

”...wiser than he himself at the time
knew”

The Histories of Archaeology and the Whig Problem

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The last decades have witnessed an increased interest in the history of archaeology, an interest which, unfortunately, has not always included theoretical and methodological issues. In this paper, therefore, the author focuses upon one vital problem in the historiography of archaeology – the problem of anachronistic reasoning. One example put forward concerns how one textbook in the history of archaeology treats the question of how the existence of thunderbolts was explained by an early scholar, the Dane Ole Worm. As a general conclusion it is claimed that different forms of the history of archaeology need different foundations with respect to the question of how to assess the past from the vantage point of the present.

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INTRODUCTION

The quotation in the title above is borrowed from a classical work on the history of archaeology: *One Hundred and Fifty Years of Archaeology* by Glyn Daniel (1975). It is part of a discussion where Daniel praises the late nineteenth-century curator of the Saint-Germain Museum, Alexander Bertrand, for having chosen the label “types” instead of “epochs” when arranging the collection of Palaeolithic material. This was done in opposition to one of the most famous scholars at the time, Gabriel de Mortillet. Daniel concludes:

But Bertrand was wiser than the de Mortillet and Piettes of late nineteenth-century pre-history; indeed wiser than he himself at the time knew. For in changing the labels he had been right: the “epochs” were assemblages of

“types”. But it took many years before this lesson was widely learnt. (ibid:126)

What we see here is a typical example of backwards history, where a historical person – Bertrand – in a narration is given the almost supernatural ability to know what would come later. Suddenly he turns into a contemporary colleague of Daniel, a colleague on the bright side of the history of archaeology. This short parable illustrates one of the most characteristic features of disciplinary histories, such as the history of archaeology: The tendency to write the history of the winners, or, in the terminology coined by the British historian Herbert Butterfield, to write “Whig history” (Butterfield 1931). Whig history, at its worst, is the ultimate example of anachronistic reasoning because of its tendency to model the past after the ideas and standards of the

present – just as in the short example above.

The problem of anachronistic reasoning has long been one of the key issues for historians of science. In the historiography of archaeology, however, the problem has been neglected until quite recently. This paper is therefore an attempt to highlight some central parts of the discussion within the history of science, as well as to contribute to the historiographical discussion within the history of archaeology.

THE PROBLEM OF WHIG HISTORY AND PRESENTISM

The concept of Whig history was introduced in Butterfield's 1931 book *The Whig Interpretation of History*, where he criticised some of his colleagues for writing biased history:

...on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasise certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present. (Butterfield 1950 [1931]:v)

The quotation captures some of the fundamental features of Whiggish history: The tendency to look for ideas in the past which can be translated, reproduced in the present, and to evaluate them in terms of the present or in terms of what came later. This is usually done without any reflections on how important these ideas were in their own time. In its most extreme form Whig reasoning leads to a kind of circular, self-explanatory history, whereby ideas, theories and concepts from our own time are used to explain the past, which, in turn, is used to explain why things look the way they do today (Nickles 1992:85-86).

According to Butterfield, the only way to reach "real" historical understanding is by making the past to our present and to see life with the eyes of another century than our own. To reach this goal one has to make detailed studies of the past – multifarious facts should speak for themselves (Butterfield 1950 [1931]:16, 72-73). The phrase "making the past to our present" captures in an expressive

way the gist of historicism, a multifaceted concept which in studies on the history of science is usually used to accommodate all ambitions to understand the past on its own terms (Liedman 1978:12-18; Outhwaite 1981:189; Shapin 1992:354). A concept related to historicism, but more narrow in focus and rarely seen, is diachronical history (Kragh 1987:90).

One typical indication of Whig reasoning in writings on the history of science is the presentation of a scientist as an isolated genius, a creative subject, who is fighting for his ideas against ignorant colleagues and a hostile society. The focus on the creative subject leads to an idealisation of seemingly rational aspects of knowledge production, that is, an internalist perspective, while external forces impinging on the scientific process are neglected.

Another typical feature is the notion that the identification of the theoretical and technical problems that scientists were facing in the past, as well as of the possible solutions open to them, is a simple and straightforward matter. Hence Whiggish historians of science employ concepts drawn from contemporary science, transferring them back to the historical situation. Unfortunately, the translation involved almost inevitably produces distorted versions of the work thus accounted for (Agassi 1963:2).

A term related to Whig history is "presentism". The meaning of this term, however, is more multifarious. In its original form the term was bound up with the Italian philosopher Benedetto Croce's (1866-1952) idealistic standpoint, that the only function of history was to work as a key to understanding contemporary society (Kragh 1987:47-48). More frequently, one can see the term "presentism" used to describe anachronistic problems in historical writing in a more general way, or simply as a synonym for Whig history (See, for instance, Stocking 1965:212; Nickles 1992:85). The latter use of the concept seems to be the most common. However,

I will return to the concept of presentism more in depth in the following.

From the middle of the 1960s and on, and quite frequently during the 1980s, scholars have questioned the one-sided criticism levelled against the consequences of Whig reasoning. Very few of those scholars reject the generally negative impact of Whig history on the historical study of science, but they draw attention to the fact that the term is often used indiscriminately, without seeing the positive aspects of drawing on contemporary science (Wilson & Ashplant 1988:1).

From a history-of-science perspective, it has been pointed out that the concept of Whig history was formulated within the discipline of political history – not the history of science. For some scholars the difference seems to be clear: political history is the history of how ideas *change and replace* each other, while the history of science is the history of *progress*. This means that for those scholars the question of Whig history is considered as much less crucial in studies on the history of science (Mayr 1990:302). A more subtle way of approaching the same problem can be found in articles written by David Oldroyd (1989a, 1989b). In brief, he stresses the fact that we must accept that interest in a specific historical problem usually has its origin in the question of *why* a contemporary idea or result has been accepted as true (1989a:358). This means that it is only natural to have the contemporary accepted result as a starting point for historical studies. According to Oldroyd this may not, however, lead to a neglect of less "rational" aspects of knowledge production, that is aspects which do not coincide with the traditional view. This is because the scientific "losers" in the long run can be just as important for the understanding of an accepted scientific finding as the "winners". He also underlines that what is accepted as a verified finding in the history of science is dependent on the context in which the result is produced, thereby indicating that the history of science cannot be

seen as an exclusively progressive activity. In this sense one can say that he advocates a kind of "restrained Whiggism" (Lynch 1989:361). Similar claims are made by David Hull when he defends certain forms of presentism in the history of science as necessary evils, thus emphasising the fact that the historian shares knowledge of the present with his readers, something which allows him to communicate successfully with them (Hull 1979:3-5).

The discussion outlined above can be used to describe the state of the debate concerning this matter within the history of science today: It is usual to stress that it is impossible to avoid the "presence of the present" in historical studies, even though the worst varieties – Whig history – shall be banned. At the same time the logical and practical impossibilities of a purely historicist history of science is emphasised. Some of the arguments used here are similar to the ones used against positivistic reasoning generally, such as the idea that it is possible to extract data without some kind of pre-understanding of an object of inquiry (Hall 1983:52-52; Wilson & Ashplant 1988:6-7). Another argument – in line with Hull above – is that the history of science cannot be viewed as a two-way communication between the historian and the past, but must be understood as a three-part relation between the historian, the past, and the consumers of historical knowledge (Kragh 1987:105-107).

The above discussions have also shown that the problem of anachronisms is a delicate matter, not only for the sake of the complexity of the problem in itself, but also for the absent consensus concerning how to use concepts such as Whig history and presentism. In view of this fact, two definitions will be used in the following: The concept of *Whig history* is used to describe a style of history devoting attention to seemingly modern ideas and findings regardless of their importance in their own time, that is, the judging of the past with the benefit of

hindsight. *Presentism*, on the other hand, will be used to denote the necessary foothold in contemporary science in line with the reasoning of, for example, Oldroyd and Hull above. This, in other words, is generally something self-evident and positive in studies on the history of archaeology.

BAD HISTORY AS GOOD SCIENCE?

In a frequently cited text by George W. Stocking, he pleads, from a disciplinary-history point of view, for a standpoint which he labels "enlightened presentism" (Stocking 1965:215). The term is used to emphasise the fact that disciplinary history is usually written from a very concrete standpoint, contemporary problems within a discipline generating the primary aim. In this sense one might say that those who write disciplinary history demand something back from history to solve contemporary problems. In Stocking's opinion this does not, however, necessarily lead to a one-sided Whig history; the foothold in contemporary problems must still be "enlightened".

A more recent and articulated objection along the same lines has been raised by the American philosopher Thomas Nickles in an essay from 1992 called 'Good Science as Bad History'. In this essay Nickles criticises an unwarranted radical anti-Whiggism common in particular among a group he dubs the "New Wave experts", by which he means the new generation of sociologists of science. They have for a long time criticised the way disciplinary history is produced, especially the tendency to seek methodological and theoretical precursors. Nickles's main objection to that criticism is that this kind of history is not written by historians of science but by "working scientists", and that the rationales of work produced by these two groups must not be confused (Nickles 1992:114). The gist of Nickles's argument can briefly be illustrated by the claim that ... "scientists are not writing about science; they are doing it" (ibid:96).

Nickles argues that Whig reinterpretations of previous work in the light of one's own may be invaluable in that this helps one to relate the work of others to one's own problems. Accordingly, he insists that it is hard to see how scientists could do good research without being Whiggish. In this sense the Whig approach is a fundamental part of scientific work, a practice whose primary goal is to solve problems in the present (ibid:98, 115). Concerning the history of science, on the other hand, history done from the outside, Nickles is more forbearing. He does accept the use of Whig reasoning in this context too, but within strict limits only. Contemporary scientific findings may be used as temporary keys to understanding past science, but they should not be used to *evaluate* knowledge claims made by historical actors (ibid:114).

DISCUSSIONS CONCERNING THE PROBLEM OF WHIG HISTORY IN THE HISTORY OF ARCHAEOLOGY

Turning now to discussions of the Whig problem in the history of archaeology, one can notice that it is usually ignored – just as in many other disciplinary histories. However, some comments on the problem have been made in discussions within the history of archaeology during the last two decades. This was evident, for example, at the TAG conference held in Cardiff in 1983. One of the sessions at the conference was called "Towards a Critical Historiography of Archaeology", and some of the papers given were published the following year in *Archaeological Review from Cambridge* (1984, Vol 3, no 1). Here one can find examples of the concept of Whig history used as a kind of theoretical straw man, and construed as an underlying principle in the study of history which is to be rejected altogether.

Bruce G Trigger has also contributed to this discussion. In his major work, *A History of Archaeological Thought* (1989), he states that he will try to:

...avoid writing a history of archaeological interpretation that is unduly presentistic and strive to understand the intellectual history of each major trend in its social context. (Trigger 1989:26)

In another context he once again stresses the importance of avoiding presentism and understanding the past on its own terms, but at the same time he underlines that *A History of Archaeological Thought* was written as a means of trying to create a perspective that is relevant to current debates (Van Reybrouck 1995:170). Here the concept of presentism seems first and foremost to be used as a synonym for Whig history, while the positive aspects of writing history in the present are taken for granted.

In some of the work by Christopher Chippendale, Tim Murray and Valerie Pinsky one also can find discussions concerning the problem of anachronistic reasoning (Chippendale 1989a, 1989b; Murray 1989; Lucy & Hill 1993:84; Pinsky 1989). They are all able to see the weakness of a one-sided, Whiggish style of history, but they are also capable of seeing beyond the concept of Whig history as a theoretical straw man and, consequently, of contributing to a more sophisticated way of describing the problem.

This can be exemplified by a statement from Valerie Pinsky, suggesting that:

...archaeologists can write the history of their own discipline for contemporary and future purposes, while preserving the integrity and detail of the past and avoiding some of the more obvious limitations of a presentist perspective. (Pinsky 1989:90)

It can be noted that Pinsky – like Trigger – refers to the concept of presentism in a way that makes it synonymous with Whig history, in my opinion, this practice is unfortunate. As we also can see, Pinsky's statement is fully in line with Stocking's enlightened presentism: it is possible to maintain a foothold in contemporary science and use archaeology's past as a reservoir for shedding light on archaeological theory and practice in the

present.

This way of approaching the problem has been criticised by the American historian Michael A Morse (1996). According to Morse, this type of reasoning automatically leads to a kind of circularity. The reason for this is that it is impossible to use history as a tool for solving contemporary theoretical concerns if one is understanding history from the same concerns. In other words we are back in the classical Whig trap – how can it be possible to claim that the historical actors had our current theoretical debate in mind when they formulated their ideas? I will return to this fundamental question in the concluding remarks below.

Before proceeding with my conclusions, however, I will try to show how a specific case of a Whiggish style of history of archaeology can be identified in one influential regional work of the genre, namely Ole Klindt-Jensen's *A History of Scandinavian Archaeology* (1975).

KLINDT-JENSEN ON OLE WORM AND THE THUNDERBOLTS

A frequently discussed theme in many textbooks on the history of archaeology is that certain artefacts, for example stone celts, during the sixteenth and seventeenth centuries in particular were considered to be of natural origin – so-called thunderstones or thunderbolts (*Cerauniae*). As these labels indicate, they were often considered to be the products of a struck of lightning – an idea which existed already in antiquity – while others claimed that they were man-made artefacts (Daniel 1975:25-26; Schnapp 1996:152-154).

One of the scholars who reflected upon this delicate matter was the Dane Ole Worm (1588-1654). In the catalogue to his *Museum Wormianum* which came out posthumous in 1655, he presented different opinions on the question. His own conclusion, however, was that they were really of natural origin, as:

...reliable witnesses state that they have

observed these stones on the precise spot – in a house or in a tree, and so on – where lightning had struck. (Klindt-Jensen 1975:23)

From Ole Klindt-Jensen's point of view, Ole Worm's conclusions are mysterious:

Strangely enough, he failed to draw the obvious conclusion from the information he had been given on various stone objects now in his keeping. An Icelandic friend, for instance, had given him a harpoon-point which had been found embedded in a marine animal, commenting that it must be the broken-off tip of a fishing implement used in Greenland. He knew of New World stone tools and weapons, and had a drawing of three sword-shaped flint daggers from Mors. Similar pieces, with clear traces of human workmanship, were found in burial-mounds. (ibid)

Worm accepted the idea that flint implements had existed, but stated that the art of fashioning flint has been lost. This standpoint is hard to explain, according to Klindt-Jensen: How is it possible that Worm, while aware of the facts cited above and having obviously reflected upon the possibility of flint as a raw material, was still unable to see that the thunderbolts were man-made? How could he fail to draw the "obvious conclusion"? With this way of approaching the problem, it is not strange that the author's interpretation of Worm's standpoint is characterised by a sentence like, "Worm was clearly puzzled" (ibid).

Interesting here is that Klindt-Jensen does in fact attempt to explain Worm's position from the information that he possessed at the time – a move towards diachronical history. This as he draws a picture of the different kinds of facts that Worm "had in his hand". But he, and this must be stressed, still fails to take the most important step, namely a discussion that starts from the point of Worm's *conceptual framework*. So the information that Worm had is projected towards Klindt-Jensen's conceptual framework, not towards his own. This creates a biased fragment of history, where Ole Worm's conclusions are hard to understand for the

contemporary reader. Therefore, in this specific case the Danish scholar rests on the dark side of history of archaeology, while a scholar such as the Swede Kilian Stobæus (1690-1742) – who, from a standpoint accepted today, came to the right conclusion – dwells on the bright side (ibid:38-39). This kind of two-sided history is a typical feature of Whig history.

In order not to single out Klindt-Jensen's pioneering work, it must be stressed that a parallel type of reasoning around this specific question can be found in two other classical works within the history of archaeology, namely Glyn Daniel's *One Hundred and Fifty Years of Archaeology* (1975:25-26) and Bruce G Trigger's *A History of Archaeological Thought* (1989:47, 52-54).

CONCLUSIONS

The first and most basic statement must be that the problem of anachronistic reasoning must be viewed in the light of our understanding of the concept of "history of archaeology". So what is history of archaeology?

A direct answer to this question is that no one history of archaeology exists – indeed, no such unitary narrative can exist – but that we have a number of histories *of* and *in* archaeology. Elsewhere I have tried to identify two analytical ideal types of history of archaeology. The first type has an unequivocally historical character, instantiated by general surveys and biographies – those texts that we routinely label the history of archaeology. The second has a less conspicuously historical nature, instantiated by the way we write historical backgrounds to specific problems or the way we use references and historical statements in our archaeological practice (Gustafsson 1996). I have labelled the first type *history of archaeology in the form of history* and the other *history of archaeology in the form of archaeology*.

To the first type I would assign the work of Klindt-Jensen mentioned above (1975). Here one can find implicit, but strong, claims

of historical truth – the ambition of telling us a story "the way it really happened". Well, how did it happen? Apparently that is a question that has no final answer, but one thing is certain about the history of archaeology: The theories and findings of today have not always been considered true and, conversely, things that have been accepted as true are no longer true. This means that a great deal of the history is being left out, since it is usually structured in an over-simplified way with the "winners" on one side and the "losers" on the other. All the "dead ends", "false" theories and "mistaken" findings are used primarily as examples of misconduct or misunderstanding, rather than as products of different contextual conditions, while the "correct" theories are always made to seem to correspond with the accepted theories of contemporary archaeology.

If we want to create a better understanding of past archaeology in a broader sense, as a knowledge-producing activity, the most crucial question is not what findings and theories of the past coincide with the ones accepted today, but why and how these claims and other, less successful ones, were accepted or rejected. If we want to produce a picture of past practice, past archaeology, as rich and multifaceted, it is necessary to minimise the impact of Whig reasoning. However, to succeed in such an endeavour one still has to use a presentist foothold in order to make one's account meaningful.

But if it is impossible to use this type of history of archaeology as an instrument for "improving" contemporary archaeology, the question must be: Why do we need a multifaceted and perhaps contradictory picture of past knowledge production? To answer that question it is important to stress the difference between our comprehension of *past archaeology* and our different ways of *doing archaeology*. That is the difference between the stories about the storytellers and the storyteller's story; or to paraphrase the sociologists of science Harry Collins and

Trevor Pinch, it is not our way of doing archaeology which needs changing, but our image of archaeology as a science (Collins & Pinch 1993:78). Challenging the understanding of our own discipline has implications for the possibilities to discuss the impact of science generally, as well as its politics. By showing that many of the so-called rational aspects of knowledge production are impregnated by factors that have very little to do with our general comprehension of science, one can make it more human, social and political, which in turn can be a starting point for discussions concerning knowledge, power, and the right to interpret (see for example Woolgar 1988:9). A history of archaeology in the form of history which does not treat knowledge as a kind of finished product and with aims not directed toward the results of today, can therefore help to make our self-comprehension more problematic and also the "obvious" scientific results seem less obvious.

As for the more subtle kinds of history, those that I have called *history of archaeology in the form of archaeology*, things turn out slightly differently. If one looks for instance at historical backgrounds to specific problems of that type, which one can find in almost every substantial archaeological text under the heading "research history" or "historical background", I would say that the historical claims made are much more limited and the historical focus more narrow than, for example, in general surveys like that of Klindt-Jensen. This means that they are more directly connected to the practice by which archaeological knowledge is produced. As a consequence, questions of Whig tendencies are far less crucial – this is primarily an archaeological business, not an historical one. Concerning this form of history of archaeology, I therefore agree with the thrust of Stocking's, Nickles's and Pinsky's arguments above: The problems of contemporary archaeology can and should be used as a point of departure for evaluating, even judging,

previous archaeological findings. This is because the procedure is part and parcel of archaeological practice – we are not, in this context, historians, but archaeologists trying to solve contemporary problems. In that the claims we make are primarily archaeological ones, history is being used the way it is in any contemporary scientific discipline, and the presentist basis of the exercise is fully legitimate. Therefore, in this context – doing archaeology – Michael A Morse's objections concerning the problem of circularity are not valid.

To conclude:

- When archaeologists write the history of archaeology in the form of history, Whig reasoning should be banned.

- When, on the other hand, archaeologists use history as one of many resources in their research, in doing archaeology, more than a whiff of Whig reasoning is natural.

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