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Conclusion

1. The arguments against evaluative historiography disarmed

We need evaluative historiography because it provides indispensable tools for historical understanding, does justice to the aim of scientists to improve over their predecessors or contemporaries, helps to avoid the negative effects of non-critical engagement with past science, such as undermining the credibility of present-day science, and provides the means to come to a measure of science as a progressive endeavour. The platform given in chapter 7 facilitates a mature form of evaluative historiography. If too little is offered in terms of guiding definitions in the eyes of the philosopher, I cannot help it. If naturalism is to mean anything, stricter a priori guidance of historical interpretation is not to be had. If the historian complains that still too much is assumed prior to empirical research, the ball can be put back in his court. The basic components of the platform have been carefully chosen. They yield modest guiding principles of interpretation to avoid falling prey to the arguments against evaluative historiography given in chapter 1. We can now see how the five main arguments have been disarmed.

The argument from theory dependence, that evaluations are not neutral but always dependent on a set of assumptions, is simply embraced. Our assumptions are made explicit in the platform. But the requirement is that the sets of virtues and strategies must be generalized from historical practice and hence are not ‘imposed’ on the past. The caveat is that these assumptions are not definitive but stand open to improvement, provided good arguments for this can be given. Similarly, a degree of presentism has been found excusable for all elements of the platform. The type-occurrence distinction ensures that historical analysis remains tied to all relevant material and socio-cultural conditions. With respect to the use of anachronisms and present-day scientific knowledge an interpretive circle has to ensure that the past is not viciously moulded into present-day concepts.

This intermediate type level is important, as it also ensures comparability between distinct periods and/or research programmes. In addition, we assume continuity in natural phenomena, in a set of basic cognitive attitudes and through the gradual nature of change in research programmes. All this must be enough to ward off the threat of incommensurability.
The argument from underdetermination is answered by drawing a distinction between strong and weak decisions. With hindsight we can consider past research programmes as wholes. If we do, a demand for full closure of scientific controversies at every step in the development of the research programme is no longer needed. It is possible to work with a series of weak decisions leading up to an eventual strong decision. When it comes to the strong decision, the argument is that the rationality of this decision can no longer be reduced to social factors and hence rational factors have an, at least to some extent, independent determining role in science.

Finally, the argument from rule following, that science cannot be captured via a meta-methodology because it does not progress in an orderly step-by-step incremental way, is also accepted. But in an ‘elements and relations’ approach, this point becomes irrelevant. The relationalist approach implies a non-hierarchical stance towards determining factors. It allows for more than one combination of factors to determine, not just the course of science, but also the evaluation of this course.

This shift in thinking is important because it helps to eliminate the persistent positivist assumptions about science. To briefly repeat these assumptions from chapter 5: in order to be evaluative we do not need a theory-neutral observation language, as the standard of comparison is given by a set of types and by the available alternative theories. It is also not necessary to demand that all scientists share the same (meta)-methodological standards. There simply is room for a great variety of choices and strategies within the soft boundaries set by the platform. Theory evaluation does not involve the application of a mechanical algorithm; it involves a delicate qualitative balancing act in which various factors and virtues need to be weighted and related to each other. Because we do not impose a hierarchy on the factors determining theory choice, it does not matter that progress is not strictly cumulative. As the examples in chapter 7 show we can still measure whether one theory scores better on particular virtues than another. Moreover the focus on the determination of theories should no longer occupy central stage but be subsumed under the wider pursuit of certainty. This creates the possibility of mitigating the problem of underdetermination. Finally, we don’t need to assume a strict demarcation between internal and external factors. While the typical is lifted out of historical context it remains firmly tied to it at the same time.
2. Novel concepts and perspectives

Getting rid of a number of positivist assumptions about science and providing a place for assessments in terms of rationality and progress required a number of conceptual innovations and the adoption of, in part, novel perspectives. These are the typical level and the type-occurrence distinction, relationalism, the perspective of uncertainty, the notion of the fertile error, the notion of ‘going amiss’ and the idea that we should go comparative ‘all the way down’ when it comes to assessments of scientific theories.

While it was clear that symmetrical study of past science leads to carving up the past in too many isolated episodes and that both absolutism and presentism assume too much unity between historical episodes, it was less clear how to find an intermediate position between the two. We have found this in a typical set of virtues, a typical set of research strategies and typical stages of research programmes. Alternative theories can be normatively compared with respect to these sets. Nowotny, Scott and Gibbons (2001) have argued that because there are no absolute standards of evaluation an open mind to alternative theories is always needed. Claims to knowledge can gain stability only if we constantly interrogate them. In their view the lack of absolute standards imposes a responsibility to engage in, and be open to, critique. It is fruitful to have confrontations between competing theories because these confrontations challenge the content and assumptions of theories. If we find participants in past science eschewing open debate, this is often a sign that they are no longer on the right track.

The type-occurrence distinction, which was borrowed from SSK, is particularly useful. With a fully descriptivist approach (to which posthumanism comes closest) hands and feet are tied to what history has to offer. It is not possible to take a little distance and analyse what has happened. This pre-empt the possibility to account for improvement in science. We can also not discriminate the merely accidental from what is of lasting value in particular historical contexts. Furthermore, sticking to actor’s categories makes it difficult to use explanations of past science that make reference to things or factors of

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502 The willingness to engage in critical debate with others is often seen as a precondition to scientific rationality (Longino 1990). One of the characteristics of pseudoscience is an avoidance of critical discourse.

503 This was for example seen in the Pflüger-Minkowski controversy. Pflüger resorted to rhetorical and authoritative arguments in a desperate attempt to avert the rejection of his theory.
which the historical actors were themselves not even aware. From a distance we can sometimes see more than when in the midst of things.

In generalizations on the type level we have found the most fruitful analytical distance. It is perhaps strange that establishing typical features of the scientific endeavour has become more important for evaluative historiography than the direct assessment of the content of scientific theories. Theories, however, come and go. We must be able to tell whether this successive line of replacement has been progressive or not. Hence we are interested more in assessing the quality of the scientific process than in the direct assessment of the quality of scientific theories.

The golden mean between absolutism and particularism has been found in an approach that is marked by relationalism, that is, by thinking in terms of interconnectedness and not in terms of hierarchy. The relationalist approach has been applied to theoretical virtues, research programmes and the determining factors in science. In all these cases sets of elements, such as the theoretical virtues, can be identified as fairly constant. It depends on the particularities of individual cases how relations between these elements have come about. By studying shifts in these relations we can study how changes in the past occurred. This relationalism provides a basis for agreement, and hence allows for a thorough comparative approach towards evaluation of scientific theories. By “thorough” I mean that theories cannot be judged independently of rival alternatives.504 Direct evaluation of belief makes no sense. It is only change of belief that triggers the question of evaluation.505 All theories can be rationally defended, but some are more rational than others.

Our relationalism needs to be carefully distinguished from posthumanist relationalism. The strong points of posthumanism were: the possibility to engage in extra-local study of past science through the network concept, the stress on the temporary character of settlements of controversies, the relationist stance towards determining factors, the use of concepts such as hybridization and finally the move beyond perspectivism with its untenable combination of ontological realism and epistemological relativism towards a more direct understanding of the interaction between our conceptual systems and the world (direct mediation). Still, posthumanism falls short: it does not incorporate cognitive factors as independent determiners of the course of science and it

504 See Laudan (1996) in which he argues that the acceptability of a theory must be relativized to its competition.
505 Kuhn (1991)
never reaches a comparative level of analysis. Hence in posthumanism we cannot speak of qualitative improvement over longer periods of time.

It would be interesting to investigate the issue of direct mediation, and its implications for the study of past science, more thoroughly in future research. Daston and Galison (2007) have pointed out that objectivity has always been about representing nature, but suggested that this may no longer be the case in future. In nanoscience, for example, researchers interact with the world and create the very phenomena they are investigating. Daston and Galison therefore tentatively call the new form of objectivity that is emerging the “nanofacture”.  

For posthumanists the blurring of the distinction between the natural and the social is not new, as they have been arguing along that what we discriminate as natural and social structures are the (temporary) products of an all-encompassing interaction process, in which all actors (both human and non-human) have played their part. Likewise in the work of Davidson we find a plea for a direct mediation between our conceptual systems and the world around us. For posthumanists and Davidson alike, direct mediation is not just an effect of the latest developments of science; it is a perspective on science that should be applied to all periods of time. Scientific knowledge is the product neither of pure representation nor of pure construction. If the human aspect is neglected we get a one-sided objectivism, if the world is sifted out we get a one-sided perspectivism, hence we must go for a form of relationalism that sits between the two. What counts as reality is part of the process of mediation that goes on all the time. Changes that come about in our understanding of the world must thus be explained by changes in relations between natural, social, personal, and rational factors. Interaction itself is fundamental, even to ontology.  

As a consequence our natural and social structures are less stable than we thought. However, without stable anchor points, the world becomes a more uncertain place.

506 Similar ideas based on the blurring of the line between nature and artefact can be found in Nowotny, Scott and Gibbons (2001).  
507 Philosophers try to find a new vocabulary to capture this fundamental interaction (see also footnote 149). Pickering’s mangle was an early suggestion. The ‘nanofacture’ is another. Haraway has suggested that we need to replace the correspondence theory of truth with a co-respondence theory. Dolphijn and Van der Tuin (2012) speak of a new materialism. Perhaps we need ontologies on other levels as well, such as an ontology of processes See also Byers (2011) in this respect.  
508 The dynamic worldview has the following characteristics: space and time are not absolute, biological species are not permanent but change and evolve, on a fundamental level the world is probably not made up out of material substances and finally the line between the perceiver and what is perceived cannot be drawn sharply (Brush 1988). This
The sociologist Bauman has argued that we have entered a phase of liquid modernity, which replaces the preceding phase of solid modernity. In solid modernity, societal structures are much more stable and robust. This is reflected in science studies in the somewhat static notions of paradigm (Kuhn) and research programme (Lakatos) and also in the localism of social constructivism. In the liquid phase individuals change affiliations much more often: they continuously enter into different relational spheres. The lack of stability this involves leads to an increase in feelings of uncertainty. Hence it is not a coincidence that approaches in science studies stressing relational dynamism need to take uncertainty into account as a central concern.

The lack of structural stability in the ‘liquid’ world presents a danger. When no sense of direction or purpose is present any longer, this will be harmful to society. With respect to the study of science I believe this lack of direction is exactly the problem with posthumanism. We need a relationalism that can provide a more or less stable ground to assess the quality of the science that is produced. The platform of chapter 7 is intended to meet this challenge.

While feelings of uncertainty are in general problematic, we have seen in chapter 4 that a shift to the perspective of uncertainty is beneficial to historiography of science. Uncertainty can be decreased in more ways than just the determination of theories. This provides the much-needed space to account for past science with concepts such as ‘fertile error’, ‘going amiss’ and ‘retrospective error’. Relying on epistemic virtues is part of the strategies for blocking errors. To get things wrong is not synonymous with being irrational, and similarly, to be right is not always the result of complying with strict rules. It is possible to treat past scientists with the utmost respect, even though the theories they defended were rejected later on and are no longer credible in light of alternatives.

Science may be full of repeated attempt to reduce uncertainty, but we have to accept that complete certainty will probably never be achieved. There is a permanent lack of closure in science. Understandably, this presents itself as a source of discomfort. However the ability to live with a degree of uncertainty is also an expression of inner refinement.

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is most clear in quantum physics in which measurement interferes with what is being measured. See Prigogine (1996) who proclaimed the end of certainties in science on the basis of this.

509 Bauman (2000), (2006) and (2007). In important respects the notion of liquid modernity is a continuation of Giddens’ older concept of late-modernity, see Giddens (1991).

3. A research programme for the history of science

This thesis has been directed first and foremost at historians of science. I have argued that the study of past science should not be frozen into a set of present-day standards and procedures. Nor should it be frozen into sets of standards and procedures that were operative in the historical contexts under study. In order to strike a good balance between being judgemental and being tolerant, a pluralism within parameters has been developed that retains room for evaluative historiography while giving up on strict demarcation between social and rational factors.

Assessments of past science are in the first place qualitative assessments. They involve a difficult balancing act of values, virtues and other determining factors. We can still learn much more about typical virtue preferences, the role that uncertainty plays in science and the phenomenon of error. Whether the normative dimension of our approach can be strengthened is for the most part an empirical matter, which requires detailed historical research. In this sense the current thesis provides the history of science with a research programme. This programme has to operate within the guidelines of the platform, but at the same time put its fruitfulness to the test. Because we find ourselves in the first phase of a new research programme there necessarily are many speculative elements. All aspects of the proposed approach are however motivated by the idea that if we equally respect all, we lose sight of what it is that should be respected in other persons, periods or theories in the first place. I believe that sophisticated discrimination, through a comparative use of a set of virtues and values, is what we need in historiography of science and perhaps also in other areas of present-day ‘liquid’ society.