonial archaeologists could, for that reason, hardly escape the cultural dynamics that

110 See for example: Simanjuntak 2006.

111 http://id.wikipedia.org/wiki/Soekmono (visited on 07/07/2015).

surrounded and complicated their work; in a colonial context, archaeologists were anything but lone travellers.

After decolonization, when Indonesia became a 'national reality', Indonesian archaeologists were able to visualize the (pre)historical roots of the new state with the help of materials and documentation collected by scholars – and the networks in which they were embedded – in colonial times. The colonial archaeologists involved were honoured for that 'gift' and appropriated by Indonesian archaeologists as family members, while their colonial and sometimes even racist worldview was regarded as irrelevant and glossed over. In the context of decolonization, archaeological knowledge dating from colonial times was considered neutral, whereas the focus – the connection with the Dutch motherland – changed. Moreover, the Indonesian archaeologists that took over the archaeological work from the Dutch were, as in colonial times, public figures; but instead of stressing the adventurous aspects of their work, they now emphasized their state-supported professional academic background.

Against the background of this process, and in reaction to what Susan Legêne and Henk Schulte Nordholt have coined 'colonial determinism', which is visible in many postcolonial approaches to the study of knowledge and power, we advocate in this paper a different approach towards colonial legacies in present day post-colonial archaeological knowledge. It strikes us as more rewarding to analyze the practices of excavation and the formal, scholarly and alternative forms of knowledge production in which the work of the colonial archaeological heroes was embedded, and to consider the later transformations of the status and meaning of these archaeologists and their academic work as a process of appropriation, in which the Dutch empire was scored off by the greatness of the Indonesian past.[112] Contemporary post-colonial Indonesian archaeology may still be state-centered and in that role it may, as the example of the axe from Papua shows, still overrule the diverse ways meanings and identities are attributed and negotiated on a local level.[113] But we hope to have shown that too exclusive a focus on colonial legacies impedes a balanced understanding of the successful efforts of the first – post-colonial – generations of Indonesian archaeologists to decolonize.

112 Cf. Soejono 1997.

113 Cf. Marwoto-Johan 2012; Moore 2003, 13–14.

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1 Swanenburg 1951, 112. **2** Van Heekeren 1969, book cover. **3** Swanenburg 1951, 241. **4** Van der Hoop 1935, 9.

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Stones and Stories. On the Use of Narratological Approaches for Writing the History of Archaeology

Summary

The last decades have seen considerable debate among theorists and historiographers about the extent to which historians resort to literary modes of representation and how far historical accounts owe their persuasiveness and explanatory power to narrative structures. As a result, the investigation of historical accounts using methods drawn from literary studies has become a highly diversified and rather confusing field. There is, of course, no reason to believe that the tendency to resort to particular narrative patterns has played an less important a role in the field of archaeology. Nevertheless, it is only recently that scholars have begun to apply narratological concepts in their investigations of the history of archaeology. A brief look at archaeological representations of human migrations demonstrates the usefulness of such approaches. Since these accounts usually cover long periods of time and encompass several historical actors and spaces, archaeologists have made use of certain narrative strategies in order to arrange their facts and to transform them into more or less coherent stories.

Keywords: History of historiography; history of archaeology; narratology; migration narratives.

Die Frage, inwieweit sich Historiker literarischer Techniken bedienen und ob die Erklärungs- und Überzeugungskraft ihrer Darstellungen auf vorgegebenen narrativen Strukturen basiert, ist von Geschichtstheoretikern und Historiographiehistorikern der letzten Jahrzehnte viel diskutiert worden. Entsprechend hat sich die Untersuchung historiographischer Werke mit literaturwissenschaftlichen Methoden zu einem komplexen und zunehmend verwirrenden Feld entwickelt. Es gibt freilich keinen Grund zu glauben, narrative Strukturen seien in der Archäologie von geringerer Bedeutung. Trotzdem sind narratologische Überlegungen erst in den letzten Jahren auch auf die Geschichte der Archäologie angewendet worden. Der Nutzen dieser Ansätze lässt sich besonders gut am Beispiel archäologischer Wanderungserzählungen aufzeigen. Weil diese Darstellungen in der Regel weite Zeiträume abdecken und verschiedene historische Akteure und Räume zusammenfassen,

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greifen Archäologen auf spezifische Erzählstrategien zurück, um ihre Fakten zu sortieren und in mehr oder weniger kohärente Geschichten zu transformieren.

Keywords: Historiographiegeschichte; Archäologiegeschichte; Narratologie; Wanderungsnarrative.

The article discusses initial considerations on migration narratives which I was able to formulate during research fellowships at the Excellence Cluster Topoi (CSG-V) in 2010 and 2011. Since 2014 further research on the subject was made possible by the *Deutsche Forschungsgemeinschaft* (project WI 4102/2-1 „Wanderungsnarrative in den Wissenschaften vom Alten Orient. 1870–1930" at the Institut für Altorientalistik, Freie Universität Berlin).

Hugo Winckler, the excavator of the Hittite Capital Hattusa,[1] wrote prolifically on the history of the ancient Near East at the turn of the twentieth century. Most of his articles and essays were written in an explicitly *gemeinverständlich* (popular) style and were obviously aimed at a readership among the so-called educated public, beyond academic circles. Although Winckler's controversial ideas would ensure that he remained an outsider in the scholarly world, some of his concepts turned out to be very influential for the study of antiquity.[2] Most important in this regard was his general theory on migrations in the ancient world, or rather, throughout human history. Starting from the assumption that the cradle of early civilization – ancient Mesopotamia – had been periodically devastated by nomadic peoples, Winckler came up with the following general conclusion:

> Die Geschichte des Altertums bis auf den Beginn des sogenannten Mittelalters zeigt ein unaufhörliches Auftauchen nomadischer, unzivilisierter Stämme, welche in die Kulturländer eindringen – oder auch selbst eine Kultur entwickeln – um damit zu ansässigen Kulturvölkern zu werden und ihr Geschick im mehr oder oft auch bis jetzt weniger hellen Lichte der Geschichte zu erfüllen. Der Übergang zur Seßhaftigkeit ist mit einem Wechsel der Lebensbedingungen verbunden, der sich um so schneller vollzieht, wenn die Eroberer sich in das warme Nest einer schon entwickelten Kultur hineinsetzen, der aber in jedem Falle eintritt, auch wenn der langsamere Vorgang der Erarbeitung einer Kultur vorliegt.[3]

In essays written in flowery and metaphorical language like that above, Winckler expatiated on the way that 'uncivilized' Semitic tribes of the Arabian Desert shaped them-

1 Alaura 2006.

2 Renger 1979, 164-165; Carena 1989, 96–112; Marchand 2009, 236–244.

3 Winckler 1903, 3.

selves into distinctive peoples (such as the Canaanites, the Amorites, the Aramaeans and last but not least the Arabs) between 3000 BC and 1000 AD and "flooded"[4] the region of the "Fertile Crescent."[5] He explains that these former predators, if not parasites, ultimately settled down and themselves developed into civilized peoples *(Kulturvölker)*. Thus, Winckler's condensed and dramatic account links together what are in fact very different occurrences taken from different historical contexts and compresses them into a more or less coherent narrative spanning over 4000 years of the region's history. It is the particular structure this narrative takes that is of greatest relevance for my purpose here: Winckler's narrative presents history as a circuit – describing the supposed never-ending process of migration and acculturation, destruction and foundation of culture. Naturally, his contemporaries would already have been quite familiar with circular narratives (meaning narratives featuring the repetition of certain plot points), since migrations had always played a vital role in traditional accounts of the 'rise and fall' of great empires or civilizations. Thus, one can assume that Winckler's approach owes its persuasive power in no small degree to his use of a well-known narrative structure. As I mentioned above, Winckler was never part of the academic establishment, and his writings – mostly published in journals and periodicals of which he himself was the editor – were more popular than academic in nature. Therefore, one could argue that his choice of type of narrative would have had less of an impact on academic archaeology. However, things are more complicated than that: Writing a historical narrative always requires the adoption of some kind of narrative strategy, and there is no clear demarcation line between popular and scientific writing.

In the following, after briefly outlining the debate about narratological approaches to the theory of history, I will discuss these approaches and how they can be applied to the history of archaeology. I have chosen, for that purpose, to focus on two aspects, selected with the two central aspects of the debate in mind – the relationship between author and narrator and the role of certain narrative structures or plots.

To examine that first aspect, I will look at the self-representations of excavators as protagonists in archaeological accounts of the nineteenth and early twentieth centuries. The central question here is how the results of archaeological research are *represented* and *rendered* to the public in a particular era, and how these modes of representation changed over time. I will then apply the concept of generic plot structures, developed by narratological theorists, for archaeological accounts on human migrations in the nineteenth and twentieth centuries and discuss the usefulness of this concept for the historiography of archaeology.

4 Winckler 1905, 3.

5 The term "Fertile Crescent" itself was coined a few years later by the archaeologist James Henry Breasted (Breasted 1916, 100).

1 Narratological approaches to the history of historiography

Discussion of literature and historiography's similarities and differences is, of course, anything but new, Aristotle being among those who have addressed the topic in the past.[6] Although reflections about the meaning of narratives in *modern* historiography began to emerge with the American analytical philosophy of history in the 1960s,[7] the current debate was clearly triggered and has been shaped by Hayden White's famous *Metahistory*.[8] Another important contribution from the perspective of philosophical hermeneutics was made in the 1980s by Paul Ricoeur in *Temps et Récit*[9] – to name the other of the two most prominent approaches. Since then, the investigation of historical accounts using the methods of literary studies has developed into a highly diversified and rather confusing field.[10] Nevertheless, the initial question was quite simple: to what extent do historians resort to literary modes of representation, and how far do historical explanations owe their persuasiveness and explanatory power to narrative structures?

In view of the fact that it is the historian's task to arrange the occurrences or events they are depicting in a certain temporal order, fill in gaps unmet by written and unwritten sources and, last but not least, transform all the information into a meaningful story, it seems obvious historians must of necessity rely both on their own imagination and on certain narrative strategies. The writing of history, then, is never just a matter of reconstructing and interpreting facts; it always also involves an act of composition and the combination of disparate elements, a transformation of contingency into coherency that can be described with Paul Ricoeur's famous definition of a plot as the "synthesis of the heterogeneous."[11] Thus, narratological approaches to historiography focus less on the epistemological question of how historical facts are generated by the historian than on this *arrangement* of *selected* events into a sequential and hierarchic order, their evaluation and composition to create a certain plot. White coined the term *emplotment* for this, meaning the *imposition* of a certain plot-structure upon a set of events and the resulting transformation of simple chronicles into "stories of a particular kind."[12] Accordingly, the explanatory power of historical accounts rests heavily upon the persuasiveness and "followability"[13] of certain repetitive plots which must be identifiable and regarded as convincing and sufficient by the reader. The point here is that one could always arrange the events in a different order and thus relate the same occurrences in different ways by using different "modes of emplotment."[14] Consequently, White regards historical narratives simply as "verbal fictions,"[15] and explicitly blurs if not demolishes the

6 See the comments in his *Poetics* (§ 9).
7 Danto 1965.
8 White 1973.
9 Time and Narrative, White1984–1988
10 Scholz Williams 1989; Clark 2004, 86-105; Eckel 2007.
11 Ricoeur 1984, 66.
12 White 1973, 7.
13 Ricoeur borrowed the term from W.B. Gallie 1964, 22–51.
14 White 1973, 7.
15 White 1978, 82.

traditional boundary between historiography and literature: "Viewed simply as verbal artifacts histories and novels are indistinguishable from one another."[16] Of course, assertions like this have provoked a great deal of opposition – both from historians and from narratologists. Critics have insisted what they see as a fundamental difference between "fictional narratives" and "factual narratives,"[17] and rightly pointed out that White completely ignores the problem of historical referentiality; others have simply accused him of de-legitimizing historiography as a scientific practice. Furthermore, fierce controversy emerged about the ethical consequences of what seemed to be White's postmodern relativism in the early 1990s.[18]

Space constraints preclude a presentation of the whole debate surrounding *Metahistory* here, but with respect to these general pitfalls Ricoeur's more cautious observations about the relationship between literature and historiography seem to offer a more promising approach. Ricoeur neither assigns history and literature to completely separate spheres, nor ignores the differences between them. Instead, he focuses on areas of overlaps between them and on the ways they have been adapted: the "interweaving of history and fiction."[19]

The strictly structuralist and a-historic character of White's approach appears to be its most problematic aspect: referring to Northrop Frye's famous classification,[20] White identifies only four "modes of emplotment" – romance, tragedy, comedy and satire – which, in the end, correspond with the four classical Aristotelian tropes[21] and which he presents as "archetypes."[22] Thus, he regards these variants as universal and immutable. However, the empirical basis for this highly general assertion is very limited: White draws on seminal nineteenth century works of historiography and philosophy of history only, simply disregarding the differences between such texts and the historical writing produced during the last century. Although he does try to take political context into account, connecting the four modes of emplotment with specific "modes of ideological implication,"[23] his typology leaves the relationship between emplotment and ideological implication unclear and inadequately defined. Just as the structuralist narratology of the 1960s has been challenged and edged out by diachronic approaches that focus on the historical and cultural dependency of narrative patterns and their variability,[24] White's critics have convincingly demonstrated the need to embed historical narratives in their cultural, political and ideological contexts.[25]

16 White 1978, 122.
17 Genette 1990.
18 The discussion was focused on the representation of the holocaust (see the articles in Friedlander 1992). His most prominent critic was Carlo Ginzburg (Ginzburg 1992) who puts Whites ignorance of referentiality and relativism close to the position of right-wing holocaust deniers.
19 Ricoeur 1988, 180–193.
20 Frye 2000.
21 White 1973, 29–31.
22 White 1973, 9 and 38.
23 White 1973, 22–29.
24 Nünning 1999; Nünning 2000; Erll and Roggendorf 2002; Fludernik 2003.
25 Rüth 2005; Saupe 2009.

However, White is not the only scholar to established a typology of historical narration. The most influential of the other typologies – in the German context – is certainly that of Jörn Rüsen. Actually, Rüsen does not address the relationship between literature and historiography in general; instead he focuses on *historische Sinnstiftung* through narration, meaning the way that historians use different narratives to make sense of the contingencies of history. He distinguishes four "functional types of historical narration": the traditional, the exemplary, the critical and what he calls the genetical narrative.[26] However, since the connection between these types of narration and specific political and cultural contexts is again highly unspecified, the problem of context and variability remains the same. Even more problematic is the evolutionary, if not teleological, as some critics have argued,[27] character of Rüsen's approach: he describes a "logical progression" from one type to the next and leaves no doubt that the "genetical narrative" is the most advanced and scientific approach.[28]

Of course, scholars of historiography have also borrowed many other concepts from narratology, and from literary studies more generally. The most important in this context is the differentiation between author and narrator. The question here is whether or not this distinction can be applied to historiographical accounts. According to the French narratologist Gérard Genette, for instance, this is precisely where the fundamental difference between "fictional narratives" and "factual narratives" lies: in historiography, in contrast to fiction, he argues, author and narrator are identical, because the historian has to assume full responsibility for his narrative. Thus, any claims and theses put forth can be ascribed to the historian as an individual under the rules of scientific historiography.[29] However, others have countered this by pointing out that the fact of authorial responsibility does automatically mean that an identifiable narrator is necessarily absent from all historiographical accounts. One need only think of the common use of the personal pronoun *we* in scientific texts: Usually, the *we* in question does not refer to a group of authors but instead suggests the individual author's affiliation with an imagined scientific community.[30] Moreover, history can be told from very different perspectives and thus by different kinds of narrators.[31] As a case in point, Axel Rüth,

26 Rüsen 2001; Rüsen 2005. I cannot go much into detail here. However, to put it in a nutshell, Rüsen's typology can be summarized as follows: The "traditional narrative" is focussed on founding myths and aims at the construction of identities; the "exemplary narrative" corresponds to the famous phrase of *historia magistra vitae* and thus bases upon the idea that the study of the past should serve as a lesson to the future; the "critical narrative" is simply characterized by the negation of established narratives; and, last but not least, the "genetical narrative" enables the historian to grasp the complexity of historical change by identifying the structural developments and by presenting history as a dynamic process (see the table in Rüsen 2005, 12).

27 Rieckhoff 2007, 20–21.

28 Rüsen 2005, 15. Furthermore, he clearly identifies the genetical type with the German approach of social history *(Gesellschaftsgeschichte)* as most prominently represented by his Bielefeld colleague Hans Ulrich Wehler.

29 Genette 1990, 763–770.

30 de Certeau 1988, 63–64.

31 Rüth 2005, 32–36; Bernbeck 2010, 240–42.

in his criticism of White's a-historic approach, shows how the presence of the historian as narrator has changed since the nineteenth century. Taking up a concept developed by the film theorist Seymour Chatman,[32] he distinguishes between the "covert narrator" in traditional historical accounts and the more "overt narrator" which emerged in the historiography of the twentieth century: whereas older accounts are for the most part narrated by a hidden ('covert'), omniscient, or at the very least "objective" narrator, modern-day historians usually reflect on the limitations imposed by their own social or cultural context, and thus could be considered to be "overt narrators."[33]

2 Archaeologists as narrators

There is, of course, no reason to believe that the tendency to resort to particular narrative patterns has been any less influential in the field of archaeology: like 'ordinary' historians, archaeologists have to arrange their facts in a certain sequence, fill in the gaps in the archaeological and historical record, and, last but not least construct comprehensive narratives in order to relate their results to existing interpretations, as well as to render them to the public. To do so, they rely on the same narrative strategies that historians use. However, theorists and historians of archaeology have only recently begun to apply narratological approaches to archaeological accounts. Both Manfred Eggert and Ulrich Veit draw heavily upon Rüsen's typology in their examinations of archaeological narratives.[34] Others have used White's approach to deconstruct what they regard as the "master narratives" in archaeology,[35] or to propose new modes of interpretation and representation.[36] However, in order to avoid the pitfalls associated with White's structuralism and keeping in mind the developments and changes of archaeological writing, the development of a more diachronic approach would appear necessary, one that can help scholars to understand the transformation of archaeological narratives against the backdrop of their political and cultural contexts.

Studies on the relationship between science and literature have clearly depicted the interrelations between fictional and archaeological writing in the nineteenth century. Christiane Zintzen[37] and Kathrin Maurer[38], in particular, have shown how archaeological accounts of the nineteenth century were shaped by certain narrative patterns borrowed from the realist novel. Furthermore, Zintzen identifies a fundamental interrelation between archaeology and modern literature, which she puts down to the way

32 Chatman 1978, 196–260.
33 Rüth 2005, 21.
34 Eggert 2002; Eggert 2006, 211–219; Veit 2006.
35 Rieckhoff 2007.
36 Leskovar 2005.
37 Zintzen 1998.
38 Maurer 2006.

that archaeology, with its focus on fragments and the discontinuity of the finds, perfectly reflects, the fragmentary character and the discontinuity of modern life and culture.[39]

Be that as it may, I would now like to focus on one particular aspect: the way archaeologists have presented themselves in their own narratives. Most interesting in this regard are those parts of archaeological texts that are devoted to the circumstances and process of particular excavations. In examining these, one should bear the most obvious innovation of historical writing in the nineteenth century in mind: the introduction of the doubled narrative.[40] Whereas running text is reserved, for the most part, for the main story, historiographical accounts usually contain an additional section that reports *how* historians arrived at their conclusions. According to the new standards set for scientific historiography, most notably by Leopold von Ranke, this "secondary story" – intended more for colleagues than for the wider public – is usually hidden in footnotes.

> The footnotes form a secondary story, which moves with, but differs sharply from the primary one. In documenting the thought and research that underpin the narrative above them, footnotes prove that is a historically contingent product, dependent on the forms of research, opportunities and states of particular questions that existed when the historian went to work.[41]

This implied a kind of imperative, according to which the historian should be more or less absent from the main narrative; in other words, the covert narrator was the ideal of classical historicism. Ranke himself expressed this demand very radically in a famous phrase telling of his wish to erase his self in order to let the facts speak for themselves: *"Ich wünschte mein Selbst gleichsam auszulöschen und nur die Dinge reden, die mächtigen Kräfte erscheinen zu lassen* […]."[42] This is not the place to discuss this often misinterpreted dictum, to determine whether Ranke succeeded in fulfilling this aspiration in his own work or whether it has never been more than just a pious dream of humble historians.

Whatever the case, false modesty is the last thing that one can impute to the archaeologists of the nineteenth century, and thus no comparable dictate was passed on to that field. Generally, the section reporting how the excavator found and unearthed material remains took a prominent position in archaeological writings, forming part of the running text. Thus, in contrast to the historiography of the time, in archaeological narratives these "secondary stories" included a personal appearance by the archaeologists themselves. In fact, large portions of the archaeological writings of the day were devoted to the stresses and strains the excavators endured. Furthermore, the more foreign or even exotic the setting of the story (the location of the excavation) appeared, the

39 Zintzen 1998, 16.
40 Rüth 2005, 45–47.
41 Grafton 1999, 23.
42 Ranke 1877 [1859], 103.

more the narrative's focus concentrated on the figure of the excavator. Since archaeological accounts of the time had a more-or-less autobiographical character, archaeologists were neither "covert" nor "overt narrators": they served as the actual protagonists of their narratives. Looking at these accounts and their transformations in the late nineteenth and early twentieth centuries, one can already distinguish the emergence of different types or modes of representation, which later established themselves in public culture as stereotypical key roles for the archaeologist. Cornelius Holtorf has identified four such types of representation: the archaeologist as adventurer, the archaeologist as detective, the archaeologist as the source of profound revelations and the archaeologist as someone who takes care of ancient sites and finds.[43]

The presentation of the archaeologist as heroic adventurer is definitely the oldest one of these. A good example for this is the famous report *Nineveh and its Remains* by Austin Henry Layard.[44] The subtitle of that work already signals it compendious and heterogeneous character; it also indicates that the ancient Assyrians are only one of multiple subjects covered: *With an Account of a Visit to the Chaldean Christians of Kurdistan, and the Yezids or Devil-Worshippers; and an Enquiry into the Manners and Arts of the Ancient Assyrians*. Thus the entire first volume deals with Layard's own experiences and adventures in the contemporary Near East – his encounters with 'wild peoples', seedy characters and strange customs. In other words, as one of his modern biographers has put it, Layard presented himself as Indiana Jones avant la lettre.[45] Upon its publication, *Nineveh and its Remains* smoothly fitted into the tradition of travelogues and adventure stories, and it was perceived precisely as such by contemporaries. Generally speaking, the adventure novel was one of the common modes of representation in the scientific writing of the time. The scholarly travelogue, in particular, constituted a special narrative type associated with certain features. For the most part, these reports tend to reveal the heroic and virile virtues of a male traveler, corresponding to those of the familiar figure of the lonesome cowboy, who faces perils and hardships in an alien but fascinating environment. The Near East, with its extreme climatic and political conditions, seemed to offer a particularly suitable setting for this kind of story.[46] The travelers, who were mostly British due to the power relationships of the time, enjoyed an enormous popularity in their homeland and established a special "English romance with Arabia".[47] However, as German-Ottoman ties grew stronger, more and more Germans and Austrians entered the arena, including Max von Oppenheim and Alois Musil, who would later become prominent excavators. Hence, to return to the comparison between historical and archaeological writings, in the latter, the structure of the doubled narrative seems to be reversed: the public appeal of archaeological reports was based less on the, rather scanty,

43 Holtorf 2007a, 63–75.
44 Layard 1849.
45 Larsen 1996, 52.
46 Wiedemann 2009.
47 Tidrick 1981; also Ure 2003.

depictions of ancient history than on the "secondary story," which contained a suspenseful depiction of the journey that brought the archaeologists to the discovery of their 'remains'.[48] With their obvious references to the genre of travelogues, their focus on excavations' adventurous dimensions and their effective self-representations of archaeologists as scientific heroes, books like *Nineveh and its Remains* were typical products of the nineteenth and early twentieth centuries. Other famous examples for this include, of course, the writings of Heinrich Schliemann, who drew upon the same literary genres: the contemporary adventure story and the travelogue.[49] One still finds representations of Layard, Schliemann and other pioneers of the discipline as adventurers or modern heroes in more recent popular accounts of the history of archaeology. Kurt Wilhelm Marek's (alias C. W. Ceram) *Götter, Gräber und Gelehrte*[50] is perhaps the best example, with its revealing subtitle (in the original German edition) "a novel of archaeology"; but there are also quite recent books that refer to this period as the "heroic age of archaeology."[51] However, taking into account the specific historical context from which these reports emerged appears to be a more productive approach than that offered by Holtorf,[52] who views this narrative as manifestation of an "archetypical narrative structure."[53] The context, in this case, is the age of European imperialism and colonialism. The relevance of that context becomes clear with a look at the standardized narrative structure. Focused on the archaeologist, representing a male European hero, who is forced to prove himself in a mostly non-European setting, these accounts can be regarded as a narrative appropriation and exploitation of unknown spaces.

These accounts are also inextricably linked to the presentation of the archaeologist as a source of "profound revelations"[54] about history and human nature in general, which corresponds to the third type of representation in Holtorf's classification. Far from merely excavators of material remains, archaeologists of the nineteenth century regarded themselves as rebels against a what they saw as a limited historical consciousness. In contrast to traditional historians they were able to bring the past back to life. A good example for this is the famous book "The resurgent Babylon" *(Das wieder entstehende Babylon)* by Robert Koldewey,[55] who tried to resurrect the ancient Mesopotamian metropolis by unearthing its ruins and ultimately rebuilding the city in a completely new context: the Berlin *Vorderasiatisches Museum*. It is, however, essential to note that neither Layard, Schliemann nor Koldewey nor any of the other famous archaeologists of the

48 It is however interesting that Layard later tried to separate the two narratives: in addition to his later books on archaeological excavations he wrote another influential travelogue on his early travels through Persia (Layard 1894; see Ure 2003, 19–24).

49 Zintzen 1998, 257–340; Samida 2010.

50 Ceram 2008 [1949].

51 Beyer 2010, 65.

52 Holtorf 2007a, 64.

53 Furthermore, Holtorf refers to the highly problematic concept of the mythical "hero" as delineated by the controversial Jungian mythologist Joseph Campbell (Campbell 2008 [1949]; see on this Ellwood 1999).

54 Holtorf 2007a, 84–91.

55 Koldewey 1913.

time had trained as philologists before they became excavators. In fact, they distanced themselves more or less openly from the established academic world and especially from the historians whose adherence to written sources seemed to them to be rather outdated. Archaeologists like Schliemann or Hugo Winckler, mentioned above, presented themselves simultaneously as both scholars and academic outsiders, drawing upon an emerging anti-scientific sentiment at the turn of the twentieth century.[56] Furthermore, it has never been more than a short step from the idea that archaeologists rescue and unveil hidden pasts to the idea that they actually *redeem* entire epochs, cultures and peoples from oblivion. Due to the assumption that archaeological knowledge had the potential to change our fundamental concepts of human culture, the act of unveiling the past became charged with religious importance. It is because of this partial overlap between archaeological and religious narratives that references to archaeology have played such an enormous role in modern western esotericism. While esoteric writers in the late nineteenth century, such as Helena Petrovna Blavatsky,[57] were already referring to a hidden knowledge of the past, modern proponents of the occult, such as Erich von Däniken, now present themselves consistently as representatives of a "forbidden archaeology,"[58] in pursuit of a "stigmatized knowledge."[59]

One could regard these self-representations, or role patterns – the adventurer, redeemer of the past or scholarly outsider – as the teething troubles of a discipline as it transforms itself "from treasure-hunting to science."[60] At least with respect to the public representation of the discipline, though, these roles have not lost their predominance; one could suggest that Indiana Jones has not ceased to serve as a more-or-less uninvited, but constant companion of the archaeologist. However, it should be emphasized that these clichés and narratives, far from being just annoyances imposed by the media, were originally invented by archaeologists themselves.[61]

In any case, both the representation of the archaeologist as heroic adventurer and discloser of the secrets of the past and the interweaving of the genres of archaeological texts with travelogues and esoteric literature, can be regarded as features specific to archaeology. There are other archaeological self-perceptions and references that do have a lot in common with historiography. Most important in this context is the relationship with crime fiction and the identification of historians and archaeologists with detectives. Accordingly, comparisons between criminalist and historical methods have been popular among scholars since the nineteenth century.[62] Drawing upon this analogy, the German documentary series "History" presents historians as "the detectives of history"; in the same way, popular books on archaeology refer to excavators as "detectives

56 Marchand 1996; Marchand 2009. – However, Winckler was a trained philologist (assyrologist).
57 Blavatsky 2008 [1888].
58 Däniken 2003.
59 Barkun 1998.
60 Beyer 2010.
61 Holtorf 2007a; Kaeser 2010.
62 Bähr 2006; Saupe 2009; Holtorf 2007b, 75–83.

of the past"[63] or "time-detectives."[64] According to a famous essay by Carl Ginzburg,[65] this analogy became established during the rise of the "evidential paradigm" at the turn of the twentieth century. Astonishingly, Ginzburg made no mention of archaeological practices in this context. In fact, drawing parallels between the archaeological work and police investigations has long been an established device in archaeological writings: the excavation site appears as the crime scene, the archaeologists as detectives trying to reconstruct the past by analyzing material traces and collecting clues.[66] It is interesting, however, that references to criminology did not become commonplace in archaeology until the turn of the twentieth century and the rise of the natural sciences. Archaeologists then felt increasingly called upon to emphasize the sophisticated methods that enable them to produce an ostensibly exact knowledge of the past. This had important repercussions for archaeological writing: underlining the adventurous character of the excavation no longer appeared sufficient for this purpose so the focus of the archaeological narrative shifted to the act of deciphering the past using scientific methods. This corresponds with a general change in the representation of scientific expeditions after the turn of the twentieth century. Instead of focusing on the person of the explorer, discoverer and discloser, reports focused more and more on anonymous experts and specialists.[67] Hence, figures such as the detective came to replace the popular self-representation of archaeologists as adventurers and heroes. Moreover, despite the fact that scholars have, rightly, pointed out the flaws and pitfalls of this analogy,[68] the "crime scene syndrome"[69] remains highly important in both popular and scientific representations of the archaeological work even today.

3 The case of migration narratives

The usefulness of narratological approaches to the history of the discipline becomes even more obvious with a look at concrete narrations. The main questions here are how archaeologists draw narrative connections between material finds and the written sources and previously published historical interpretations, and whether they refer to certain plots when doing so. For this kind of investigation, the most promising approach appears to be one that focuses on the representation of certain *types* of incidents – especially on those kinds of occurrences that are believed to recur repeatedly through the whole course of history.

63 Korn 2007.
64 Fagan 1995.
65 Ginzburg 1989.
66 Gründel and Ziegert 1983; Neuhaus 1999; Platzek, Hausers, and Dudde 1999; Mante 2003; Korn 2007.
67 Torma 2011, 216–220.
68 Holtorf 2003; Holtorf 2004; Holtorf 2007b.
69 Kaeser 2010, 54.

This definitely includes the history of human migrations – a field which is still one of the most important subjects in archaeology. In this sense, a historical (or archaeological) migration narrative is one specific way of representing and retelling the story of human migration in a given context. Since a migration narrative describes the movement of human beings through time and space, it expresses an "intrinsic connectedness of temporal and spatial relationships" and thus can be regarded as one of the central *chronotopes* of archaeology, to use Mikhail Bakhtin's famous concept.[70] Furthermore, the reconstruction of migration routes has always been linked to the question of the origins of certain peoples or 'races': traditional narratives of migration usually tell a linear story, covering the departure of a certain group at its mythical *Urheimat*, the migration itself and finally its definitive settling in the new territory. To that extent, they simply meet the basic – Aristotelian – definition of stories, namely they have a discernible beginning, a middle and an end. As a result, historical accounts of migrations usually cover long periods of time – sometimes ranging from an *Urheimat* up to the present. To create such accounts, archaeologists make use of a common narrative strategy, compressing long-term historical changes in order to transform them into single events – an "effect similar to that of a speeded-up film".[71] Of course, migration narratives are far from immutable and have always been affected by political and cultural change. This becomes obvious with a look at the representation of human migrations in the late nineteenth and early twentieth centuries. What is most striking in connection with these accounts is the fact that at the time archaeological interest was almost exclusively focused on the "Wandering of Peoples,"[72] meaning the movement of whole groups or collective entities, for example, certain nations, peoples or 'races'. Following Ricoeur[73] one can identify these groups as the "quasi-characters" of migration narratives. Furthermore, in historical surveys this kind of mass movement of people served as a central marker or turning point, permitting the demarcation of different periods. Most important in this context is, of course, the role of the so-called *Völkerwanderung* as a watershed between classical antiquity and the Middle Ages. In this sense, migrations – or to be more precise, the migrating peoples – were seen as responsible both for the destruction of whole civilizations and for the founding of new ones. Thus, migrations played a vital role in the classical narrative of the rise and fall of cultures and empires. As I have already shown, Winckler used migrations exactly in this way to construct his circular narrative. But how did archaeologists arrive at the "quasi-characters" of their narratives? In other words, how did they identify and distinguish different migrating peoples and how did they compose coherent narratives on the basis of the material remains they excavated?

70 Bakhtin 1981, 84.
71 Ricoeur 1984, 109.
72 Haddon 1911.
73 Ricoeur 1984, 200–202.

Up until the second half of the nineteenth century, historians usually drew upon philological methods to distinguish one ethnic group from another and to reconstruct the origins and roots of migration. However, having lost much of its persuasive power at the end of the century, philology began to appear less and less adequate to this purpose. The colonial penetration of what had, for Europeans, until then been almost completely unknown territory resulted in a massive extension of the geographical and ethnological knowledge base, which in almost no respect accorded with the traditional narratives set down in the written sources. However, the emerging science of archaeology, with its spectacular successes in excavating and visualizing the past, seemed poised to fill this gap by drawing upon new (material) sources and introducing new methods to historical research – methods borrowed, for the most part, from the increasingly influential natural sciences.[74] The introduction of anthropological methods and narratives into historical writing was archaeology's contribution to the debate on the supposed origins of certain peoples or races. Most important in this context was (physical) anthropology,[75] as a new methodological framework for the interpretation of certain kinds of objects. These included not only skull and skeletal finds, but also excavated monuments. Thus, statues, reliefs and other ancient representations of human beings were perceived less as artificial or typological portrayals than as one-to-one reproductions of the physical appearance of past peoples. This proved especially important for the identification of supposedly culturally gifted races. Accordingly, archaeologists tried to determine what race the ancient Egyptians or the Babylonians belonged to by studying representations of them on historical monuments.[76] Furthermore, the spatial and temporal dissemination of certain anthropological types was seen as indicating the origins and roots of migration of certain races from antiquity to modern times. In contrast to traditional philological methods, anthropological investigation led not only to an important extension of the temporal (as well as the spatial) perspective, but enabled the connection of two different kinds of narratives which had been separate up to then: the *longue-durée* histories of (physical) anthropology, with its *biohistorical narratives*,[77] – meaning the history of mankind in general, the development and dissemination of different races according to the geographical conditions, etc. – and *ethnohistorical narratives* in a more narrow sense – meaning the history of civilization, the recorded history of a single people, etc.

The cardinal problem associated with this new kind of archaeological interpretation involved aligning the material finds with the written sources and existing philological classifications. Although some observers were fully aware of the differences between linguistic and anthropological concepts,[78] in practice the mixture of classifications was in-

74 Trigger 2006; Eberhardt 2011.

75 Unlike in the Anglo-American context, the German term *Anthropologie* (without further attributes) meant just physical anthropology and should not be confounded with cultural anthropology.

76 Wiedemann 2010.

77 Lipphardt 2008, 35–38; Lipphardt and Niewöhner 2007.

78 See for instance Meyer 1910, 73–75.

creasingly predominant in archaeological and historiographical works – with the result that certain groups of peoples, which had previously been classified as language families, were promoted to anthropological types or races that exhibited specific physical traits.[79] Furthermore, since archaeological research was dependent on public support (just as it is today), producing narratives of general interest was of great importance. One could not rely upon anthropological data alone to produce such a narrative. The shape of a skull or the representation of some unknown human being on a monument remains more or less meaningless without a relevant story behind it. What really mattered here was to get the skulls and monuments to speak by putting them into a certain narrative context. This, of course, could not be done without referring to written sources and existing interpretations. Assigning different anthropological types to well-known subjects of history appeared to offer a way to visualize the central peoples and races of the ancient world. To give an example, the representation of human beings found on Mesopotamian monuments were immediately related to the Bible and used for a typological interpretation of the peoples of the ancient Near East.[80] The aim of establishing a complex ethnohistorical cartography of the entire region, from antiquity to modern times, required the comparison of material from different epochs. Accordingly, a change in representations was taken as evidence for mass migrations, or at least for violent incursions by foreign invaders. This kind of reasoning seemed to offer a way to identify the racial character of several historical peoples and a way to verify their origins and routes of migration.

Finally, the lingering importance of the written sources resulted in a great similarity between archaeological and historical accounts of migrations. This becomes clear with a look at the plots and role patterns common in the nineteenth and early twentieth centuries, which cannot be reduced to White's four-type classification. One can identify a restricted number of relatively flexible migration narratives with certain plots, roles and patterns of sequence in these texts. Individual narratives contradict one another and can appear almost incompatible, but in a way they all belong to the same mode of historical representation and explanation. With respect to the protagonists or "quasi-characters," most archaeological accounts of migrations relied on a narrative pattern which can be characterized as 'heroic'. Accordingly, migrating peoples were presented as – physically as well as morally – superior conquerors or bearers of civilization, and the *Völkerwanderungen* were elevated to the status of crucial factors explaining historical changes.[81] This is, then, to a large extent consistent with White's description of the "Romance" as a

79 However, an increasing awareness of the difference between anthropological and philological concepts led to the establishment of new classifications in physical anthropology. The best example in this context is the substitution of the philological term "Semitic race" by the anthropological concept of an "Oriental race" (Kiefer 1991; Wiedemann 2010; Wiedemann 2012).

80 Rawlinson 1862; Meyer 1913.

81 Chapman and Hamerow 1997; Trigger 2006, 314–385.

"drama of self-identification symbolized by the hero's transcendence of the world of experience".[82] It has been clearly shown that the rise of archaeological "migrationism" and "diffusionism"[83] was closely connected with the politics of nationalism and imperialism:[84] For obvious reasons, stories about the diffusion of culture and migrating carriers of civilization appeared in an especially flattering light to those European nations who regarded their own colonial expansions as cultural missions. The most important example in this context is the Aryan or Germanic myth, i. e. the idea that the Aryan or Indo-Germanic peoples had appeared in every historical context as bearers of culture – a "civilized people" *(Kulturvolk)* par excellence. As is well known, this narrative fitted perfectly into racial and extreme nationalist – *völkisch*[85] – concepts of history[86] which emerged after the turn of the twentieth century. In this context, the focus of representation shifted more and more from bearers of culture to ruling castes of conquerors; in other words, the image of the Aryans as *Kulturvolk* was partly replaced by the concept of *Herrenvolk*. However, Marxist archaeologist Gordon Childe's adaptation of ideas expressed by the *völkisch* prehistorian Gustav Kossina clearly demonstrates that this kind of "migrationism" is not associated only with a specific set of ideological or political convictions.[87]

The colonial narrative and the heroic epic did not remain unchallenged however and were thus just two options available for representing the history of invasions. An alternative narrative focused more on the violent character of invasions: archaeologists presenting the history of invasions from the perspective of the conquered peoples established a narrative type that might be called 'tragic'. Most influential in this context was the general theory on human migrations in history put forth by the Leipzig geographer Friedrich Ratzel. Proceeding from the assumption that there is a fundamental dichotomy between sedentary peoples and nomads, Ratzel believed he had detected a historical law under which what were called *Kulturgebiete* (meaning areas populated by sedentary peoples) were periodically overwhelmed by nomadic invaders from the surrounding deserts and steppes, who steam-rolled over them.[88] The immediate adoption of Ratzel's theory in anthropological and archaeological writings of the time was due in no small part to his alluring imagery: Using metaphorical language suggestive of thermodynamic forces, he called the areas populated by nomadic peoples "cauldron of peoples" *(Völkerkessel)* in which the masses were brewed and bubbled until the "cauldron" exploded and the nomads flowed into the *Kulturgebiete*.[89] In this form of narrative,

82 White 1973, 8.
83 Adams, Van Gerven, and Levy 1978.
84 Trigger 2006, 202–204.
85 The untranslatable (Hutton 2005, 7–13) term *völkisch* refers to a distinctive branch of the extreme national right in early twentieth-century Germany: the so-called *völkisch* movement (Puschner 2001).
86 Häusler 2004; Wiwjorra 2006.
87 Trigger 2006, 235–248; Veit 1984.
88 Ratzel 1890, 8–10.
89 Ratzel 1898, 69.

migrations tended to be presented not as the result of social transformations or environmental changes, but through imagery of volcanic eruptions, leading Ratzel to refer to migrations as "floods of peoples" *(Völkerfluten)*.[90] Applied to ancient history this meant that migrations and invasions were portrayed as tragedies, with the immigrants or invaders presented as barbaric villains who were to blame for the destruction of civilization. In ancient Near Eastern studies, for instance, historians and archaeologists pointed to a number of such barbaric invasions of Semitic peoples from the desert in order to explain what they saw as ups and downs in Babylonian culture.[91] The most influential author in this context was that same Hugo Winckler, whose circular narrative of the history of the ancient Near East simply represented the rigorous application of Ratzel's theory. Thus, Winckler described the Arabian Desert as a "chamber of Semitic peoples" *(semitische Völkerkammer)* and tried to distinguish different "waves of peoples" *(Völkerwellen)* which invaded the lands of the Fertile Crescent and destroyed the civilizations that existed there.[92]

Of course, the history of migrations was presented in ways other than through the heroic and the tragic narratives. The most influential among these other narratives was the romantic narrative of cultural pessimism. Against the backdrop of the discontent with modern civilization emerging in *fin de siècle* Europe, interpretations of human history and culture were subjected to fundamental changes and re-evaluation. Attitudes toward culture and civilization grew increasingly ambivalent; ultimately people began to see them as manifestations of decadence. In this light, the supposed destruction of civilizations by outside invaders took on a different appearance: sedentary civilizations of antiquity began to be depicted as morally or racially degenerate while invading 'wild peoples' from outside began to embody natural virtues such as moral purity, virility and artlessness. In narratives of this type, historians and archaeologists could simply refer to the traditional myth of the 'noble savage'. In the context of the history of the Near East, this topos was represented by the "Noble Bedouin."[93] We see this in Berlin Orientalist Otto Weber's eulogizing of the role of the 'Semitic' Bedouin in the history of the ancient Near East, for example. Weber refers to the invasions of the "brave sons of the desert", who represent a pure and original element *(urwüchsiges Element)*, refreshing the decadent and dying cultures of ancient Mesopotamia.[94] Furthermore, this narrative made it possible to draw a parallel between the ancient Germans and the Arabs, as both were presented as 'young peoples' who had destroyed the decadent civilizations of late antiquity. Arthur Ungnad, for instance, a German assyriologist who was later to become an enthusiastic supporter of National Socialism, did not stop at underlining the nomadic roots

90 Ratzel 1923, 74.
91 Wiedemann 2010.
92 Winckler 1899.
93 Toral-Niehoff 2002.
94 Weber 1902, 3–4.

of Germanic and Semitic peoples, but even speculated about the existence of racial connections between them.[95] In a sense, one could see this as transformation of the heroic: whereas the attribute of the heroic was ascribed to the *founders* or distributers of culture and civilization in the imperial or colonial myth, the narrative of cultural pessimism reserved heroic features for the *destroyers* of culture.

4 Migrating narratives

To sum up, archaeologists have always told the history of human migrations in different ways and made use of different narrative patterns. What is crucial here is the possibility to narrate the same occurrences – in this case the same migration processes – in multiple ways: The archaeologist's choice of the heroic (colonial), the tragic, the romantic or the circular narrative to relate the history of a certain migration has never been dictated by facts or finds but is a question of, to use White's famous term, emplotment. The same applies to the assignment of particular roles to certain historical subjects. What this means with regard to the archaeological accounts of human migration in the nineteenth and early twentieth centuries, is that the representation and valuation of certain historical subjects as heroes or victims, founders or destroyers of human culture, was relatively arbitrary. Nevertheless, the flexibility of this 'casting,' the ease with which one role could be exchanged for another, was restricted by contemporary nationalist and racist resentments and prejudices. As a case in point, presenting African peoples or 'Negroraces' as the primary bearers or carriers of human civilization obviously seemed impossible to the archaeologists of that era.

However, neither the (self-)representation of excavators in archaeological writings or in popular culture, nor migration narratives are immune to change, and thus neither should simply be attributed to archetypes or cognitive patterns. Both have always been subjected to continual change. Hence, like historical narratives in general, they must be analyzed against the backdrop of their specific cultural and political contexts. It seems clear, for example, that both heroic narratives analyzed above – the presentation of the excavator as heroic adventurer and the identification of certain heroic peoples – perfectly corresponded to the colonial and imperialist contexts in which they took shape.

The purpose of this paper was to demonstrate the possibilities and the usefulness of narratological concepts for the historiography of archaeology. Yet, further investigation could shed light on the general dissemination of these narratives beyond the disciplinary

95 Ungnad 1923, 5. It is most important to mention that according to a new anthropological classification after the turn of the twentieth century, the Jews were no longer grouped into the same category as other Semitic peoples. Accordingly, Ungnad could speculate about racial relations between the Germans and the Semitic Arabs without including the Jews (see Wiedemann 2012).

borders of archaeology (and history). This, of course, would raise new questions, such as whether one could identify specific scientific or literarily contexts from which central narratives or metaphors originally emerged and reconstruct their 'migration routes' – meaning the ways they were adapted and transformed in different disciplines.

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"More Important than all Technical Features Would Appear to Us the *volkliche* Differences". Gotthard Neumann and the *völkisch* Thought in German Prehistory, 1920s to 1960s

Summary

This paper examines the theoretical and methodological value of combining *Begriffsgeschichte* (conceptual history) with Pierre Bourdieu's theory of habitus and social field by focusing on definitions of the *völkisch* thought in German prehistory. The theoretical perspective in this paper is that concepts on the semantic level are interlinked with historical processes in the social space, or the scientific field in this example. On the one hand, it is evident that *völkisch* elements belonged intrinsically to prehistoric archaeology in its development as an autonomous discipline in the scientific field. On the other hand, racist and *völkisch* thoughts were a result of the heteronomization that was enforced during the Nazi regime, when prehistoric archaeologists tried to use the Nazis to establish their discipline in academia.

Keywords: Habitus- and field-theory; *Begriffsgeschichte*; *völkisch* thought; prehistory; Nazi regime.

In diesem Artikel frage ich nach der theoretischen und methodologischen Tragweite einer Kombination von Begriffsgeschichte und Pierre Bourdieus Habitus- und Feldtheorie. Der Ansatz wird am Beispiel eines Definitionsversuchs völkischen Denkens in der deutschen Prähistorie ausgelotet. In dieser theoretischen Perspektive sind Denkhaltungen auf der semantischen Ebene mit historischen Ereignissen und Prozessen im sozialen Raum, das heißt in diesem Fall im wissenschaftlichen Feld, verknüpft. Auf der einen Seite kann dadurch gezeigt werden, dass völkische Elemente zur Entwicklung der prähistorischen Archäologie als selbständiges Forschungsfeld gehörten. Auf der anderen Seite wird deutlich, dass rassistisches und völkisches Denken das Resultat einer Heteronomisierung der Prähistorie war, die sich während des NS-Regimes massiv verstärkte, als deutsche Prähistoriker versuchten, mit Hilfe einer Zusammenarbeit mit NS-Politikern ihren Forschungsbereich akademisch zu etablieren.

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Keywords: Habitus- und Feldtheorie; Begriffsgeschichte; völkisches Denken; Prähistorie; NS-Regime.

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1 Semantic structure and the scientific field

Recent studies in the history of science and the humanities have effectively used approaches of conceptual history and historic semantics for explaining discursive transfers of terms and concepts between scientific communities and between the scientific field and other social fields, such as politics, economics, and the public.[1] Yet, most of these studies lack a theoretical discussion of the ways that concepts were interrelated with the trajectory of the researchers who developed these theories and methodological approaches in their social context. In this paper, I argue that one was closely linked to the other: semantic structure and the development of concepts are interconnected with social processes and historical incidents. To illustrate my theoretical assumption, I will examine the role and function of the *völkisch* thought in German prehistory. I will consider how the usage of this concept was determined by the situation of prehistory in the scientific field as well as by the specific habitus of prehistoric archaeologists, and how this situation, reciprocally, influenced the meaning of *völkisch* thinking during the Nazi regime, taking into account that, in this period, prehistory was established as an academic discipline at German universities. In addition, I will look at the development of *völkisch* thoughts after the fall of the Nazi regime, arguing that these thoughts had to be transformed and adapted to the new political situation of the German Democratic Republic (GDR) and the Federal Republic of Germany (FRG).

To exemplify the meaning and function of *völkisch* elements in German prehistoric archaeology in the first half of the twentieth century, I will focus particularly on one example, namely, the Thuringian prehistoric archaeologist Gotthard Neumann (1902–1972). Neumann serves as a gauge for my argument because his professional career spans from the 1920s through the Nazi regime to the GDR. Yet, focusing on Neumann as a unique example would define my approach as neo-historicist, which is why I will, with the help of the method of prosopography, relate Neumann's habitus and his

1 See Eggers and Rothe 2009; E. Müller and Schmieder 2008.

scientific thoughts as well as his social position in the scientific field to those of other German prehistoric archaeologists.

According to Reinhart Koselleck, the use of specific terms in language is dependent on contemporary incidents: there is a structure of historic incidents and a structure of the terms and concepts that interact with one another in a reciprocal manner.[2] As I said above, most studies of the history of concepts lack theoretical and methodological definitions of the social structure under examination. To conceive the social structure theoretically, I will define it with the help of Pierre Bourdieu's habitus- and field-theory. In Bourdieu's theory, the social space of individuals is the social field, which is specified in several subfields such as the political, the economic, or the scientific field. This differentiation is a main characteristic of every modern Western society.

The field, in this case the scientific field, is a net of objectified relations between the agents' positions, whereupon the analysis of these relations locates the position of an individual in the field.[3] The position of a scientist or a scholar in the scientific field is strongly dependent on his specific habitus. Habitus is the expression of lifestyle, transcending individual and collective forms of practice.[4] Moral and political values as well as scientific thinking form the habitus of a scholar. The scholars are not aware of their habitus, rather their habitus is a result of their specific socialization in family, school, and university, which directs their agency.[5] Both, habitus and social field, are mutually interrelated with each other.[6] The scientific field is constituted by the habitus of the scholars and, simultaneously, the field constitutes their habitus. In other words: the scientific field is the radius of operation of the various forms of the scholars' habitus.[7]

There are two main groups of individuals in the scientific field: one group holds the powerful positions (dominants); the other aspires to these positions (dominated). The battle between these groups is the normal condition of the scientific field. In order to attain dominant positions, newcomers, who are always in a dominated position, need to accumulate scientific capital,[8] which includes scientific and technical knowledge or academic titles and publications: in short, scientific authority. This sort of capital is only significant in the scientific field. Scientific authority can be accumulated by the exchange of types of capital from other fields, for example, economic and social capital from the political field.[9] Types of capital mobilized from other fields can be used effectively in the scientific field only if researchers transform them completely into scientific authority, because scientific authority is the only kind of capital that really counts in the scientific field. Furthermore, this transformation is possible only if the scientific field has achieved a symbolic autonomy from all other fields, an autonomy which contains rules and values

2 Koselleck 2006 [1986], 56 and 62–63.
3 Bourdieu 1999, 365.
4 Bourdieu 2009 [1976], 179 and 182.
5 Bourdieu 1974, 40; Bourdieu and Wacquant 1996, 102. Also see Raphael 1991, 239.
6 See Bourdieu 1974, 19–20; Bourdieu 1998, 20.
7 Raphael 1991, 241.
8 Bourdieu 1976, 92–93 and 96.
9 Hachtmann 2007, 36.

that matter solely in this field.[10] Within the scientific field, the process toward autonomy is a characteristic of particular disciplines or sub-disciplines as well. For the scientific field, autonomy means only symbolic autonomy, not economic autonomy, which the scientific field can never achieve because it is always economically dependent on other social fields.

After graduation from university, newcomers entering the scientific field have two strategic opportunities: namely, either to follow the dominants and their orthodox opinions, methods, and approaches, or to adopt a revolutionary strategy and struggle against orthodoxy. Of course, these two strategies should be understood as a simplification of social reality. To follow the dominants (orthodox strategy) does not mean that newcomers stubbornly follow the professors in an obsequious way; it allows that younger scholars build upon, expand, or modify existing paradigms, models, or interpretations. However, they don't challenge the paradigms established by their masters. In contrast, the revolutionary strategy, which Bourdieu calls heterodox, encompasses for example the founding of new journals or the establishment of new methods and theories in order to break the *doxa* or scientific paradigm established by the dominant group.[11]

It is most important that both kinds of dominated scholars, the orthodox and the heterodox, respect the rules of the scientific field and share the goal of accumulating as much scientific authority as possible. They both fight with 'permitted' or 'legitimate weapons.'[12] Permitted weapons are tools or means for accumulating scientific authority considered as 'legitimate' in the scientific field. These rules are defined by the dominating, powerful scholars, but they are also traditionally established over the course of the historical development of the scientific field and of a discipline in particular. I would like to explain this principle with the help of two examples. To found a new journal in which a group of authors challenges the scientific assumptions of older and established scholars is a 'legitimate weapon' (and a heterodox strategy) in the acquisition of scientific authority. To suppress a dominant scholar from his position with the direct help of politicians is an 'illegitimate act,' because the younger scholar is not fighting with scientific means but with political ones. The heterodox strategy becomes illegitimate only with the usage of weapons that are not permitted by the scientific field. This Bourdieu calls a heteronomous strategy. Illegitimate weapons can include political power or economic means that allow researchers and scientists to attain powerful positions in the field that they would never have achieved if they had fought the battle with 'legitimate weapons.'

Using Bourdieu's theory for analyzing the development of German prehistory and the role the *völkisch* thought performed within it, I want first of all to determine whether *völkisch* thoughts entered into prehistory from outside the scientific field, presumably

10 Bourdieu 1998, 120–121.

11 Weber 1984, 342.

12 Bourdieu 2001, 51.

from the political field, or if this concept was from the beginning a part of prehistory, genuinely developed by prehistoric archaeologists.

2 Prehistory and the *völkisch* thought

As Peter Rowley-Conwy demonstrates, the concept of 'prehistory' and as chronological term originated in Scandinavia. It was developed by the Danish historian and philologist Christian Molbech in the early 1830s. The emergence of these terms was connected with the aim of eighteenth-century scholars to focus on early myths and legends "to extract the kernel of historical truth" in them and to name the era before 800 AD as 'prehistoric.' For the investigation of this period, scholars should examine artifacts, namely the material remnants of ancient people, as C. J. Thomsen proposed.[13] Since these artifacts did not represent any evidence regarding historical developments, the focusing on culture, religion, and myth was crucial for the development of prehistoric archaeology. This has also to do with the fact that Nordic mythology was very popular among the Scandinavian public, which is why prehistoric archaeologists could make use of this public resonance in order to expand their research field.[14] In addition, the political and intellectual debates between Danish and German claims on Schleswig and Holstein in the 1840s and 1850s proved to be crucial for the further development of prehistoric archaeology. For example, the Danish scholar J. J. A. Worsaae used archaeological evidence to counter the historical argumentation of German philologist Jacob Grimm.[15] The close connection between myth, nationalist thinking and ethnic and racial constructions in the development of prehistoric archaeology established the concept of the existence of an 'eternal' and constant *Volksgeist* (folk spirit) of ethnic communities.[16] The role of the *völkisch* thought in prehistoric archaeology, especially in Germany and Austria, has to be seen in the context of this development.

The term *völkisch* is closely related to specific social groups of the life-reform movement and to political groups, as well as to individual advocates summarized in what we call today the *völkisch* movement.[17] These groups originated in the late nineteenth century and gained social relevance and political power in the 1920s.[18] There is no coherent translation of *völkisch* in the English language. *Völkisch* means folkish and ethnic, but it also encompasses the meaning of nationalist and sometimes, but not always, 'racial' and racist. *Völkisch* thinking could also relate to culture or cultural phenomena. Significant for this term, however, is the fact that it was from its origins linked to a biological-organic concept of people. The nearest translation in English would be,

13 Rowley-Conwy 2006, 107–109.
14 See Ross 2003.
15 Rowley-Conwy 2006, 112–120.
16 Brather 2004, 29–52 and 77–89.
17 See Breuer 2008.
18 Puschner 2001.

therefore, ethnic-nationalist or ethnic-racial. Although apolitical groups within the life-reform movement as well as some leftist groups used the term *Volk* in their alternative ideas on how German society should be organized, the semantic connection of *Volk* and race, and, related to that, of tribe or league, was mainly found in right-wing German political movements.[19] Only right-wingers labeled their own political group as *völkisch*. These groups not only consisted of politicians but were also comprised of right-wing scholars or public intellectuals, such as Houston Stuart Chamberlain, a member of the *Alldeutscher Verband* (Pan-German League), or the scholars Martin Spahn and Max Wundt, who were members of the *Völkisch* Reich-Committee of the German National Party (*Völkischer Reichsausschuss der Deutschnationalen Partei*).[20]

In the political philosophy of these thinkers, *Volk* is the starting point and central category.[21] At the fin de siècle, *Volk* functioned in the political language as a term distinct from the concept of nation and the Wilhelmine constitutional state.[22] In Germany, *Volk* characterized a certain group that was constituted through biological criteria, lifestyle, and customs.[23] Based on the geopolitical assumptions of the nineteenth-century geographer Friedrich Ratzel or the cultural historian Karl Lamprecht, German scholars connected the biologically-culturally defined *Volk* with the *Boden* (soil) on which the *Volk* lived and in which it was rooted. During the 1920s, this concept was transformed into the idea of *Raum* (space), in particular a biological-cultural *Volksraum* or *Kulturraum* (people's space or cultural space).[24] Soil or space and *Volk* formed an organic entity that became an ethnic group distinguishable from other ethnic groups by its arts and crafts, for example. Such an ethnic group was on the smallest scale a family, on the middle level a tribe, and on the largest scale a *Volk* and a 'race'. In addition, *Volk* had a temporal dimension: by exploring the space and the cultural findings therein, one could examine the ancestry of the contemporary people. Epistemologically, the study of ancient ethnic groups through the examination of the contemporary *Volk* was only possible with the assumption of a stable biological substratum that outlived historical development. Another element that was crucial for these political groups was the German nationalistic aspect. The mindsets of *völkisch* thinkers were determined not only by an ethnic-racial philosophy of history and sociology, but also by the belief that one group was superior to all other ethnic groups in world history. For the majority of those German intellectuals, this group was the *Germanen* (Germanics) and the German *Volk* which developed from the *Germanen*, a notion which they regarded as an objectively given element.[25]

19 Koselleck 1992b, 390; Muller 1987, 20.

20 BAR, R 8048/315, fol. 5: Letter dated 8 August 1916, Houston Stewart Chamberlain to J. F. Lehmanns (publisher); BAR, R 8048/223, fol. 60: Letter dated 16 October 1924, Völkischer Reichsausschuss der Deutschnationalen Partei to the Alldeutschen Verband.

21 Herbert 1996, 59.

22 Koselleck 1992a.

23 Koselleck 1992a, 383.

24 See T. Müller 2009.

25 Koselleck 1992b, 144.

In Germany, prehistoric archaeology became more and more specified at the exact time of the emergence of the first intellectual supporters of the *völkisch* movement. In the late nineteenth century, researchers with various different backgrounds engaged in prehistoric archaeology. Some were architecture historians, philologists, and classical archaeologists who began to shift their focus from antiquity to the remains of the Stone, Bronze, or Iron Ages (in the categories of today), as for example Carl Schuchhardt, the director of the department of prehistory at the Ethnological Museum in Berlin. Others were non-academic archaeologists who organized themselves into historical-cultural associations.[26] Gustaf Kossinna, a scholar of German philology who turned to prehistory and received an applied professorship at the University of Berlin in 1902, claimed that there was a direct correlation between ethnicity, language, and archaeological objects. He summarized his approach in the following phrase: "Clearly outlined archaeological cultural provinces always correspond to specific peoples or tribal communities"[27]. He and his followers believed that the Germanic element was an a priori material as well as immaterial (spiritual) entity.[28]

To understand the connection between *völkisch* thoughts in prehistory and the development of prehistory from a minor research area in the scientific field to a relatively autonomous and academically established discipline, I have to explain some of prehistory's structural characteristics around 1900. The development of prehistoric archaeology from a predominantly laymen-practice to a scientific discipline was part of the establishment of the disciplinary structure of the scientific field that happened in the course of the nineteenth century and that proved to be characteristic for science and the humanities until today.[29] Disciplines are defined as stable social organizations in the scientific field, in which scientific knowledge is produced.[30] Several elements are crucial for such organizations. There must be a community of communication, in which scholars debate about methods, contents and aims of their discipline.[31] Even though many European prehistoric archaeologists advocated a highly nationalist attitude, they developed an international scientific community. Since the mid-nineteenth century, scholars visited museums abroad and maintained international correspondence about the newest developments in their research field. Books written in other languages were translated and there were also international congresses for prehistoric archaeology.[32] Important elements for the development of such a community was the establishment of standardized knowledge one finds in lexica and journals as well as the development of well-defined objects of research and specific methods for investigating these objects.[33] Further, it is

26 See Marchand 1996, 154–156 and 162–180.
27 Cited in Klejn 2008, 317. My translation.
28 Kossinna 1911; see Veit 2000.
29 See Stichweh 1984.
30 Laitko 1999, 31; Morell 1990.
31 See Weingart, Carrier, and Krohn 2007, 41.
32 Kaeser 2008; Rey 2002.
33 Clark 1974; Krohn and Küppers 1989, 105; see also Guntau and Laitko 1987, 22; Reinhardt 2006, 386–388; Weingart, Carrier, and Krohn 2007, 1983–184.

most crucial for scientific disciplines that they have a system of symbolic reputation and institutions that exclude laymen.[34] Prehistory had such characteristics and, therefore, it was a scientific community that was not yet completely established at the German universities of the 1920s.

This lack of academic acceptance rooted in the epistemological and methodological characteristics of prehistoric archaeology. Prehistory belonged to the humanities but was one of the newer cultural research areas such as *Volkskunde* (folklore studies), relying on material, empirical evidence and using methods from the natural and technical sciences, such as those from physical anthropology. While the classics could refer to high-end aesthetics that corresponded to the tastes of the upper bourgeoisie, prehistoric archaeologists principally researched small, broken fragments of brown or black colored pots or holes in the ground.[35] Prehistory scholars did not research the classical world; rather, they studied the history of 'barbarians' such as the Germans or the Celts. This was one of the reasons why prehistory had problems in gaining credibility in the scientific field. Prehistory was therefore in a dominated position in the scientific field of the early 1930s, although a relatively large community of researchers existed. Prehistoric archaeologists did not possess enough scientific authority in the scientific field to establish academic institutes to a significant extent, even though they held some strong positions in museums or worked in antiquities and monuments offices in several regions in Germany. Thus, although prehistoric archaeology in Germany was clearly a scientific community in the 1920s, academia was still dominated by the classics and by philology.[36]

Finding themselves in dominated positions in the scientific field, Kossinna and others tried to import forms of capital from other social fields to reinforce prehistory. Kossinna was not the only archaeologist who championed *völkisch* interpretation, but he was one of those researchers that chose a heteronomous strategy because he used politics as a means to gain a powerful position in the scientific field. For him, *völkisch*-nationalist, neo-conservative, and imperialistic cultural-political organizations such as the Pan-German League (*Alldeutscher Verband*), the German League (*Deutschbund*), the Nordic Ring (*Nordischer Ring*), or the Gobineau Society (*Gobineau-Gesellschaft*) offered promising resources.[37] Already in 1925, Kossinna collaborated with the German-*Völkisch* Freedom Party (*Deutschvölkische Freiheitspartei*), the NSDAP (National Socialist German Workers' Party), and the Economic Association (*Wirtschaftliche Vereinigung*) in proposing that the Prussian State Diet (*Preußischer Landtag*) encourage prehistory in public schools, teacher training, and universities.[38] Kossinna offered scientific expertise to these groups, which were constructing a *völkisch* identity based on the 'glorious history' of the Germans and

34 Guntau and Laitko 1987, 40.
35 See Eberhardt 2011, 151–188.
36 See Wiwjorra 2002.
37 Grünert 2002, 310, 312–315.
38 Pape 2002, 167.

were therefore optimally served by the theory that Europe's high culture originated in the 'Germanic race' that had come from Scandinavia and the northern German regions. This relationship produced an intermingling of political-philosophical ideas with scientific problems,[39] a sort of scientization of politics and, mutually, a politicization of prehistory.

Yet, to portray the *völkisch* thought solely as an element taken from outside the scientific field and adapted by those prehistory scholars who held dominated positions in the field and intended to bolster their positions with the resources of right-wing politicians is to tell only one part of the story. It is dangerous to argue that the *völkisch* element in prehistory was not scientific or that it was divided from 'real research,' because this line of argument separates Nazi and *völkisch* ideology from scientific principles. This separation was created by prehistory scholars after World War II in order to legitimize their work and allow them to continue it after the fall of the Nazi regime.

In prehistory, there existed a genuine scientific theory, namely, an ethnic epistemology of material culture. The central assumption of this particular theory was the supposed connection between cultural phenomena and material biological entities, which were the *Völker* (peoples). From the perspective of habitus- and field-theory, the difference between constructions we call *völkisch* and this scientific ethnic-historic principle was the degree of credibility. Whereas *völkisch* assumptions were usually Germanocentric, taking the 'high culture' of the *Germanen* and their origin from the north (Nordic theory) as a given fact, the Germanics in the genuinely scientific approach were not the core figures but only one of several *Völker*, besides the Celts or the Slavs. Kossinna was a representative of the Germanocentric version owing from his heteronomous strategy. Representatives of the more credible approach were the well-known archaeologist Carl Schuchhardt or his student Wilhelm Unverzagt. Although they worked together with so-called *völkisch* researchers around the network of German *Ostforschung* (Eastern Studies),[40] which attempted to legitimize German claims after 1918 on lost territories in Eastern Europe, Schuchhardt's assumptions, theories, and methods had more credibility in the scientific field and above all in prehistory than those of Kossinna. In this case, *völkisch*-ethnic thoughts were part of prehistory's development as an autonomous scientific discipline. It marked prehistory as symbolically distinct from other disciplines such as the classics.

In summary, *völkisch* concepts originated in two ways in German prehistory: first, they were a result of the import of radical right-wing ideas from the political field; second, there was a genuinely scientific *völkisch*-ethnic epistemology that served as a social distinction between prehistoric and classical archaeologists. Whether *völkisch* elements in prehistory were regarded as scientifically legitimate depends very much on the

39 See Bourdieu 1998, 19 and 59–60.

40 See Burleigh 1988, 56 and 66; Grunewald 2009.

amount of scientific credibility these concepts had in the scientific field. Direct imports of racial or *völkisch* ideology from the political field without a complete transformation of them into the language and scientific values of prehistory did not have much credibility.

3 Habitus, concepts, and social structure: Gotthard Neumann's trajectory

3.1 Gotthard Neumann's habitus

Gotthard Arno Ernst Neumann was born in Schwabsdorf in the administrative district of Weimar in Thuringia. He was socialized in a milieu that can be characterized as a mixture between petit bourgeois and educated middle-class, for which a national-conservative mindset and evangelical-Lutheran religious values were significant. Neumann's father was first a pastor, but turned to be a teacher around 1900. He later became a senior teacher and principal of the *Realgymnasium* (high school with a focus on sciences) in Weimar and was a representative of the *Landtag* (state diet) for the German People's Party (*Deutsche Volkspartei*) from 1920 to 1924. His father was an intellectually active man, who was particularly interested in science. Thus, Gotthard Neumann's family was closely connected to the region of Weimar and the state of Thuringia.

Already during his high school years, Neumann was interested in archaeology and went to the privately hold lectures of the archaeologist Wilhelm Dörpfeld in Jena, whose excavations in Olympia and Troy became very famous.[41] Neumann studied prehistory, history, auxiliary science of history, and German philology in Jena, Munich, and Marburg. Besides of that he was also interested in classical archaeology, art history, diluvia geology, physical anthropology, philosophy, religious studies, and ecclesiastical history. Neumann studied with scholars who predominantly advocated a positivistic scientific practice in terms of collecting and categorizing artifacts and human remnants, such as Gustav Eichhorn in Jena, the geologist Ferdinand Birkner, and the physical anthropologist Rudolf Martin in Munich. Particularly Eichhorn, who was the head of the prehistoric institute at Jena University, was very influential on Neumann,[42] and from the prehistoric archaeologist Walter Bremer in Marburg, Neumann learned to connect academic and applied science, archaeology and conservation.[43]

41 UAJ, D 3194: Personal file Prof. Dr. phil. Neumann, Gotthard, Curriculum vitae Gotthard Neumann, dated 1 October 1938, fol. 2–4; Gotthard, Curriculum vitae Gotthard Neumann, dated 29 August 1953; BAR, R 4901/13272, fol. 147, no. 6919.

42 See Grabolle, Hoßfeld, and Schmidt 2003, 871 and 877–878; Peschel 2010, 70. See Birkner 1913; Proctor 1988, 142.

43 Schuchhardt, Jacobsthal, and Macalister 1926, 283. Also see Klüssendorf 1999.

In December 1926, Neumann graduated at the University of Marburg in prehistoric archaeology with Gustav Behrens and Paul Jacobsthal who rated his dissertation as "very good" and "excellent."[44] Shortly after graduation he became first a volunteer, then assistant at the State Museum of Mineralogy, Geology, and Prehistory (*Staatliches Museum für Mineralogie, Geologie und Vorgeschichte*) in Dresden.[45] This trajectory shows that Neumann was interested in the development of a purely scientific approach to analyze prehistoric artifacts and that he was a practitioner who preferred applied science to theory. This kind of scientific practice was very common in prehistoric archaeology. Due to the lack of academic institutes, scholars such as Martin Jahn and Walther Schulz had to prove the relevance of their research field for the public by working on excavations, in museums, and monuments offices, proving that archaeological research mattered for society.[46]

The majority of Neumann's professors in history and philology represented the type of the 'German mandarin' (Fritz K. Ringer). They usually advocated a national-conservative attitude, rejected the political system and the culture of Weimar Republic, and welcomed the authoritarian order the Nazis propagated. *Geist* (spirit) and *Kultur* (culture) were the most important elements of their habitus.[47] Yet, some of Neumann's teachers were more than national-conservative. For example, the Marburg historian Edmund Ernst Stengel advocated an anti-Semitic mindset and later supported the Nazis,[48] and the philosophers Bruno Bauch and Max Wundt propagated strongly *völkisch*-nationalist and anti-Semitic ideas. For them, the Jews were *fremdvölkisch* (belonging to a different *Volk*) and could never be considered as German.[49] For others, such as Karl Helm, who taught *Altgermanische Religionsgeschichte* (history of the old-Germanic religion), and the philologist Hans Naumann, *völkisch* ideas such as the 'Germanic spiritual world' (*germanische Geisteswelt*) were at the core of their intellectual agenda.[50] This mixture between a positivistic epistemic practice in archaeology applying 'objective' methods and a conservative, *völkisch* and anti-Semitic mindset was crucial for Neumann's scientific habitus.

Neumann, as many other prehistoric archaeologists, such as Herbert Jankuhn or Hans Reinerth, belonged to a generation that was "too young of having been drafted into the German military, and too old of having experienced the Great War as a distanced event."[51] He belonged to the 'war youth generation,' whose members were born between 1900–1910. The members of this generation often advocated right-radical or neo-conservative ideas, because they had lost confidence into the bourgeois values of their fathers and often experienced material poverty as a result of World War I.[52] This

44 UAJ, D 3194: Personal file Prof. Dr. phil. Neumann, Gotthard, Curriculum vitae Gotthard Neumann, dated 1 October 1938, fol. 2–4.
45 Grabolle, Hoßfeld, and Schmidt 2003, 877.
46 Mehrtens 2006, 317.
47 See Gottwald 2003, 913–914; Pöthe 2003.
48 See Grundmann 1968; Klee 2005, 601.
49 Sluga 1993, 84, 94 and 112–118.
50 Pöthe 2003, 851.
51 Wildt 2005, 172.
52 Herbert 1996, 43; Wildt 2002, 46–52.

was also a reason why Neumann concentrated on practical archaeology; he had to work on excavations and in the monuments office in Hesse as preservationist when he studied in Marburg in order to finance his studies.[53] One of the main characteristics of this generation was the longing for a spiritual interpretation and experience "of the big picture, of the *völkisch* and social problems"[54]. For Neumann, this aspect was crucial, and he interpreted and experienced the "big picture" by researching the culture of the past in his *Heimat* (habitat) Thuringia. The first article Neumann published during his last year as a student demonstrates this mindset. In this article, he discussed the influences of modern technology on the German people in the manner of German *Kulturkritik*. He thought that one should find counterweights to technology and velocity of life which dominated Weimar Republic. People should "bethink themselves of the particular elements of our being and how we came to be as that, they should collect and herd these elements as a holy legacy of simpler but richer times." According to Neumann, this was the only way to "ground the own emotional life on the basis of a real insight into the *Volk*", which one could attain by a "yearlong examination of the landscape of the *Heimat*, nature, prehistory, history, custom and art, in short, of our complete *Heimat*-culture"[55]. Thus, Neumann both welcomed modern technology and modern scientific methods and advocated *völkisch* and anti-modern ideas at the same time.[56]

3.2 Neumann's ethnic-*völkisch* concept

The first extended scientific article based on his dissertation Neumann published in 1929 in the *Praehistorische Zeitschrift* (Journal for Prehistory) on the *The Classification of the Bell Beaker Culture*.[57] In this article, Neumann proposed a reordering of the cultural-chronological system of Thuringian prehistoric findings. The main concept in his publications from 1928–1932 was *Kultur* (culture). *Kultur* meant foremost material culture, but from the beginning this concept implied an ethnic idea: material culture always derived from and thus referred to a certain ethnic group. Neumann presented himself as a scholar in the tradition of Kossinna. Nonetheless, he was trying to modify Kossinna's Nordic interpretation.[58] Thus, he agreed with Kossinna's ethnic approach in general but disagreed with the assumption that European high culture must have originated in northern Europe in prehistoric times. In contrast to Kossinna, Neumann did not deduce his theoretical assumptions from linguistic methods;[59] rather, he concentrated only on material culture. Advocating the assumption of an inherent connection between

53 UAJ, D 3194: Personal file Prof. Dr. phil. Neumann, Gotthard, Curriculum vitae Gotthard Neumann, dated 1 October 1938, fol. 2–4.
54 Herbert 1991, 116–117. My translation.
55 Neumann 1926. My translation.
56 For this kind of mindset see Herf 1984.
57 Neumann 1929; see Neumann 1930b.
58 Neumann 1930b, 45; see Neumann 1928.
59 Andresen 2004, 99.

ethnic groups and material objects, Neumann viewed *Kultur* as the link between material culture and ethnicity. Neumann, following Kossinna, called the particular human-biological entity *Gruppe* (group), *Volk*, or *Volksgruppe*.[60] The connection between an ethnic *Gruppe* and archaeological objects led to the characterization of peoples through an analysis of types of objects, and vice versa. To these two elements Neumann added a third aspect, which he did not explicitly use as a term: namely, space, which is very obvious in his usage of techniques such as cartography. This relates to Kossinna's approach of so-called settlement archaeology, the core concept of which was that the history of ancient tribes or *völkisch* groups was a sequence of migrations.[61] Therein, maps were an important tool to show the migrations of the *Völker*. Thus, for Neumann, a certain material *Kultur* referred to a *Gruppe* that could be examined by researching archaeological objects in a specific space. This space was the settlement area of the group, which could be limited by the characteristics of archaeological objects; differences between the forms of those objects and other ones correlated to different ethnic groups. Therefore, the diffusion of material objects enabled Neumann to analyze the migration of those groups.

The core concept in Neumann's approach was ethnicity. He defined ethnicity not only by material objects, but also by time: "Some centuries later, probably in Slavic time, the mound was broken up and the precious part of its content was robbed"[62]. In this example, a certain ancient period is ethnicized by referring to the term *slawisch* (Slavic). But Neumann took a critical approach to the ethnic concept in prehistory. For example, he rejected his colleague Werner Radig's simplistic associations of archaeological things with ethnic categorizations. According to Neumann, Radig neglected the fact that Slavic ceramics could also have been used by German settlers, and, therefore, the connection between archaeological artifact and ethnicity was problematic for him.[63] Indeed, Neumann wanted to use only "streng wissenschaftliche" (strictly scientific) criteria for his ethnic constructions. And, in contrast to colleagues such as Hans Reinerth or Herbert Jankuhn,[64] Neumann did not apply the Nordic idea.

At the same time, Neumann used methods from physical anthropology and racial theory. According to him, the *mitteldeutschen Kulturgruppen* (middle German culture groups) belonged to branches of the *kurzköpfigen Glockenbechervolkes* (short-capped bell beaker people).[65] As many scholars of the Kossinna group,[66] Neumann connected racial categories with his semantic net of *Gruppe*, *Kultur*, and archaeological objects. It is important for understanding this early period of Neumann's trajectory that he – contrary

60 See Brather 2004, 65.
61 See Andresen 2004, 95–99.
62 Neumann 1930c.
63 Neumann 1930a.
64 See Reinerth 1925, 19; see Jankuhn 1941/42.
65 Neumann 1929, 36.
66 See Kossinna 1936.

to Kossinna – hardly used the term 'race' concerning physical anthropology, but preferred *Gruppe* and *Volk*. In addition, Neumann used only strictly scientific language and terminology in his publications and avoided any nationalistic-chauvinistic terms. Neumann's and Kossinna's works differ not only in their use of language, but also in their understanding of certain concepts. For Neumann, 'race' was a genuinely scientific sort of umbrella category which had to be differentiated by *Völker* and *Gruppen*. For Kossinna, 'race' was equivalent to the Indo-Germanics, who were for him the bearers of biological and cultural superiority. In Kossinna's scientific thinking 'race' appears as a key concept.[67]

In summary, there were five major differences between Neumann's ethnic-*völkisch* concepts and those of Kossinna and his school. First, for Neumann, the Nordic theory was not central to his research. Second, ethnic constructions were only valuable if he could prove them by a critical examination of the connection between people and material culture and not by a priori assumptions. Third, Neumann had a genuine material culture-based approach and was not influenced by linguistic methods. Fourth, even though Neumann used the term race, he used it not as a major concept but as a category among many others. Fifth, he did not link *Volk* and race with nationalistic thoughts.

3.3 Neumann's trajectory and the institutional situation of prehistory at the University of Jena

In the late 1920s, Neumann decided to vote for *völkisch* political groups and, around 1930, for the NSDAP.[68] It is difficult to state whether his voting choice resulted from a radicalization of his political mindset during his student years – in the early 1920s, he preferred the *Deutsche Volkspartei* (German People's Party), in which his father had been active – or a strategic or opportunistic one. Whichever the case, it is necessary to emphasize two circumstances that contextualize this shift. First, the NSDAP in Thuringia constituted the first Nazi government in Germany in the early 1930s;[69] second, Neumann, being a native Thuringian, was very much attached to the native soil as his *Heimat*.[70] Neumann had negotiated with government officials in Thuringia before 1930 to get a position at the Germanic museum in Jena. Since the museum's director Gustav Eichhorn had died in 1929,[71] Neumann's decision to vote for the Nazi party appeared to be profitable: with the help of the Nazi minister of Thuringia, Wilhelm Frick, Neumann, at the age of twenty-eight, was appointed the head of the Germanic Museum in Jena and assistant professor in the Department of History at the University of Jena in

67 Andresen 2004, 103.
68 Grabolle, Hoßfeld, and Schmidt 2003, 879.
69 Fleischhauer 2010, 63–67.
70 Peschel 2010, 71.
71 See Peschel 2010, 69–71.

November 1930.[72] Neumann's first career step was, therefore, at least partly the result of a heteronomous strategy.

This heteronomous strategy becomes more obvious later. After 1930, Neumann accumulated more and more social capital due to his interaction with the Nazi party. He became a member in Alfred Rosenberg's Battle League for German Culture (*Kampfbund für deutsche Kultur*)[73] and in its sub-organization, the Reich League for German Prehistory (*Reichsbund für deutsche Vorgeschichte*),[74] in which he took a position as a regional leader of Thuringia.[75] With the support of Fritz Sauckel, the later *Reichsstatthalter* (governor of the *Reich* in Thuringia), Neumann was able to establish the first supra-regional excavation law in Thuringia in 1932.[76] In Neumann's case, couplings between science and politics were particularly promising before the so-called seizure of power by the Nazis. However, enforcing one's position in the scientific field with the help of politicians was equal to fighting for power in the field with 'illegitimate weapons.'

Neumann was thereby only one example of many German prehistoric archaeologists: similar cases include his East German colleagues Martin Jahn and Walther Schulz, or Hans Reinerth, Herbert Jankuhn, Kurt Tackenberg, and Bolko von Richthofen, all of whom also engaged in Nazi politics. Most of them were Kossinna scholars. Kossinna and his disciple Reinerth, for example, were more extreme than Neumann because they engaged more actively in Nazi cultural politics; Kossinna became one of the founding members of Alfred Rosenberg's Battle League.[77] German prehistory scholars considered Nazi politicians, such as Rosenberg or Heinrich Himmler, and their worldviews as a resource for advancing the academic establishment of prehistory. For Nazi-ideologists, reciprocally, prehistory offered scientific authority to their racist and *völkisch* ideology. As mentioned above, prehistory around 1933 was still in the process of achieving scientific autonomy; its scholars had to fight for acceptance and authority in the scientific field, which was dominated by the canonical disciplines.[78] Although university institutes, museums, and journals in prehistory were being developed, they had not yet been completely established. In 1929, only six ordinary and extraordinary chairs of prehistory

72 UAJ, D 3194: Personal file Prof. Dr. phil. Neumann, Gotthard, Curriculum vitae Gotthard Neumann, dated 1 October 1938; ThHStAW, Thüringisches Volksbildungsministerium, no. 21858, fol. 3: Letter dated 22 October 1930, Thüringisches Volksbildungsministerium to Gotthard Neumann. Also see BAR, R 4901/13272, p. 147.

73 Since 1934: *NS-Kulturgemeinde* (Nazi Culture Community).

74 UAJ, D 3194: Personalfragebogen Gotthard Neumann, dated 31 July 1934; APM/Akten Reichsbund: Fragebogen Deutsche Vorgeschichtsforscher u. Vorgeschichtsfreunde, dated 16 Sepember 1935.

75 ThHStAW, Der Reichsstatthalter in Thüringen, no. 440, fol. 78–79: Letter dated 4 August 1937, NSDAP-Gauleitung Thüringen to Reichsstatthalter in Thüringen. Also see APM/Korrespondenz Reinerth: Letter dated 24 March 1933, Hans Reinerth to Gotthard Neumann; NL Neumann, file "Kyffhäuser – Grabungen und Werk": Letter dated 3 July 1933, Hans Reinerth to Gotthard Neumann.

76 Neumann 1932, 192.

77 Grünert 2002, 308–309 and 317–318; see Bollmus 2002.

78 Wiwjorra 2002, 82.

at German and Austrian universities had been founded. The most important institutions for archaeology, such as the Institute of Archaeology of the German *Reich* (*Archäologisches Institut des Deutschen Reichs*)[79] or the Roman-Germanic Commission *(Römisch-Germanische Kommission)*, were predominantly managed by scholars of the classics who occupied the powerful positions in the archaeological disciplines.[80] Prehistory's situation would change profoundly after 1933/34. By 1942, prehistory was well established at German universities. In the early 1940s, twenty-five chairs at universities existed, of which seventeen were full professorships.[81] This extraordinary success was only possible through the exchange of sorts of capital between prehistoric archaeologists and Nazi politicians.[82]

On a micro-scale, this development becomes obvious regarding Gotthard Neumann's career, which took off after 1933. In 1934, he became the *Staatliche Vertrauensmann für vor- und frühgeschichtliche Bodenaltertümer* (State Representative of Prehistoric Relics)[83] under the sponsorship of Fritz Sauckel. Although he had done this job since 1932 voluntarily, he now received a salary. In 1935, Neumann received the position of the first curator of the Museum of Prehistory in Weimar.[84] The preliminary peak of Neumann's career was marked by the offer of the official extraordinary professorship at Jena University in the same year,[85] even without having finished his second thesis (*Habilitationsschrift*), and the establishment of the prehistory journal *Der Spatenforscher* (The Spade Researcher) in 1936 with the support of Wilhelm Frick[86] as well as of a new scientific monographic series called *Irmin* in 1939.[87] These instances of the consecration of archaeological knowledge were important steps toward the status of prehistory as an academic discipline at Jena University.

One detail concerning this development is very important: in the early 1930s, when prehistory was not yet fully institutionalized at German universities and scholars de-

79 Later *Deutsches Archäologisches Institut* (German Archaeological Institute).

80 Altekamp 2008; Junker 2001, 505–506.

81 Grabolle, Hoßfeld, and Schmidt 2003, 868.

82 See Bollmus 2002, 37; Pape 2002, 168.

83 BAR, R 4901/13272, fol. 6919.

84 ThHStAW, Thüringisches Volksbildungsministerium, No. 21858, fol. 6: Letter dated 19 September 1932, Thüringisches Volksbildungsministerium to Gotthard Neumann. Also see UAJ, D, 3194: Letter dated 23 May 1935, Thüringisches Ministerium für Volksbildung to Gotthard Neumann; ThHStAW, Thüringisches Volksbildungsministerium, no. 21858, fol. 35: Letter dated 18 February 1935, G. Neumann to Thüringischer Volksbildungsminister; UAJ, D 3194: Personal file Prof. Dr. phil. Neumann, Gotthard, Curriculum vitae Gotthard Neumann, dated 1 October 1938, 2–4; BAR, R 4901/13272, fol. 6919.

85 ThHStAW, Thüringisches Volksbildungsministerium, no. 21858, fol. 16: Beschluss des Thüringischen Staatsministeriums, dated 10. April 1934; fol. 35: Letter dated 18 February 1935, Gotthard Neumann to the Thüringischen Volksbildungsminister; UAJ, D 3194: Letter dated 12 June 1934, Fritz Sauckel, Der Reichsstatthalter in Thüringen; ThHStAW, Der Reichsstatthalter in Thüringen, no. 440, fol. 94: Letter dated 2 February 1937, Fritz Sauckel to Gotthard Neumann.

86 UAJ, C, 799, fol. 152: Letter dated 24 February 1939, Gotthard Neumann to the Thüringische Minister für Volksbildung, F. Stier.

87 UAJ, D 3194: Personal file Prof. Dr. phil. Neumann, Gotthard, Curriculum vitae Gotthard Neumann, dated 1 October 1938, 2–4.

cided to implement a heteronomous strategy to obtain more authority in the scientific field, prehistory had not yet attained an entirely symbolic autonomy. Thus, when scholars imported sorts of capital from the Nazis, they could not transform it completely into the specific scientific language and rules of prehistory, because these components did not yet exist. This circumstance led to a more or less direct import of elements from Nazi ideology into the scientific concepts of German prehistoric archaeologists.[88]

3.4 Conceptual developments and semantic changes, 1933–1945

After 1933, there was, first, a change concerning the place of Neumann's publications. Whereas he had published his first articles in journals with high credibility in the scientific field, he now started to publish in popular cultural and in Nazi journals.[89] He followed exactly the demand that Nazi science and culture politicians imposed upon the scientific field: namely, that scientific research should be addressed more to ordinary Germans than to academics.[90] Prehistoric archaeologists such as Neumann considered this demand as a chance to popularize the contents of prehistory in order to create social relevance. This stance was another heteronomous element in the strategy of Neumann and his fellow colleagues, because they did not follow the rules of the scientific field but those of the field of power.

As the Nazis preferred the racial and Germanocentric idea of German prehistory, Neumann began to treat the *Germanenfrage* (Germanic question) more frequently than he had before 1933[91] and thereby imported more and more ideas and concepts from the political field into prehistory. In 1934, he spoke about the "politische Karte Thüringens" (political map of Thuringia) in prehistoric times.[92] He created an analogy between the prehistoric periods and the contemporary Third Reich. A new term in Neumann's constructions was *Stamm* (clan, tribe),[93] which was an ethnic category between the terms *Gruppe* and *Volk*. In the same context, he wrote about "das gute germanische Blut" (the good Germanic blood), which was used together with the term *deutsches Reich* (German *Reich*).[94] Thus, Neumann changed his categories from *Kultur* and *Gruppe* to the semantic net of *Blut*, *germanisch*, *deutsch*, *Reich*, *Stamm*, *Volk*. *Blut*, *germanisch*, *deutsch*, *Stamm*, and *Volk* were ethnic attributions; the terms *Reich* and in addition *Boden* were spatial representations of these ethnic categories. Combining both referred to the blood-and-soil-ideology of Nazi ideologues like Richard Walter Darré. This development does not mean that Neumann abandoned the terms *Kultur* and *Kulturgeschichte* (cultural history); rather, they had become more and more interchangeable with more biological terms.[95]

88 See Bourdieu 1998, 19.
89 Neumann 1933a; Neumann 1933c; Neumann 1934b; Neumann 1934a; Neumann 1935c.
90 Grüttner 2000, 565.
91 See Neumann 1933b, 1.
92 Neumann 1934c, 12.
93 Neumann 1934c, 1.
94 Neumann 1934c, 2.
95 Neumann 1933a, 10.

This process of a biologization of cultural history was linked to an intensified use of racial concepts that had become virulent since the establishment of the Nazi regime, which was another demand Nazi science politicians placed on the scientific field: namely, to focus more on the 'racial question.' The semantic change here was that Neumann now combined the term *Rasse* with *nordisch* (Nordic) and *indogermanisch* (Indo-Germanic), which became synonymous with Aryan. From an ethnically unspecified focus, Neumann started to concentrate more on the 'Nordic race,' which was the *Germanen*.[96] It was exactly for this concept that Neumann had criticized Kossinna some years before.

It is noteworthy that Neumann was unable to create a very consistent scientific theory in the sense of a Nordic 'racial' conceptualization. He remained biased in this respect. Yet, in 1935, he gave a speech on the occasion of the celebration of Jena University, in which he praised Nazi rule and took a stance against the Germanocentric prehistoric archaeologists at the same time.[97] The problem of the Celts, for example, who many prehistoric archaeologists considered to be non-Germanic, Neumann solved by integrating them into the category of Indo-Germanic *Volk*.[98] This assumption differed from the knowledge constructions of other prehistoric archaeologists. In contrast to researchers such as Jankuhn, von Richthofen, or Reinerth, Neumann did not want to elevate the Germanics above than the Celts; rather, he intended to develop a scientific concept for a *völkisch* categorization of ethnicity in prehistory beyond a Germanocentric classification.[99] He treated the Slavs slightly differently than the Celts. According to Neumann's argumentation, the Slavs were also originally an Indo-German people, but their *Volkstum* (nationhood, folklore) degenerated and became inferior to that of the early *Germanen* and the Germans in a later period of ethnic differentiation. In the course of the 1930s, Neumann increasingly considered the Slavs to be an inferior *Volk* by characterizing their archaeological remnants as the products of their inferior culture.[100] According to him, only German influence could bring a higher developed material culture to the Eastern regions.[101] The evaluation of certain ethnic groups as superior or inferior was definitely a new element in Neumann's publications.

Whereas Neumann retained more or less stringent *völkisch*-ethnic concepts for the analysis of ancient cultures during the Nazi regime, the exchange of sorts of capital with the Nazi politicians led to the import of more and more political problems into the research questions of prehistory, because Neumann was unable to transform his mobilized kinds of capital from the political field completely into scientific authority and credibility. Both thought structures intertwined as time went on. As a result, Neumann treated political questions by scientific means: "the clarification of all questions relating to the recovery of the Germanic East after the Slavic flood from the seventh until the

96 Neumann 1934b, 14.
97 Neumann 1935b, 5–9.
98 Neumann 1934b, 22.
99 Neumann 1935a, 143.
100 Neumann 1935b, 141.
101 Neumann 1935d, 142.

ninth century A.D. is the foremost purpose of German historic and prehistoric research for national political reasons."[102]

At the same time that Neumann imported ideas from the political field, he conceptually consolidated his earlier approaches for the analysis of archaeological findings through ethnicity. In 1936, Neumann published a short programmatic article, in which he presented the future order of archaeological findings in the Jena Museum. Neither typological nor chronological criteria were conducive anymore; a "biological and historical key" was central now. Archaeological objects in the collections were to be ordered along the following categories: "1. The cultures of the Neanderthal and *Loess*-races (Paleolithic Age). 2. The cultures of pre-Indo-Germanics ([*Indogermanen*] Middle Stone Age). […] 7. The cultures of the Celts (Bronze, Iron, Latène Age: Southern culture circle). The cultures of the Germanics […]."[103] Neumann's reordering of the archaeological knowledge system suggested that there was a teleological line from the primeval *Volk*, the Indo-Germanic *Urvolk* (primeval people), to the *germanisch-deutsche* (Germanic-German) ethnic group that was thought to have developed in the twelfth century A.D.[104] Neumann established a biologization of cultures and historic periods. He substantiated this new order by arguing that the old chronological ordering of archaeological objects was wrong because it implied the same kind of cultural development for every ancient culture in Europe and the Near East, an idea which was rooted in Enlightenment thought. According to Neumann, this meant a generalization of the cultural development of all archaeological cultures, which signified for him a kind of internationalism of scientific knowledge.[105] Rather, he assumed that every culture or ethnic group should be analyzed from the standpoint of its own specificities and spatial origins.[106] Similarly, right-wing scholars, such as the philosopher and sociologist Hans Freyer or the legal scholar Carl Schmitt, assumed that knowledge had no validity on an international level. Rather, they claimed that 'particular norms' were significant only for particular peoples, because these norms developed from the particular space in which these peoples were living. Thus, for Germans, only German law was valuable and not the principles of Roman law. Likewise, for Germans in the contemporary Third Reich, only the norms coming from German soil should count, such as the supposedly genuine German form of social organization, the *Volksgemeinschaft*; liberal values, universal ethics, or human rights were illegitimate ideas for the ordering of German society, because they had originated in French and English Enlightenment culture.[107] Like Neumann's assumption, this theory found its equivalents in Nazi ideology such as Rosenberg's idea that there was no law

102 Neumann 1935d, 140. My translation.
103 Neumann 1936, 44. My translation.
104 Neumann 1934a.
105 Neumann 1939, 134.
106 Neumann 1939, 135.
107 Muller 1987, 29–30.

as such, but only laws that had originated from a 'particular *völkisch* order.' The ideological principles of National Socialism, such as the creation of an ethnocracy by 'racial' categorizations and imperial nationalism, could be such 'particular laws.'[108]

In January 1941, Neumann had to serve in the Wehrmacht in Ukraine. During this time, he radicalized his concepts toward open anti-Semitic racism. As a soldier in the signal corps, he found the time to do some small excavations[109] and cooperated with archaeologists who 'saved' (i.e., stole) cultural artifacts on Rosenberg's order.[110] Neumann reported his scientific discoveries in two articles. There he characterized the local people as 'racially' inferior to the Germans in the sense of an underdeveloped culture. He linked this construction with an anti-Semitism that had not been of interest to him until that moment. Expressions such as *jüdisches Unwesen* (Jewish shallowness)[111] were linked with *fremd* (strange), whereas the contradictory semantic bundle was *organisch* (organic) and *Boden*. Accordingly, he characterized Jews as inorganic and not rooted in the soil; they were a threat to the local people and to the Germans. In Neumann's eyes, Jews were not represented in archaeological cultures because they had been strangers to East European soil from the beginning of their settlement in those regions.[112] This anti-Semitic semantic became highly virulent regarding the war of extermination in the East, directed by the SS and the Wehrmacht. Neumann's scientific and political constructions were crucial for the question about which kind of "order semantic,"[113] and *völkisch*-racist thinking was the discursive core of the mindsets of German intellectuals and academics toward genocide and the legitimization of the Holocaust, even though Neumann had never been involved in any genocide activities.[114] There is no doubt that Neumann was not alone in this mindset, but there is also no doubt that other intellectuals who were involved in the war in the East and who had a mentality and a position in the Wehrmacht similar to Neumann's recovered their sense of human rights during the war of extermination in the East,[115] whereas Neumann radicalized his 'racial' and political categories.

3.5 Neumann's career until 1945

Let me summarize briefly what has been said so far. From the beginning of his career, Neumann subscribed to an ethnic concept for the analysis of archaeological objects. This was a *völkisch*-ethnic epistemology, in which the terms group and *Volk* were central. By exchanging kinds of capital with Nazi politicians in Thuringia, Neumann added elements

108 See Paxton 2005, 84.
109 Neumann 1941, 35–39.
110 Neumann 1942, 18; see Heuß 2000.
111 Neumann 1941, 39.
112 Neumann 1942, 18.
113 Raphael 2001; Raphael 2004.
114 Fritzsche and Hellbeck 2009, 337–339.
115 See Jarausch 2008, 36–42.

from *völkisch* and Nazi ideology to his thinking, a union which resulted from the non-autonomous situation of prehistory in the scientific field. This situation meant that Neumann's *völkisch*-ethnic approach adhered to a *völkisch*-ethnic-racial-Germanocentric concept, which he transformed into open racism during his service in the Wehrmacht. At the same time, Neumann still differed in his ideas from the hardliner Germanocentrics and from Nazi ideologues: for him, the Celts were originally an Indo-Germanic people, as were the Slavs, even though he believed that the latter had degenerated in the following centuries.

It is difficult to say to what extent this difference of semantics caused Neumann's career to come to a standstill in the late 1930s. Unlike other prehistoric archaeologists, he did not obtain a full professorship until 1945, when his position was finally converted into a full chair after several trials by the principals of Jena University, Abraham Esau and Karl Astel.[116] As Neumann was serving in the military on the Eastern Front at that moment,[117] the conversion did not take effect. Altogether, one may also ask why Neumann had to perform military service from 1941 until the end of World War II and remained at the rank of a constable,[118] a military career that was not very common for university professors, who usually reached higher ranks.[119]

There were two reasons for Neumann's failure to accumulate successfully more social capital. First, concerning Neumann's engagement with Nazi organizations, one notices that there was a certain lack of enthusiasm on Neumann's part. To be sure, Neumann was a member of the Nazi League of Old Fellows (*NS-Altherrenbund*) and the Nazi League of Teachers (*Nationalsozialistischer Lehrerbund*),[120] and he was also a Patronizing Member of the SS (*Förderndes Mitglied der SS*).[121] However, he did not engage actively in more important Nazi organizations that would have provided him with a certain amount of social capital, which for academics were primarily the SS and, in the first years of the regime, the Nazi League of University Teachers (*Nationalsozialistischer Deutscher Dozentenbund*). Today, it is not possible to determine whether Neumann had become a

116 ThHStAW, Thüringisches Volksbildungsministerium, no. 21858, fol. 47: Letter dated 11 February 1939, Prof. Porzig, dean of the Faculty of Philosophy at Friedrich-Schiller-Universität Jena, to Thüringischer Minister für Volksbildung. Also see Vorschlag zur Ernennung des planmässigen ao. Professors Dr. phil. Gotthard Neumann zum ordentlichen Professor in der Reichsbesoldungsgruppe H1b oder der ihre entsprechenden Landesbesoldungsgruppe. An den Herrn Staatsminister und Chef der Reichskanzlei des Führers und Reichskanzlers/an die Ministerialgeschäftsstelle bei der Universität Jena, gez. Knopp, dated 24 June 1944. Also see Letter, dated 10 August 1944, the principal of the Friedrich-Schiller-Universität Jena to Thüringischer Minister für Volksbildung.

117 UAJ, D 3194: Personal file G. Neumann, Personalakte, no. 1100.

118 Grabolle, Hoßfeld, and Schmidt 2003, 881.

119 See K. J. Arnold 2008, 57–59.

120 APM/Akten Reichsbund: Fragebogen Deutsche Vorgeschichtsforscher u. Vorgeschichtsfreunde, dated 16 September 1935.

121 ThHStAW, Der Reichsstatthalter in Thüringen, no. 440, fol. 78–79: Letter dated 4 August 1937, NSDAP-Gauleitung Thüringen to Reichsstatthalter in Thüringen.

member of the NSDAP or not; we only know that he applied for membership.[122] Contrary to Neumann, more ingrained Nazi-scholars such as the East German prehistoric archaeologist Walther Schulz became members of the SA and the NSDAP.[123] Furthermore, Neumann had some conflicts with important Nazi politicians, such as Robert Ley, who wanted to create a student hostel out of Neumann's institute in 1941,[124] and Wilhelm Reinhard, the chief of the veteran league Nazi Soldiers' League '*Kyffhäuser*', with whom Neumann had to cooperate because Reinhard financed a huge excavation project in Thuringia.[125]

Second, there was a change in Nazi science policy in the late 1930s, which was related to the establishment of Hermann Göring's 'Four Year Plan' in 1936. While Nazis politicians after 1933 initially encouraged professors and junior scholars who were confirmed Nazis or young researchers who focused on the *Germanenfrage*, the direct preparation for war after 1936 demanded different kinds of expertise, namely, that of the 'hard sciences,' such as armament technology or agronomics.[126] Prehistory undoubtedly worked as part of the cultural war policy of the Nazis by rescuing, stealing, and researching archaeological objects in occupied countries. But prehistoric archaeologists did not produce any goods that could be directly used for warfare. Neumann probably found himself in a similar situation; his research was not considered important enough to be boosted during the war.

3.6 Neumann's trajectory and semantic transitions, 1945–1972

Neumann was released from Jena University in late 1945.[127] His minor position and lack of support in the scientific field were important reasons for his release. In addition, he had relations to the social group surrounding Hans Reinerth, Kossinna's most famous acolyte, whom the scientific community after 1945 characterized as *the* Nazi archaeologist.[128] Of course, finding a scapegoat was a post-1945 strategy of those archaeologists who held more powerful positions in the field than Reinerth and others. After some years of exclusion from the scientific field, Neumann again became an assistant professor at the Institute for Prehistory in Jena and, finally, received a full professorship in 1953.[129] Neumann's trajectory was very similar to that of his older colleague in the GDR Walther Schulz, who received a position at the University of Halle in the same

122 Grabolle, Hoßfeld, and Schmidt 2003, 879; for another opinion see R. Müller 2001, 95.

123 See Eberle 2002, 38–39, 98, 117.

124 Grabolle, Hoßfeld, and Schmidt 2003, 879; for another opinion see R. Müller 2001, 95.

125 NL Neumann, file "Kyffhäuser – Grabungen und Werk": Letter dated 17 January 1943, Gotthard Neumann to Captain a. D. von Schlick. See Aly and Heim 1991, 50–68.

126 Flachowsky 2008, 232.

127 UAJ, D 3194: Friedrich-Schiller-Universität (Prof. Dr. Zucker, Dr. Bense) to G. Neumann (not dated).

128 See Halle 2002, 22–36.

129 R. Müller 2001, 95.

year, after he had been disqualified from academia.[130] In the end, Neumann received exactly the position he wanted to achieve since his student years, even though he was excluded from academia a couple of years. In the GDR, he became a member of the *Liberal-Demokratischen Partei Deutschlands* (Liberal-Democratic Party of German) and engaged in the evangelical church of Thuringia.[131] It was very common for scholars and scientists in East, but even more so in West Germany, to appeal supposedly Christian-humanistic and non-political values after 1945.[132]

This repositioning in academia in the then established GDR required specific semantic and epistemic transitions of the former *völkisch* and 'racial' knowledge order. Neumann continued to publish scientific articles in the early 1950s. Phrases such as "das landfremde Volk der Glockenbecherleute" (the bell beaker people being foreign to the soil) or the continual usage of the term *Gruppe* indicated a re-combination of older terms and the addition of new ones.[133] Whereas soil, ethnicity, and archaeological objects still shaped a semantic bundle, 'racial' concepts, above all the Nordic theory, or methods from physical anthropology were not in use anymore. This becomes most obvious in the usage of the word *Leute* (people, without racial connotation), which Neumann added to *Volk* and *Gruppe*. It was possible for Neumann to combine terms such as *Volk* and *Volksgemeinschaft* with the doctrine of the German Democratic Republic (GDR), whereas 'racial' categories were unacceptable. Thus, he removed biological and racial elements from his prehistoric scholarship through a reinvention of the term *Kultur* in the period after 1945, and – parallel to that – through an enforced shift of his focus from Germanics to Celts and Slavs. This shift does not mean that Neumann only revitalized his terminology and conceptualization from the 1920s. Instead, the ethnic categories in Neumann's concept of prehistory were transformed after 1945; the term combination *deutschmittelalterliche Scherben* (German-medieval ceramics)[134] shows that the ethnic term *deutsch* was still semantically linked with *mittelalterlich* (medieval) as a historic period. Therefore, the linkage between ethnic and material culture and historic periodization that could be converted into a materialistic-Marxist approach endured beyond the end of the Nazi regime. Exactly the same conceptual transitions and transformations are to find in works of other East German scholars who had made career during the Nazi regime and continued their academic trajectory in the GDR, such as Martin Jahn, who focused on the concept of *Kulturgruppen* (culture groups) in the 1950s.[135]

130 Eberle 2002, 390.
131 Vgl. R. Müller 2001, 106.
132 See Rabinbach 2003.
133 Neumann 1954, 8.
134 Neumann 1957, 32.
135 Jahn 1952.

4 Conclusion: The *völkisch* thought and the establishment of German prehistory

The *völkisch*-ethnic concept in German prehistoric archaeology, originating in the second half of the nineteenth century, cannot be considered as "pseudo-science," as Bettina Arnold proposed.[136] Instead it had the following functions in German prehistory: First, in the period from the late nineteenth century to the 1920s, it served as a tool of distinction. The *völkisch*-ethnic concept – the linkage of the ethnic terms *Volk*, *Gruppe*, and *Rasse* with geographical and temporal categories – was an element of the identity of this emerging discipline, drawing boundaries between it and others such as the classics. Ethnicity in prehistory appeared as an important element in achieving an autonomous position as a discipline in the scientific field. This does not mean that ethnicity did not play any role in the classics. After 1933 in particular, many scholars turned to the Nordic theory and connected the classic Greek culture with the *Germanenkultur* in middle Europe. Sparta, too, served as a model for legitimating historically the bellicose and racist social model of Nazi Germany.[137] But, in contrast to prehistory, ethnicity in terms of a biological 'racial' interpretation has never been a tool of symbolical distinction in the scientific field.

Second, the *völkisch* concept was closely related to political right-wing groups, with which the archaeologists surrounding Kossinna had close relationships. In the early 1930s, above all in Thuringia, the most promising of these parties was the NSDAP. Neumann chose from the beginning of his career a heteronomous strategy, using the right-wingers to attain a strong position in the scientific field that he could not achieve by fighting with the 'permitted weapons' of the scientific field. Because prehistory was not yet a symbolically autonomous and fully established discipline, prehistoric archaeologists such as Gotthard Neumann imported political problems, semantics, and terms into the language and the concepts of prehistory.

Third, for prehistory as a discipline, the exchange of kinds of capital between archaeologists and Nazi politicians was a success story. Through the predominantly heteronomous strategy adopted by scholars, prehistory was established in the 1930s and 1940s and held onto this position even after 1945 in the GDR and the FRG. This was only possible because prehistoric archaeologists developed their main epistemological principles, such as the ethnic concept, before the Nazis came to power. Therefore, they were able to adapt the main elements of the Nazi ideology as easily as they renounced Germanocentric and 'racial' categories after 1945. Even though Neumann lost his job as a professor at Jena University, he regained his position in 1953, having abandoned

136 See B. Arnold 2006.

137 See Altekamp 2008, 167–191.

völkisch-Germanocentric and 'racial' thoughts but having re-combined and transformed the *völkisch*-ethnic concept.

5 Archival Records and Abbreviations

APM
: Archiv des Pfahlbaumuseums Unteruhldingen/Bodensee: files "Korrespondenz Reinerth", "Akten Reichsbund".

BAR
: Bundesarchiv Berlin-Lichterfelde, Bundesarchiv, Koblenz: files "R 26/III 1, R 490/13272, R 8048/223, R 8048/315, R 4901/13272".

NL Neumann
: Nachlass von Gotthard Neumann (personal papers of Gotthard Neumann), verwaltet von Karl Peschel: file "Kyffhäuser – Grabungen und Werk".

ThHStAW
: Thüringisches Hauptstaatsarchiv Weimar: files "Thüringisches Volksbildungsministerium, no. 21858", "Der Reichsstatthalter in Thüringen, no. 440".

UAJ
: Universitätsarchiv Jena: files "BA 2055, D 3194".

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Serge Reubi

Why is the Dialogue so Difficult between the Historiography of the Social Sciences and the Historiography of Science?

Summary

In this paper, I observe that the historiographies of the social sciences differ sensibly from those of the sciences. I start by proposing a three-part typology of this specific development and then look for the origin of these separate historiographies. I test three groups of hypothesis: (a) the social sciences are so much different from the 'hard sciences' that it is impossible to understand them using concepts and methods which have mostly been developed within the historiography of the 'hard sciences'; (b) the second hypothesis assumes that the object changes less than the look at it: hence, sharing their object, it suggests that these historiographies differ because the identity and aims of the scholars who write them differ; (c) it is neither the object nor the historiographers which differ, but their relation.

Keywords: Historiography of social sciences; historiography of science; discipline; presentism; historicism; disciplinary function of history.

Im vorliegenden Aufsatz beobachte ich, dass sich die Geschichtsschreibung der Geistes- und Sozialwissenschaften unterscheidet von jener der Naturwissenschaften und schlage eine dreifach gegliederte Typologie dieser spezifischen Entwicklung vor. Im Anschluss frage ich nach den Ursprüngen der genannten Ansätze und untersuche drei Hypothesen: a) die Geistes- und Sozialwissenschaften unterscheiden sich so sehr von den ‚exakten' Wissenschaften, dass es unmöglich ist, sie mit Konzepten und Methoden verstehen zu wollen, die aus der Geschichtsschreibung der Naturwissenschaften hervorgegangen sind. b) die Untersuchungsobjekte ändern sich weniger als die Betrachtungsweisen. Nicht unterschiedliche Objekte sondern unterschiedliche Identitäten und Ziele der Historiker/innen bedingen also die abweichenden Geschichtsschreibungen. c) Weder die Untersuchungsobjekte noch die Historiker/innen in den beiden Bereichen differieren voneinander, sondern ihre je spezifische Beziehung zueinander ist ausschlaggebend für die profunden Unterschiede.

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The first paragraphs of Henrika Kuklick's introduction to her edited volume *A New History of Anthropology*[1] summarized several important problems of the contemporary historiography of the social sciences[2] and interestingly pointed out the specific path which it is following:

> This collection will appeal to a range of readers, anthropologists and historians prominent amongst them. For historians, the value of its essays will be their contextualization of anthropological ideas and practices in specific times and places. Anthropologists will find not only discussions of the discipline's major branches but also analyses of portions of its history that rarely feature in its oral tradition […]. The classic typology of historians of the human sciences is Stocking's, a dichotomous scheme of ideal types: "presentists" and "historicists". Presentists […] frame their accounts in contemporary terms, often seeking lessons from the past for the present: their tone may be celebratory, as they trace antecedents of ideas and methods now considered commendable, or mournful, regretting the loss of exemplary practices. Historicists […] are not explicitly concerned with contemporary standards and debates: they show that when we read old texts as if they had just been written, we frequently misunderstand their authors' intended meanings.[3]

For obvious reasons, the curious tone of these few introductory sentences strikes every scholar aware of the innovations which have revolutionized the history of science in the last thirty years. First, Kuklick stretches the value of 'contextualization' as if it was a

1 Kuklick 2008.

2 Interestingly, the English language offers no unanimously accepted word to designate the various disciplines which are united in the French *sciences humaines* or the German *Geisteswissenschaften*. As a matter of fact, in English, these disciplines are distributed in four, partly overlapping, categories: the social sciences, which comprise anthropology, sociology, archaeology, history, geography, linguistics, economics and psychology; the behavioral sciences, which are composed of psychology, anthropology, and the cognitive sciences; the humanities which consist of art, literature, history, linguistics and anthropology; the human sciences, which gather the cultural study of the human being (archaeology, anthropology, …) and the biological (medicine, physical anthropology). Hence most of the disciplines appear simultaneously in different categories, a fact which underlines the imprecision of the division: it is a convention, as Roger Smith puts it (Smith 1997; 4, 17; see also Smith 1999, Reubi to be published). Put shortly, the word is missing, and so, probably, does the concept – which may be, in part, an explanation for its curious historiography. In this paper, following Porter and Ross 2003 and against Kucklick, I will use the category "social sciences".

3 Kuklick 2008, 1.

new concept, yet, when her volume was published, David Bloor's principle of causality[4] had already been recognized, and questioned, for more than three decades. Second, she does not name Bloor's concept, even though it is most improbable that she is unaware of it, but rather uses the less specific word 'contextualization'. Last, she indicates that the question of contextualization constitutes a 'classic' problem for the historiography of the social sciences which has been identified by George Stocking who labelled the two possible postures of the historian of social sciences, presentism and historicism. Thus, in short, Kuklick's introduction does not only show that the historiography of the social sciences uses different concepts than the other historians of sciences; it also indicates that it does not fall within the scope of the chronology of what could be labelled the general historiography of science[5] and, nevertheless, tackles the same questions. Hence, this example addresses the question whether all sciences are objects of equivalent interest for the history of science. I will argue that it is not the case, and in particular that the social sciences are given a specific place in the historiography and that their histories differ sensibly from the ones of the hard sciences. This is not a problem, of course; the problem is rather that the historians act as if the social sciences were studied like the hard sciences' disciplines, while, in practice, they are not.

Hence this paper will slightly broaden the scope of the "new historiographical approaches to archaeology"[6] and apply the question to the case of the other social sciences. As Kuklick's example shows, the limits of the historiography of archaeology, which Gisela Eberhardt and Fabian Link have pointed out, can indeed be observed in the history of anthropology, but also in the history of history, linguistics, or sociology. In most of these fields, as Raymond Boudon has shown,[7] it can be seen that the historiographies do not match the historiography of the hard sciences. The authors who have decisively contributed to the general field of the history of science, from David Bloor[8] to Barry Barnes and Steve Shapin[9], and from Pierre Bourdieu[10] to Bruno Latour[11], are barely referred to; the questions which they raise, the methods which they use, and perspectives which they follow are mostly different, and, when similar, their origins are not mentioned.

The aim of this paper is to identify the reasons for this specific path. To do so, I propose to identify the types of historiography which historians of the social sciences write and observe that they differ strongly from hard science's historiography. Secondly,

4 Bloor 1976.

5 The general historiography of science consists of the study of natural, physical, and medical sciences. It is hence limited to what English names "science" and what French describe as *sciences dures*. In this paper, to mention this limited segment of historiography, I will use the formula: history of hard sciences.

6 The workshop during which the first version of this paper presented in September 2010 in Berlin was named: "New Historiographical Approaches to Archaeological Research".

7 Boudon 1992, 304.

8 Bloor 1976.

9 Barnes and Shapin 1979.

10 Bourdieu 1984.

11 Latour 1984; Latour 1989.

I will submit for discussion three hypotheses which may explain the reasons for the mutual distrust by the historians of 'general science' and the historians of the social sciences in both the tools developed and the results obtained. Finally, I will suggest that, since it was a historical process which gave this orientation to the historiography of the social sciences, there are possibilities to end this mutual distrust.

1 The historiography of the social sciences – an attempted typology

Over the past 40 years, the historiography of the social sciences has produced an incredible volume of work which presents so many national, institutional, or epistemological varieties that it is merely impossible to keep the overview. And even if it was possible, the few pages of this modest contribution would not suffice to describe these in any satisfying way. However a quick glance at a representative selection of publications on this topic may suggest dividing them into three broad and partly overlapping categories:[12] philosophical, disciplinary, and historicist historiography.

1.1 Philosophical history

The studies within the scope of the philosophical approach present three characteristics. In the first place, they are normative studies of a discipline, in the sense that Gaston Bachelard intended when he asserted that "*en opposition complète aux prescriptions qui recommandent à l'historien de ne pas juger, il faut au contraire demander à l'historien des sciences des jugements de valeur.*"[13] These studies analyze a discipline's past in order to help scholars to improve their practice of the discipline. Secondly, these studies ground almost solely on published texts and are therefore limited to a history of published ideas. They neither consider the history of scientific practice nor question the financing problems in science, which is considered a purely cognitive activity. Lastly, they focus on one specific discipline. To be sure, a few cross-disciplinary studies have been attempted, among them, Georges Gusdorf's *De l'histoire des sciences à l'histoire de la pensée*[14] and Michel Foucault's *Les mots et les choses.*[15] As the first general studies of the social sciences, they were crucial since they contributed to validate these activities as legitimate objects of the philosophy

12 It is important to underline that this typology is by no means a chronology. As Blondiaux and Richard have shown, the historicist turn in the history of the social sciences does not occur synchronically in the different disciplines (Blondiaux and Richard 1999, 120–121), nor does it have an absolute character (Blondiaux and Richard 1999, 116).

13 Bachelard 1972, 141.

14 Gusdorf 1966.

15 Foucault 1966.

of science and emancipated them from the domination of the practitioners' historiography. Moreover, they shaped later studies by attempting to understand the social sciences through the study of their past, thus redefining the past as a key to the essence of science. Finally, they attempted to identify the nature of the matter unifying the social sciences. Although they shared very different views (anti-humanist vs. humanist), followed different processes (unconscious vs. conscious), and had different perspectives (discontinuity vs. continuity), they tried in their more or less accurate studies of the history of the social sciences, to identify the processes, changes and continuities which have led to the birth of the social sciences and, with more ambition, to understand what has been their conception of the human being.

What is more interesting for this paper, however, is the development of this normative philosophy within each of the individual social sciences, all of which share this type of literature. In the history of archaeology, studies like Laurent Olivier's *Le sombre abîme du temps*[16] correspond to this. Archaeology, he argues, shouldn't try to put together again the events of the past but must understand the modes of memory processes through materiality. In the history of history, Michel De Certeau's *L'écriture de l'histoire*[17] or Paul Veyne's *Comment on écrit l'histoire*[18] also fit into this first category, identifying the essential characteristics of historiography and the problems arising from the use of specific tools or concepts. James Clifford and George Markus's *Writing Culture*[19] tackled the same questions for anthropology. The majority of these texts indeed make use of the history of these various disciplines to identify their current problems and to discover their *essence* by studying its past. Hence, although these studies may present significant methodological propositions, they differ sensibly from the historians' or the practitioners' positions: essentialist and anachronical, their analyses are more valuable to the practitioners because they are *bonnes à penser*. Interesting for their reflexive perspective, the philosophical essays may hence present a normative dimension.

1.2 Disciplinary history

This normative dimension is a characteristic which is again found in the second, disciplinary approach. In contrast to the philosophical approach's consideration of processes, however, this historiography focuses on analyzing what falls within the scope of a discipline and what is of interest for their practitioners *today*. As Bruno Latour puts it, they study the "stabilized state of affairs", rather than the "affair being stabilized"[20] – or in Bourdieu's words, they are concerned by the *opus operatum*, rather than the *modus operandi*.[21] Identifying the discipline-in-the-past to the discipline-in-the-present, these

16 Olivier 2008.
17 Certeau 1975.
18 Veyne 1971.
19 Clifford and Marcus 1986.
20 Latour 2005, 1.
21 Bourdieu 1997, 86.

historians assume that there are things such as anthropology or archaeology, and do not try to understand the processes which have created them. Hence, they reify and naturalize the various fields of research.

Moreover, a disciplinary historiography is also a historiography *which* disciplines. Judith Schlanger[22] and Claude Blanckaert[23] have shown that the writing of a history is generally a constitutive (and last) step in the formation of a discipline. The quest for "epistemic heroes"[24] and "forgotten precursors"[25] falls within its scope. While this is globally true, it is a particularly important process in the social sciences in the context of the reflexive turn. In the historiography of anthropology, for example, the reflexive move of the 1970s which resulted from a profound legitimacy crisis influenced many works. Hence, chronology has worked here as a *mise en ordre* of the disciplinary landscape. As exemplified in Raymond Aron's *Etapes de la pensée sociologique*,[26] history is a pretext to clear discussions about the contemporary state and the future of a discipline and Jean Jamin could explicitly link both issues: "*l'ethnologie est entrée dans une phase de remaniement, qui passe peut-être par une réévaluation de son histoire*".[27] Thus, written by leading scholars of the field, disciplinary history aims at the reproduction of the relations among the actors of the field and perpetuates the state of the discipline. Alternatively, it may be written by ambitious newcomers who use history to redefine the *doxa* and the limits of the field.[28] In both cases, history is used to discipline the discipline and this is why this historiography usually appears in the introduction of popular disciplinary handbooks. As a consequence, numerous publications written by practitioners follow this path[29] and dominate this historiography.[30]

These disciplinary histories mostly follow what George Stocking (and Kucklick, after him) has named a presentist view of a discipline's history. This view, Stocking has argued, is the position where the historian tends to demand of the past something more than simply why, where, and how something has happened. The past must be related to, and even useful for, furthering his professional activities in the ongoing present. It has a normative commitment, like Butterfield's whig interpretation of history,[31] and wrenches the individual historical phenomenon from the complex network of its contemporary context in order to see it in abstracted relationship to analogues in the present. While it is worth noting that this distinction echoes, but never explicitly refers to, the broader and famous controversy of the 1950's between Gaston Bachelard and Alexandre Koyré, Stocking asserts that it is of particular importance to the historiography of the behavioral sciences. If this is so, the explanation should be found, in his view, in

22 J. Schlanger 1992.
23 Blanckaert 1995.
24 Bourdieu 1984, 34.
25 Kaeser 2001, 202.
26 Aron 1967.
27 Jamin 1988, 474; see also: Lepenies and Weingart 1983, XVII.
28 Bourdieu 2001, 72–77.
29 Kuper 1975; Daniel and Renfrew 1962; Daniel 1981.
30 Blondiaux and Richard 1999, 116.
31 Butterfield 1931.

the fact that social sciences are, in Kuhn's words, mostly pre-paradigmatic[32]; hence, this historiography is more likely to be open to certain vices of presentism than the general historiography of science: "When there is no single framework which unites all the workers in the field, but rather competing schools, historiography simply extends the arena of their competition".[33] It means that the history of the social sciences is consubstantial to their practice.[34] Adapting Clausewitz's dictum, one might say that historiography is merely a controversy pursued by other means.

1.3 Historicist history

The historicist approach constitutes the origin of the last type of social-sciences historiography, which, like Kuklick's volume, aims at contextualizing the production of knowledge. Noël Coye's analysis of archaeological practice,[35] Nathan Schlanger's studies of the relations between nation-building and science[36] or Marc-Antoine Kaeser's biography of Edouard Desor[37] are some publications of the historiography of archaeology which fit this category; Bertrand Mueller's use of Lucien Febvre's book reviews to write the French founder of the *Annales*' biography[38] or Claude Blanckaert's study of the relations between anthropology and politics[39] are their counterparts for history or anthropology. Thus, these historians of the social sciences adopt various questions and methods from the general historiography of science. However, although one may find for obvious reasons numerous exceptions in the historiography of psychology and of governance,[40] rarely do they affiliate themselves to the different trends of the historiography of science which invented them, and seldom do they acknowledge their debts – they content themselves with the claim of being historicists. They adopt a contextualist view; analyze practices; study unpublished material which is not *a priori* a scientific archive; follow the relationships between and among the public, policymakers, and the sciences; examine controversies and relate context to science; or seek to understand the processes by which these disciplines were constituted. However, they barely mention the research in other fields, specifically in the history of hard sciences, and do not use their results. In other words, studying similar objects using a similar perspective is not a sign for sharing a disciplinary field. They do not refer to each other, and publish in different journals, as one may observe from the case of the historiography of anthropology or archaeology. Historians of the social sciences indeed have their own journals and some disciplines even have their own publication or series (*History of Anthropology*, *Bulletin of the History*

32 Kuhn 1962, 15.
33 Stocking 1968, 8.
34 Matalon 1992, 9.
35 Coye 1997.
36 N. Schlanger 2002.
37 Kaeser 2004.
38 Müller 2003.
39 Blanckaert 2001b.
40 See for example: Danziger 1990; Morawski 2005; Carson 2006; Rose 1996.

of Archaeology), or readers.[41] Moreover, historians of the social sciences are organized in specific societies (ESHHS), and are present at separate conferences. To put it clearly: they belong to a distinct discipline.

Broadly, the situation already described by Friedrich Engels[42] is thus the following: Research in the history of social sciences fits into a normative history of ideas, with a philosophical and a disciplinary pole, and neglects the recent developments in the historiography of science. Another group of historians of the social sciences shares topics, methods, and perspectives with the vast majority of historians of science, but clearly constitutes a different community of research. As Theodore Porter and Dorothy Ross have put it, "actors in this field [...] have not always been aware of one another, and some perhaps have discovered only recently that all along they have been writing this species of prose".[43] At the end of the day, these three types of historiography of the social sciences share features that differ significantly from those in the general historiography of science; these common differences (might) explain why historians of the social and the natural sciences do not explicitly share their historiographical methods and perspectives; they most probably explain why one may talk about "new approaches" for innovations of more than thirty years.

2 The origins of separate communities

Nevertheless, it is interesting to understand why and how this situation occurred. Of course, no simple answer is available here and various paths of explanation should be explored. In my view, three hypotheses might be pursued, which I wish to offer for discussion. The first hypothesis suggests that the objects of these two disciplines are merely too different to be examined in the same way. The second tracks the identity and aims of the scholars who write these different historiographies and wonders if the difference originates here. The third supposes that the origin of these two different conceptions of the historiography of science lies in the relation between the objects and the observers or, in other words, between and among the knowledge-in-the-past, the knowledge-in-the-present and the observer.

2.1 Objects

The first proposition is that the objects observed in the social sciences and in the hard sciences are too different for their historiographers to share disciplinary elements. Therefore, so this proposition, they use different concepts and publish in different journals.

41 Murray and Evans 2008.

42 Boudon 1992, 303.

43 Porter and Ross 2003, 9.

Chemists, after all, study molecules, while theologians study sacred texts, and this is why they use different tools, follow different aims, and publish in different journals. The most important series on the topic such as the Cambridge and the Norton History of Science series follow this pattern too and have published special volumes dedicated to the social sciences. The hypothesis is hence that concepts and methods in the history of science were developed within the study of a specific object, the hard sciences, and they cannot be used in another domain. To be sure, this is far from being a new hypothesis. Wilhelm Dilthey already proposed a similar position[44] and Charles Perry Snow had argued that the humanities and the natural sciences were parts of two different, incommensurable cultures.[45] More recently, Wolf Lepenies's view was still very much alike when he suggested in *Die drei Kulturen* that the history of sociology should be analyzed with the tools of literary analysis, rather than of the history of science, because it was more of an aesthetic than a cognitive activity.[46] Thus he agreed with Raymond Boudon who argued that the social sciences followed four different goals – cognitive, aesthetic, critic, and cameralist – which explained their greater diversity. Quite in opposition to the hard sciences which, he suggested, were only cognitive, the cameralist social sciences could not favor the accumulation of knowledge since they necessarily constituted a situated knowledge. This, according to Boudon, explains the need for history in the social sciences.[47]

This hypothesis is tempting[48] – although the idea following which the hard sciences would not be situated knowledge seems quite outdated – because it can help to understand why methods and concepts in the history of social science have developed in a specific way, and in a particular chronology. Since the object differs, the relevant questions and the legitimate controversies are not necessarily the same and, if they are, they do not have to follow the same chronology. This hypothesis is also appealing because it calls into question the hard sciences' imperialism,[49] which lifts these disciplines to a scientific benchmark; transposed to historiography, it would assert that the methods which allow historians to study the past of these sciences must work to understand the history of other sciences.[50] It is eventually an intriguing hypothesis, because it finds an echo in the very proposal of the STS which asserts that places play a role in the constitution of science.[51] Thus, the essential differences between the social and other sciences would be rooted in the various *loci* in which they are practiced.

However, this hypothesis is problematic: The idea that the social sciences and hard sciences are so different is, as I have already pointed out, an old *serpent de mer*. From

44 Dilthey 1883.
45 Snow 1959.
46 Lepenies 1985. – Jerome Kagan recently pursued the same argument and suggested that there were three incommensurable cultures: the social sciences, the humanities and the natural sciences (Kagan 2009).
47 Boudon 1992, 306–311.
48 See also Smith 1997, 13–19.
49 Matalon 1992, 9.
50 Porter and Ross 2003, 6.
51 Livingstone 2003.

Dilthey's *Verstehen* and *Erklären* to Kuhn's pre-paradigmatic science, concepts have been proposed by many scholars to explain this putative difference without any of them ever standing out. This is not entirely surprising, for the distinction, in many respects, does not appear to be meaningful. The study of scientific practices, rather than ideas, or essences, has thus allowed one to construct communities of knowledge which are transversal to the social sciences – hard sciences border, as Robert E. Kohler has convincingly shown with the category of collecting sciences, uniting archaeology, zoology, ethnography or botanic[52] or as Ian Hacking has done with interactive and non-interactive kinds.[53]

Aside from these transversal categories, the social sciences and hard sciences present more similarities than it has previously been thought. As Claude Blanckaert has shown,[54] the relations between science and society as well as the processes of emergence are identical in both the social and hard sciences. While he admits that these are less visible in the social sciences, he suggests searching for the origin of this phenomenon not in some undefined, essential difference, but rather in the fact that the social sciences are less of an issue than the more strategic, expensive hard sciences. They are indeed quite identical and, if something is of interest here, it might be to identify the origin of this distinction and what is at stake in this historical, but naturalized difference.[55] Hence, the validity of the category has to be questioned and, although it is certain that what is understood today as social sciences does not overlap what was understood 100 years ago, it is still unknown, as Porter and Ross have underlined,[56] if what is branded and united under the label 'social sciences' shares enough features in synchrony.

2.2 Observers

The second hypothesis turns away from the object and focuses on the identity of the historians of science and social sciences. It is known that before the late 1970s, the vast majority of historians of science were either philosophers or practitioners of their own discipline. This is true for the hard sciences, as Thomas Kuhn, David Bloor or Alexandre Koyré testify, while the examples of Robert Lowie, Raymond Aron or Colin Renfrew and Glyn Daniel show that the same goes for the social sciences.

However, from the 1970s on, a community of scholars has emerged in the study of hard sciences who are neither philosophers nor practitioners. Inventing a tradition which goes back to Karl Mannheim, Max Scheler or Ludwig Fleck, they proposed an objectifying and distant look at the sciences of the past, which quickly dominated the field. Many of these studies of science have been identified as a reaction to the realm

52 Kohler 2007.
53 Hacking 1999.
54 Blanckaert 2001a, 15.
55 Smith 1997, 7.
56 Porter and Ross 2003, 1.

of big science in the 1950s and 1960s. The critical posture towards the Cold War and eurocentrism contributed to an ideological position opposed to science, rationality and progress and led to a general critique of power and knowledge. In this process, the hard sciences have been analyzed and criticized from the outside as being part of the condemned military-industrial, capitalistic complex and, as Dominique Pestre has argued,[57] the aim of these studies was to de-essentialize and to demystify science, among others by showing its diversity, its contingency and its practices.

For the social sciences, the movement was both similar and different. A critical view in anthropology or in history appeared in the 1970s (e. g., Paul Veyne, Michel de Certeau, or James Clifford), which was rooted in the same phenomenon found in the general field of science: Anthropology's relationship to the colonial power or to the military (Project Camelot), or history's reification of past traditions to invent national, or local, or religious identities, showed significant similarities to the other sciences. However, contrary to the hard sciences' case and although there are some rare exceptions,[58] the critical move came from within. While this is quite understandable for history at least, it nevertheless constituted a decisive difference with numerous and important consequences.

Three of them should be pinpointed. Firstly, the insider's view favors a presentist perspective. This is of importance, because the social-science historians' critical view is not solely rooted in the anti-science movement, but also in the reflexive turn. In this context, history is not simply a way to criticize science, but also a path to better, and renew the orientations of the single disciplines. Hence, the presentist perspective reifies the discipline, and promotes the actual state of the discipline to a benchmark. They look for operating ancestors, take the discipline for granted, and rarely question its coming into being. Secondly, the internal critique is not weaker, but may be less conflictual. It is not a war between sociologists and scientists; it is rather a discussion among practitioners of a same discipline about the state of an art whose axioms are shared. Because of that, the fronts stiffen less and a dialogue in which everyone has the same competences appears. Lastly, the histories which they write are locally competent, i. e. at the scale of a single discipline and nation. Thus, an historian of anthropology will rarely mention, nor be concerned with, the naturalist perspectives of his objects of study, because today's anthropologists do not master these issues; similarly, they also will not be able to produce transdisciplinary studies, because it is not of any interest for the discipline, despite what such studies can bring, as Peter Galison has demonstrated[59]. This limited field of competence also hinders their joining the community of the historians of science and they remain identified as archaeologists, anthropologists, or linguists. In other words, they are amateur historians.[60]

57 Pestre 2006, 6.
58 Porter and Ross 2003, 8.
59 Galison 1996.
60 Blanckaert 1988, 451–452.

Institutionally, this has important consequences: as amateurs, they develop their networks in their own professional and disciplinary fields, that of archaeology, anthropology, or geography, and this explains their publishing in specific journals; as amateurs, they are also financed by institutions of their own field, which, of course, favor research in their field. Thus, this volume is limited to the history of archaeology, as was the workshop and while the scope could have broaden to epigraphy or papyrology, it would most probably not have been to that of geography or psychology; the scholars' publishing strategies finally proceed from these links to one specific discipline and they do not indicate a social science's historians strong wish to talk to historians of science. Hence, in spite of the quantitative importance of the social and social sciences in the contemporary university and in spite of the social scientist's interest in their own history, this historiography finds little echo among the historians of science and remains of little interest.

However, this hypothesis isn't entirely satisfying either because it appears that is not so much the identity of these scholars – both the historians of science and the historians of social sciences are social scientists, what delegitimizes the difference between amateur and professionals[61] – which is at stake, but rather the question of their relationship to their object. Some study themselves or their own tradition, others study external objects. The position is different. This leads to the last hypothesis.

2.3 Relationships

The last hypothesis supposes that the important difference lies neither in the object, nor in the historian, but in their relationship. We observed this earlier in what I called the disciplinary function of historiography, which arises from a new type of relationship between observer and object. Since Henri-Irénée Marrou,[62] social scientists and historians have been aware of the ambiguous connections between historiography and the historian. His famous "*l'histoire est inséparable de l'historien*" stressed the impact of the scholar's present, social and cultural references and *Weltanschauung* on his scientific work. The issue for the historiography of the social sciences, if similar, is slightly more complex. As in Marrou's case, the construction of the past is mediated through the historian's present. But the present comprises a conception of the present discipline itself. Therefore, history, or the discipline-in-the-past, is shaped by the discipline-in-the-present. Simultaneously, the discipline-in-the-present is of course historically determined, which means that the discipline-in-the-past shapes also the discipline-in-the-present. This double relation is important for the historiography of the social sciences since it links the discipline-in-the-past and the discipline-in-the-present through the mediation of the historian. And

61 Blondiaux and Richard 1999, 115.

62 Marrou 1954, 47.

it is all the more so when the historian is a historian-cum-practitioner-of-the-discipline as it is so often the case for the historiography of the social sciences. Hence, it offers the possibility that the historian uses the past to build the discipline in the way he wishes it as a practitioner of the discipline whose history he is writing. Thus the relationships between object and scholar are multiplied, because the historian relates both to the object in the past and the object in the present.

The historiography of the social sciences is therefore much less autonomous than the general historiography of science. The past is at stake, for the discipline in the present and for its historiography which is in return a weapon to control the present and the past. This explains why it is considered a sphere with a high value coefficient, and this is why it has been generally dominated by the big names of the field. It comes with important symbolic capital within the disciplinary fields and constitutes a major issue. For this reason, and for the motives proposed by Boudon[63] as well as in stark contrast to the historiography of other sciences, disciplinary histories are central for the education of the students in the social sciences. Historiography is a means to reproduce or change, and in any case direct, the future of the discipline.

This is significant for the argument. The development of a new historiography and sociology of general science indeed brings new actors into the field of the historiography of sciences. Moreover, these actors are quite powerful ones. Their professionalship, their external view, and the growing place of science studies in the academic world give them major symbolic capital and thus menace the field of the historiography of sciences and for those dominating this historiography. As I have argued, the threat is even greater in the case of the historiography of the social sciences, since mastering the past helps to shape the discipline's present. If in the case of the hard sciences, the intrusion of these new methods has transformed the field of the history of science with some violence, in the history of social sciences both history and the discipline itself are at stake. The issue is hence much more critical for the dominants who may lose on both counts. Their position as historians dictates their position as archaeologists, anthropologists, or geographers. Therefore, the reluctance of the historians of the social sciences to the new developments of historiography is bigger.

At the same time, one might think that the historians and sociologists of science would invest the history of the social science and make it their territory. But they do not and do not show more interest in the object "social science" than the historians of social sciences for the new tools. This is true for several reasons: First, the social sciences are of little importance in the scientific field. They are thought to spend less money, do not develop in the big science, and seem of little ideological or political use. They don't develop in big institutions, which are studied in the frame of the sociology of scientific

63 Boudon 1992, 306–311.

organizations. They are also insignificant in the social field, since they do not raise fundamental questions as the genetically modified organism, or the global management of the swine flu. Moreover, the social sciences have strongly criticized their own practices and principles in the wake of the reflexive turn; thus the desacralization of science, which was an important aim of the Science Studies, is not an issue any more. In addition, the hard science's imperialism mentioned above lets one believe that the social sciences are like any other science, and hence that there is no interest in studying their specific case. Finally, most leading names in the history of science have studied the hard sciences, and both the institutions' force of inertia and the laws of imitation maintained them for long in the core of interest.

Thus, this relational issue between the past, the present, and the viewer may explain the state of the historiography of the social sciences. On one hand, the distrust towards new historiographic trends is a reassuring posture for disciplinary historians: Rejecting these methods, concepts, and results delays the arrival of new actors and the transformation of the discipline. On the other hand, the historicist historians have been entangled in an isolated field designed by the presentist historians and neglected by the historians of science and have developed their own concepts and methods. Sometimes similar to those of the other historians, sometimes different, they only rarely mention their affiliation, if any. They only raise the flag of historicism as if it was sufficient to identify them. As a matter of fact, historicist historians of the social sciences, satisfied and obsessed by the founding separation of presentism underlined by Kuklick,[64] but insufficiently examined and certainly less significant than it is generally assumed,[65] may have neglected another dividing wall which isolated their historical object and its historians from the general history of science. Hence they must act now, so that they do not become the new presentists of the historians of science, after winning the battle against presentism – which would thus consolidate a seducing but uncompletable comparison in the context of this paper with the nineteenth century historicists who won the battle against romantic historians before losing against the nomothetic historians influenced by sociology.

3 Conclusion

I have argued that the historiography of the social sciences follows a specific path. Considering the hypotheses suggested above, I would like to offer some modest solutions to change the situation. First, historians of general science and historians of the social sciences study the same object. Thus, there should be no objection to sharing methods

64 Kuklick 2008, 1.

65 Blondiaux and Richard 1999, 110–112.

and concepts. Second, the identity of the scholars is not an issue; what matters is their relationship to the objects. Hence, historians of social sciences need to rid themselves of their disciplinary perspective rather than excluding other historians. Not only should they abandon studies which focus on one single discipline, but they must eliminate the presentist perspective. To achieve this, a possible path would be to open up their field and invite historians of science to consider their topics in order to re-shape the field dominated by the presentist views. The issue here is to abandon the "ghetto mentality"[66] which dominates the field and become professional. In this respect, it is of central importance to reject the idea that the history of archaeology or the history of anthropology are merely dynamic subfields of archaeology or anthropology.[67] In addition, they should, fully use *and* recognize the heuristic capacities of new historiographical trends. Nevertheless, this should not be an unconditional surrender. Historians of the social sciences should make use of their specific case studies, no longer with the aim of developing parallel concepts and methods, but to participate in the elaboration and the modification of the history of science's general concepts through their own results. Eventually, they must claim the value of their own concepts, such as the presentist-historicist distinction and all the critical thought born in the reflexive turn, to promote them as useful concepts in the general field of science.

In this respect, if I may express myself in military terms, these solutions address the question of the *strategic* relevance of a workshop on "New historiographical approaches to archaeological research", which constituted the starting point of this publication. First, the adjective "new" has to be questioned. What is our posture, if we assert that these thirty years old methods and trends are new, since we all know they are not? It puts us in an outsider position. Furthermore, it is limited to archaeology, most probably because of some institutional motives which have been indicated earlier. Hence it perpetuates the, incorrect, idea following which history of archaeology would be apart from the history of sciences. The tactical relevance of this is perfectly clear, since we are still, obviously, cornered in an uncomfortable situation with disciplinary historians who are dominant in our discipline pushing us on one side, and advocates of the STS-field, which are dominant in the social sciences' field, pulling us on the other. However, strategically and quite to the contrary to Blondiaux and Richard's proposition, it is crucial to take root in the field of the history of science rather than turn back to the disciplines again.[68] To do so, it may be interesting, after harvesting the results of the mentioned workshop, to go two steps further: First, another workshop might be organized, which would overstep the disciplinary boundaries, and address these issues again at the general level of the social sciences; second, after having assessed our achievements, we should confront the general historiographies of science, to understand what our specialization in the science

66 Collini 1988, 398.

67 Handler 2000, 3.

68 Blondiaux and Richard 1999, 123.

studies' field can bring to the whole field. Here we shall question whether and how the tools developed in the STS work (or not) with our object and try to contribute to their improvement or the invention of new tools. And, in this process, archaeology, standing at the interface between the natural and social sciences, will again, but for other reasons, have to be in the middle of the interest.

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