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Nature and history : towards a hermeneutic philosophy of historiography of science

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Citation

Bouterse, J. (2016, February 25). *Nature and history : towards a hermeneutic philosophy of historiography of science*. Retrieved from <https://hdl.handle.net/1887/38041>

Version: Corrected Publisher's Version

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Note: To cite this publication please use the final published version (if applicable).

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Issue Date: 2016-02-25

Conclusion: Nature in History

The answer to our main question – the role of the world in historiography of science – works on two levels, since the question itself has turned out to have two sides: it is the question *what* explanatory work nature does, and how *what* explanatory role it has in historiography of science relates to other explanatory factors; but it is also the question in what sense *our* knowledge of nature is capable of providing us any insight into a history of science that predates that knowledge. In other words: it is the question of the explanatory role of nature as well as the status of our beliefs about nature in history of science. These different readings of our question correspond strongly to different parts of the thesis. But in the end, the position defended in this thesis can be understood fully only if we bring the two together.

The answer is that the role of nature is itself historically variable, not just because nature itself develops historically – in this development, there may, after all, be significant continuities that some sciences seek to identify – but because its mode of influence is dependent on historically formed scientific traditions. Upon these traditions themselves, the natural world has also exercised a causal influence, dependent on previous modes of interaction between nature and culture.

It follows that nature is causally integrated within the whole of history of science, without thereby rendering final beliefs about itself inevitable. In this, we depart from the inevitabilist perspectives in chapter 4, which, in so far as they were genuinely inevitabilist, turned out to involve an explicit or implicit belief that the final shape of science reflects only the shape of nature; and we depart from the perspectives dealt with in chapter 5, which said that science can be understood without referring to nature at all. The reason is not just that nature is neither completely impotent nor all-powerful in the history of science, but that its mode of causal influence upon the history of science is itself historical and cannot be simply disentangled from non-natural influences. Here we agree with Latour, who, as we have seen in chapter 6, has done a lot to counter ways of looking at science that need to draw a chasm between nature and society. But what we take from him is not the suggestion that we are not allowed to use what categories and labels we possess for the mere reason that these labels are a contingent product of history; rather, we follow him in saying that there is no ontological chasm, and that the entities that constitute science and the entities that constitute nature (as the object of scientific interest) are in continuous interaction with each other. There is no meaningful sense in which we can claim there to be a ‘net influence’ of society, or of nature.

This inextricability of nature and culture has repercussions for both sides of our original question. With regard to the role that nature plays in determining what science looks like, it pushes us, in effect, towards a naturalist perspective. It entails the rejection of an ontological dualism between nature on the one hand and science or society on the other hand; it is a perspective that recognizes entities for their causal interaction with other entities. The most sympathetic account of scientific development, when it comes to the

explanatory role of nature, is that by David Hull, discussed in chapter 7. His combination of evolutionary and invisible hand mechanisms leads to a plausible picture in which natural and non-natural entities are seamlessly integrated, without thereby forgetting that it is science and its development which have our interest, and without foregoing the possibility to explain this development. His account allows us to say that what physically happens in air-pumps matters because different events in air-pumps would have led to different observations and a different differential proliferation of beliefs. That is, the selective environment of science – which comprises both natural and non-natural entities – would have been different if air-pumps behaved differently. However, we need to embrace even Hull selectively, and ignore the inevitabilist claims he sometimes makes, especially in so far as these are based on the idea that the natural objects that science investigates render the development of science inevitable; his own model does not lend support to such an inevitabilism, since the content of science does not adapt to an environment that consists only of the natural entities it studies.

With regard to the other side of the question, the answer is that our current beliefs about nature are hermeneutically inevitable (or ‘inescapable’, in order to distinguish this sense of inevitability from the thesis that our current science is historically inevitable), precisely because nature is causally integrated in the whole history of science: we cannot understand past science without assuming anything about the natural world in which it developed, and there is no reason why in so doing, we would opt for any assumptions about the natural world other than those we actually hold. Here our alignment with many authors discussed in previous chapters gets inverted relative to that concerning the causal question: though we may disagree with Weber, Merton, Koyré and the Marxists about the historical inevitability of our science, we embrace their presentism. Though we disagree with Bloor and Collins about the explanatory relevance of nature, we follow them in their acceptance of current science as providing the best understanding of nature available to us. And though we agree with Latour about the contingency and historicity of our current beliefs and categories, and about the causal inextricability of nature and science, we decline the methodological prescriptions he draws from this, that in order to understand, we need to be radical empiricists and simply follow the actors.

What we have shaken off, then, is the doublet of assumptions that in order for something to be a legitimate point of departure for understanding in history of science, it needs to be historically inevitable; and that we can find the history-independence this requires in nature more easily than in society. Most other perspectives we have dealt with worked by at least one of these assumptions: the inevitabilists we discussed could be presentists because they were also inevitabilists; the contingentists we discussed mostly decided that since nature failed to render current beliefs inevitable, it could not be used as a stable vantage point for historical understanding. Latour consistently concludes this logic, by the idea that since everything we have is the result of a contingent history, nothing of it can be of help when we want to account for that history.

The hermeneutic perspective we are embracing here allows us to shake off both the nature-society polarity, and the preoccupation with identifying the history-transcendent and separating it from the historical. If a polarity still remains, it is between historical continuity and discontinuity: we build our understanding of the past on what we seem to share with it – on the extent to which the horizons of this past and our present overlap – and we seek understanding of what seems alien. What we share may be the way air-pumps or other experiments seem to behave; or it may just as well be ways of reasoning, or aspects of a cultural or political environment – the point here being that what is natural is as such not necessarily more robust than what is social or cultural, nor the other way round. Perhaps we cannot step in the same river twice, but we can read the same Platonic dialogue twice; but then again, perhaps human political systems change their form and behavior, in the sense in which those interest us, at a quicker pace than the solar system does.

Which things change, when, how much, and under what circumstances, are not questions to which we can decide the answers *a priori*, but neither are they questions to which the historical sources will single-handedly teach us the answers. Answers to these questions are based on historical knowledge, part of which is accumulated in the traditions of which we are part – historical *and* scientific; for these are not mutually exclusive categories, and our belief that air-pumps and other instruments exhibit stable behavior over time is itself based on historical knowledge. This is not to say that knowledge about vacuums turns out to be subsumed under the expertise of historians and that all other disciplines should concede their territories to history; rather, it is to remind ourselves again that the tensions between history and science sketched in the introduction of this thesis rest on taking a bit too seriously the neo-Kantian distinction between the idiographic and the nomothetic, or between the cultural and the natural sciences.

It is the productive tension between continuity and discontinuity that makes a hermeneutic perspective so well suited for a *historical* study of science – rather than a rationalist, idealist, Marxist, social constructivist, actor network-theory or generic invisible hand perspective. The evolutionary perspective discussed in chapter 7 comes closest, in its treatment of science as a historical entity and its transcendence of a nature-society dichotomy. We can treat the development of science, or developments in science, as contingent in the sense described in chapter 2 – with results that are path-dependent rather than inevitable, implying that it was historically possible for us to have other scientific beliefs, without thereby taking it any less seriously or denying ourselves any of the resources it might provide us for understanding its past.

How does all this help? Much like Gadamer's *Truth and Method*, the aim of these considerations is not to draw methodological lessons, but to elucidate what historical understanding means within the context of history of science. I believe that this elucidation can be helpful in, among else, the following ways:

- 1) Overcoming the Whig – anti-Whig polarity: in this hermeneutical perspective, “scientists’ history” and “historians’ history” are no longer separated by an unbridgeable chasm, representing incommensurably different epistemic aims and attitudes towards the historical sources.

What remains, is a range of possible opinions about the modal structure of history: about what determines – in general, or in particular instances – scientific developments, and in what sense and in which ways these could have gone differently. In short, what remains is a debate about causality in history of science, and a competition between explanations, in which no side *a priori* carries better cards than the other – in which there are very few claims of which we can deduce from their language that they must be ‘unhistorical’.

- 2) Enabling historians to employ more liberally their actual beliefs about what did and could have happened in the past, by showing the real problems that the concept of Whig history points at to reside in causal anachronism, rather than in presentism or conceptual anachronism.

We can say that there were microbes before Pasteur, and that people died because of them, without worrying too much that we are committing a deadly historical sin. We must always remain open to new historical insights into the question under which circumstances microbes are and are not possible, but this is because we cannot know *a priori* which of our beliefs may constitute causal anachronisms, and therefore none of our claims is ever absolutely historically safe. It is certainly not because we are allowed to speak of microbes in our narratives only after our actors have started speaking about them.

- 3) Showing in what sense history of science can provide and has provided, both in principle and in practice, genuine corrections to our understanding of science. This is not by leaving our presuppositions about science and the world as we know it through science at the door, but precisely by bringing them to our historical investigation and keep an open eye to where tensions arise.
- 4) Giving a meta-account of what is good historiography: rather than defining the methods or desired outcomes of the study of science in history (for instance, Harry Collins’ Empirical Programme of Relativism, which is supposed to show how science is determined by social rather than ‘scientific’ factors), a hermeneutical perspective on historiography of science does justice to the extent to which historiographical work is in dialogue with pre-existing understandings of science in history. It contextualizes and historicizes, rather than defining generally, the very notion of good historiography.

Thus, it can admit the importance of Collins’ EPOR, and of SSK in general, in a specific background, as a critical corrective of a rationalistic and transcendentalist view of science. It can see Latourian ANT as in turn in critical dialogue with rationalism and SSK, and invisible hand accounts as a response to

constructivism. It recognizes that new historiographical perspectives themselves build on material from a contingent history. Thus, a hermeneutic philosophy of historiography of science unproblematically meets the criterion of reflexivity: everything it assumes about science in history it is prepared to assume about itself.

Above all, this thesis has attempted to outline how we can think of science as genuinely historical and genuinely about the world. We saw at the start of this thesis that Sarton set out to do precisely this but failed to deliver. The rest of our work has been a search for a consistent and plausible way to look at the relation between science, history, and the world in the writing of history of science. A hermeneutical perspective constitutes such a way, however contingent and path-dependent the formulation of such a perspective as provided in this thesis may be itself.