

1 Does disciplinary history matter? An introduction

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“Studying the history of our discipline is a nice pastime for a retired scholar, but an unsuitable job, and quite frankly a waste of time, for a young practitioner”, a German archaeologist once stated when confronted with the PhD research of one of the contributors to this volume. And indeed, one may ask, why study the history of any discipline or, as specifically concerns us here, that of archaeology and palaeoanthropology in particular? And what kind of history, if any, should one pursue? Even a cursory glance at the few available histories of archaeology shows that there are vast differences in approach between such (archaeologist-)authors as Glynn Daniel and Alain Schnapp, or Bruce Trigger and Wiktor Stoczkowski.¹ Not surprisingly, scholars have different views on how to approach the history of their discipline; therefore, the available histories of archaeology, as few as there are, display a striking diversity in approach and content. In *The Growth Of Biological Thought*, Ernst Mayr, one of the most influential biologists of the 20th century, proposed a classification of the various historical approaches, which in concrete studies are mostly combined, although one usually dominates.² Although this classification is not exhaustive, it may be of some use when reading the historical exercises in the present volume.

Lexicographic histories display a strong emphasis on What, When and Where questions, and generally yield very descriptive accounts of what happened in the past. *Chronological histories* consider time sequences – what happened to biology in the 18th and 19th centuries – whereas *biographical histories* aim to portray the history of science through the lives of leading scientists. *Cultural and sociological histories* stress that science is a human endeavour and hence inseparable from contemporary values and institutional milieus. They emphasize external factors that influence the overall scientific activity in a period and bring about the popularity and demise of specific theories. *Problematic histories*, finally, study the development of sciences in terms of attempts to solve problems, and origins of and changes in key conceptual issues. Such an approach acknowledges the longevity of earlier concepts, and sees the study of the history of a discipline as one way of evaluating current conceptual structures and research practices and reformulating them productively. It is an approach that stresses the heuristic value of historical studies and that stands diametrically opposite the approach often more or less explicitly present in archaeology textbooks, where one encounters a showcase view of the past: the past is over and done with; it can be isolated and put on display in a showcase, where it illustrates the long and difficult road that our ancestral colleagues had to toil along to get us where we are now – a road full of errors, freak opinions and bizarre concepts finally discarded, curiosities that can be studied by historians, including scientific sins that should be catalogued so that they may be avoided in the future. According to the historian Lübbe, showcase views ‘musealize’ the past, and the past serves to differentiate traditional, outdated archaeological practices from current solid ones, and to legitimize current practices by giving them a respectable ancestry.³

¹ Daniel 1967; Schnapp 1996; Trigger 1989; Stoczkowski 1994.

² Mayr 1982, 2-9.

³ Lübbe 1977.

Lepenies and Weingart note that such showcase views of disciplinary pasts were predominant in the history of science until the 1960s.⁴ Historians of science constantly reminded practitioners whom they could better forget, and whenever fundamental changes occurred within a discipline, the disciplinary past took a new shape. Disciplinary history was to a large degree 'preface history', to quote Thomas Kuhn.⁵ Kuhn's *The Structure of Scientific Revolutions* led to a growing awareness of the need to incorporate sociological and theoretical considerations in the history of science, and ultimately to the development of social studies of science.⁶

The present volume deals with the history and epistemology of human origin studies. By 'epistemology' we mean the analysis not primarily of scientific data as such, but of ways of handling and interpreting those data in terms of usually quite implicit conceptual frameworks, or put in more simple terms: how do we know what we think we know about the past? In this book a number of key episodes and issues from various disciplines, especially from palaeolithic archaeology and palaeoanthropology, but also from primatology, ethnology, and evolutionary theory, are analysed, from various perspectives on how such reconstructions should be approached, and on what uses such historiographic and/or epistemological exercises can have for current research, if any at all. The contributions range from quite abstract epistemological considerations which relegate disciplinary history to a minor, ancillary role⁷ to more detailed historical reconstructions without much explicit epistemological reflection (e.g. Delisle, this volume). Most of the chapters, however, explicitly pursue historical and epistemological concerns in tandem – a rare combination, all the more so because the history of disciplines dealing with human origins and the epistemology of those disciplines each, taken as such, are at present scarcely studied, though slowly emerging, fields.

Why the history of the field should be studied and how this history should be written are connected issues; the answer to the former has implications for the latter, and vice versa. The earliest versions of most contributions to this volume were discussed at a 1998 round table at Leiden University, entitled *Studying Human Origins: Does History Matter?*⁸ The meeting brought archaeologists together with representatives of other disciplines (ethnology, palaeoanthropology, philosophy, history of science) for two days of discussions on the basis of precirculated presentations. We invited colleagues to participate in the Leiden meeting because of their active involvement with the issues at stake there, disciplinary history and epistemology, with the emphasis on disciplinary history. To bolster the epistemological aspect, two more scholars were brought in into this volume, De Regt and Clark, whereas Delisle was subsequently invited to counterbalance the archaeology in the volume with some additional physical-anthropological input.

In retrospect, one could say that there were basically *two* round tables at the 1998 meeting, one on how to do 'proper' history, and one – in our opinion the more interesting one – on how to use history for archaeological/disciplinary purposes. All of the attendants agreed that disciplinary history did matter in the study of human origins, but a number of different reasons were given for why this was

⁴ Lepenies and Weingart 1983.

⁵ Kuhn 1962.

⁶ Kuhn 1962; cf. Theunissen, this volume.

⁷ E.g. De Regt, this volume.

⁸ Participants at this workshop were, in alphabetical order: Peter Bowler, Matt Cartmill, Raymond Corbey,

Robin Dennell, Tim Ingold, Tim Murray, Wil Roebroeks, Wiktor Stoczkowski, Bert Theunissen, David Van Reybrouck, and Alexander Verpoorte. The workshop was sponsored by the Dutch NWO-PIONIER project *Changing Views of Ice Age Foragers* and the Research School ARCHON, Leiden, the Netherlands.

the case, and a serious clash of cultures, a kind of territoriality problem, developed around the first question. That culture clash is clearly visible in the contributions to this volume, but it may be useful to provide a short survey of the discussions at this point. The bottom line was the following. The historians of science (Peter Bowler, Bert Theunissen) considered most historical exercises of the practitioners-turned-historians rather poor, in fact, precisely the kind of thing they teach their students not to produce, as elaborated in their contributions to this volume. At the same time, however, they both acknowledged that professional historians have neglected human origin studies, which is strange, as in other disciplines, for example evolutionary biology, there is quite some interaction between historians and practitioners. Why aren't historians of science interested in archaeology and palaeoanthropology? Bowler suggested that part of an answer might be that historians of science enjoy 'demystifying' science and prefer to study the 'hard' sciences, where such undertakings have the biggest impact. Contextualizing archaeology, he suggested, is like shooting a sitting duck; as a historian you will have much more of an impact by concentrating on other disciplines.

While there is little interest on the part of professional historians in the history of archaeology and palaeoanthropology, the practitioners of those disciplines themselves have never really warmed up to the study of their disciplinary roots either, except, of course, marginally in the form of the aforementioned 'preface history'. The citation given at the beginning of this introduction is only too typical of that attitude, and there are as yet no signs of drastic changes in the near future. To some extent, these critical colleagues might be right. In his *Vom Nutzen und Nachteil der Historie für das Leben* ("On the uses and disadvantages of history for life"), Nietzsche elaborated on the disadvantages of too much historical consciousness.⁹ He describes a herd of cows, quietly and happily ruminating along, observed by a human who envies these animals for their quiet happiness, at the same time realizing that he will never be able to be happy in a cowish way. The observer walks to a cow and asks her why she does not look at him and tell him about her happiness. The animal wants to answer and tell him that she is happy because she instantly forgets, but she immediately forgets that answer, and goes on silently with being purely a cow, leaving our observer behind in puzzlement. An animal, according to Nietzsche, has no history; it is entirely bound up in the present, hiding nothing, being an animal every moment of its full, honest and beastly life. Human beings, on the other hand, are creatures aware of their history, and therefore run the risk of no longer being able to believe in their own existence. They may eventually become lethargic: "There is a degree of sleeplessness, of rumination, of historical sense, by which the living is damaged and finally perishes, whether it be a human being, a people, or a culture".¹⁰

In quite the same vein, science historian Bert Theunissen pointed out at the symposium that scientists should refrain from paying too much attention to the history of their discipline; they should rather be entirely bound up in the present and be equipped with a romantic belief in scientific progress. Why else, as Theunissen states in his contribution to this volume, would they return to their laboratories every morning if not to obtain a better insight into the workings of nature and to lift another corner of the veil that covers it? The historian of science, on the other hand, looks upon the working scientist as someone who is producing tomorrow's obsolete knowledge. If the dangers are indeed as large as Nietzsche and Theunissen seem to think, why should practitioners become involved with the history of their discipline at all? What could we possibly gain from such a dangerous enterprise?

As Wiktor Stoczkowski pointed out at the meeting, there are numerous reasons for studying the history of archaeology and palaeoanthropology. One could simply be in it for the money, as history of

⁹ Nietzsche 1994 [1874].

¹⁰ Nietzsche 1994 [1874], 158-159.

archaeology books, peopled with stubborn and idiosyncratic individuals, sell better than archaeology itself, where people tend to be overshadowed by stones, pots and bones. The success of ‘historical’ bestsellers like Trinkaus and Shipman’s *The Neandertals*¹¹ shows that there definitely is a large market for history of science narratives about the fields at stake here. Another (kind of) immediate return that one can get as a practitioner from studying the history of a discipline is the simple fact that such an exercise can yield important data for assessing and evaluating the work of earlier scholars, which can be put to immediate use in one’s own current research. Theunissen gives an example of such an immediate return when discussing the differences between his research on Eugène Dubois and historical research carried out by the palaeontologist John de Vos. Along similar lines, one of us has studied the 19th- and early 20th-century (amateur-)archaeologists who searched for traces of the earliest Europeans in Tertiary and Early Pleistocene deposits and, as has become clear in retrospect, failed to find any.¹² One could therefore think of their activities as “byways in the history of ... Palaeolithic research ... and not spend too much time exploring them”.¹³ However, such a focus on historical ‘winners’ leads one to forget that these earlier colleagues were solid scholars who worked within perfectly acceptable scientific traditions and published in outstanding scientific journals, assembling an impressive *negative* data set that is still very relevant for current scientific research on the earliest occupation of Europe.

Furthermore, historical exercises can also be very useful to those who want to show that their specific archaeological pet theory has been around for quite a while already, because such an ancestry can heighten its credibility. Apart from the immediate return motif and justificatory role, there is the pedagogic aspect: the history of a field of study can be a suitable and entertaining introduction to a disciplinary framework, a good way of getting a first grip on its key concepts and their historical changes, gaining insight into the construction and refutation of earlier assumptions, and learning about what are now held to be freakish viewpoints but that were once totally logical and acceptable theories. As Ernst Mayr has stressed, in science, one learns not only from one’s own mistakes, but also from the mistakes made by others.

Though certainly important, the reasons mentioned above are not at the root of most of the work reported in this volume. The primary concern of most – but not all – of the contributors is a heuristic one. Most of the contributors treat the history of our disciplines as a source of knowledge of alternatives and as a means of denaturalizing current opinions and practices, a point made in great detail by Tim Murray. It is indeed a general interest in the history of approaches, of concepts, and of the many attempts to solve scientific problems that constitutes the thread running through this volume, against the background of the question of how to use history for palaeoanthropological purposes, i.e. how to further the knowledge of the remote past. Indeed, Wiktor Stoczkowski turned to the history of archaeology not to find data for a better knowledge of that history, but to further the knowledge of the past studied by his discipline, archaeology.¹⁴ Can the history of archaeology assist archaeologists in doing a better job? Yes, most of the contributors to this book would say, though for varying reasons.

Stoczkowski is very outspoken in this respect, with his thesis that historical analysis unmask many of the core assumptions of the disciplines studying human origins, their basic facts, as cultural constructs whose solidity is an artefact of their longevity, through a constant recirculation from generation to generation. His point is that a limited number of *idées reçues* pertaining to early hominids has obstructed progress in human origins studies. And although his specific stance is quite extreme, he

¹¹ Trinkaus and Shipman 1993.

¹³ Roe 1981, 28.

¹² Roebroeks 1996.

¹⁴ Cf. Corbey 1995.

does not stand not alone in this respect. A decade earlier, Mayr discussed the “paralysing power of entrenched conceptualizations” in biology and pointed out that most scientific controversies extend over far longer periods of time than generally thought: “Perhaps the greatest difficulty any conceptual history of biology must cope with is the longevity of the controversies. Many of the current controversies had their origin generations or even centuries ago, some indeed going all the way back to the Greeks. It is precisely the historical study of such controversies that often contributes materially to a conceptual clarification and thus makes the ultimate solution possible”.¹⁵

At this point, however, one runs into major problems with the ‘real’ historians of science. Thus far, we have emphasized the utility of history for practitioners of the disciplines – both Stoczkowski and Mayr are practitioners – and on the science historian’s possible contributions to current research practices. Historians, however, have a different approach to the past, which, in their view, should not chiefly be guided by present-day concerns of the disciplines involved. The two different sets of interests and abilities were played out at the Leiden meeting and are at stake in this volume in discussions between Peter Bowler and Wiktor Stoczkowski, who can be seen as representing the two opposing camps of ‘real’ historians and practitioners-turned-historians, respectively. Bowler is a trained historian of science, specializing in the history of evolutionary studies. In fact, his *Theories of Human Evolution*¹⁶ is one of the rare professional history products on palaeoanthropology. Wiktor Stoczkowski, on the other hand, trained and worked as an archaeologist, and then turned to the study of the history of archaeology, for the aforementioned reasons.

In his *Anthropologie naïve, anthropologie savante*, Stoczkowski focused on one genre in palaeoanthropology, scenarios of hominization, and within that on recurrent explicatory ideas and the reasons for their constant resurfacing.¹⁷ Bowler attacks Stoczkowski’s lack of historical contextualization and neglect of the specific, idiosyncratic ways in which explicatory ideas are put to use, lumping the latter’s argument with that of Misa Landau in her *Narratives of Human Evolution*.¹⁸ In that book, Landau analysed a number of theories of human evolution as versions of a universal hero tale from folklore and myth. Like Landau, Stoczkowski uses history to undermine the view that modern palaeoanthropology has advanced far beyond pre-evolutionary attempts to explain human origins, suggesting that modern theories are, in fact, reshuffled, recirculated versions of old ideas, many of which go back to classical antiquity. In his view, important elements of modern adaptive scenarios are already present in Greek and Roman classical texts.

Bowler, supported by Theunissen, proved himself to be entirely unhappy with this search for long lasting interpretive icons, and contended that such approaches were antihistorical, applying hindsight to the evaluation of the past and reading modern preconceptions into it: exactly what the historian is trained *not* to do. Moreover, he argued, Stoczkowski’s and Landau’s lack of understanding of the history of the discipline actually stands in the way of their efforts to uncover the real impact of cultural stereotypes on our thinking. He reproached Stoczkowski for focussing on establishing ancestries for current ideas and concepts, for time and again singling out specific topics in classical texts without paying sufficient attention to their specific contexts and the different meanings the concepts had in earlier settings. In Bowler’s eyes, this was what historians of science called precursor-hunting, pushed to the point of absurdity, whereas the whole point of a historical approach was, as he put it, to recover the context of the text and appreciate how that differed from our modern preoccupations.

¹⁵ Mayr 1982, 8, 17.

¹⁷ Stoczkowski 1994.

¹⁶ Bowler 1986.

¹⁸ Landau 1994.

Whereas Stoczkowski attempts to identify deep ideas and centuries-old binary oppositions, Bowler takes these for granted and is much more interested in why various models surface in specific times. Why does a specific side of a dichotomy become popular at a particular point in time? As he argues in his contribution to this volume, Bowler is worried that what to Stoczkowski appears to be a re-emergence may only be a superficial similarity.

The differences between practitioners as historians and 'real' historians are clear, although we may have exaggerated them a bit here.¹⁹ Once acknowledged, however, they can be made useful. Arguably, they are complementary to some degree, as Stoczkowski and Bowler indeed admitted: if you want to understand the context of a scientific debate (as attempted by Bowler), you have to understand its history, and the history of the concepts used in the debate (Stoczkowski's concern). If this is the case, how can one discriminate 'real' re-emergences of old ideas from superficial ones (cf. Van Reybrouck, this volume)? How can we reconstruct cognitive processes from old texts? Did 19th-century archaeologists just quote Greek classical authors in a rhetorical vein, to legitimize and lend credibility to their own 19th-century ideas, or did these ideas come directly from the classical texts? How do we understand these texts, both the 19th-century and the Greek ones?

The aforementioned clash of cultures partly reflects the different clienteles of practitioners and historians. Whereas the latter group addresses fellow historians and, to a lesser degree, practitioners, the practitioner-turned-(amateur-)historian generally writes for fellow practitioners; here, the clientele is the discipline itself. In such a setting, histories by practitioners always have a justificatory interest to some degree, and hence are classified as 'service history' by historians. Historical analysis is bound to be biased though, and most historians agree that it is impossible to write a history without some kind of goal being served. Hence, in Carr's view, we should study the historian before commencing to study the facts. "When you read a work of history, always listen out for the buzzing. If you can detect none, either you are tone deaf or your historian is a dull dog".²⁰

Most papers in this volume are quite 'loud' in this respect. Robin Dennell's emphasis on external factors influencing the fieldwork in early hominid studies, for instance, needs to be read within the context of his own work in Pakistan, which yielded surprisingly old stone artefacts. Tim Murray's problems with many aspects of contemporary archaeological interpretation have triggered his rehistoricization of the field: in his view, we should constantly return to the history of the discipline in order to 'denaturalize' current conceptions of the nature and purposes of archaeological knowledge, in order to demonstrate that these are not eternal truths and that other archaeologies are possible. De Regt's philosophical analysis of epistemic attitudes in archaeology elegantly complements Murray's view of the nature of the archaeological record and is unashamedly normative.

The interests of these practitioners are quite different from those of the professional historians, as sketched. There lies the 'buzz,' the 'spin' of this book: it was produced by practitioners, and most of the chapters were written by non-historians. The end product primarily aims at the practitioner in the wide field of human origin studies. In fact, one of the professional historians who refereed two of the papers in this volume was very strict in his comments: as editor of a historical journal he would have rejected both without a flicker of doubt. Both papers deal with an assessment of current research problems, and the authors argue that the historical data they have collected can enlighten our understanding of current controversies. To some degree, our historian-referee could follow and agree with their line of argument, in the trivial sense that, of course, the present is always determined by the past.

¹⁹ See the contributions by Bowler and Stoczkowski to this volume as well as Mayr 1982 for a less dichotomous treatment.

mous treatment.

²⁰ Carr 1984 [1961], 23.

In his view, the papers did not contain historical analysis and were more aptly classified as scientific reviews with some historical dressing, products of a kind of service history. But, as he stated at the end of his referee report: "One could also look at these articles in another way: as an applaudable attempt from the profession to create a historical consciousness. These attempts are authentic and will certainly prove to be valuable." We hope our readers will ultimately agree with this mild judgment by a professional historian.