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Introduction

1. The problem

Current historiography of science is for the most part a non-evaluative discipline. In an earlier period things were different. Historians of science saw it as their task to safeguard the tradition of scientific progress and voiced opinions about what good and bad science was. Hence the discipline was decidedly evaluative in character. In this respect historiography of science was also closely connected to philosophy. It operated in accordance with philosophical projects of providing context-independent norms of rationality and progress, on which both present and past science could be judged.

This type of historiography met with a strongly dismissive reaction from the 1960s onwards, which has changed the discipline almost beyond recognition. The view of science as a unique and united endeavour, as well as a pure quest for knowledge, gave way to a different view of science, namely as a disunited collection of activities, not much different from other human activities and ‘impure’, in the sense that the quest for knowledge is always inextricably mixed with values and interests stemming from the socio-cultural context in which scientific research and theory formation take place.

From this it follows that the acceptance and rejection of claims to knowledge have to be understood in relation to evaluative standards operative in particular contexts. An overarching evaluative procedure, connecting individual contexts, is no longer recognized. As a consequence, a sense of qualitative improvement of our theories of the world, on such dimensions as empirical adequacy, predictive adequacy and validity, has almost entirely disappeared from historiography of science. This has always struck me as peculiar because one of the motors behind scientific change is a wish for improvement, as many utterances of past and present scientists testify. Why then have accounts of improvement disappeared from historiography of science?

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1 In Dutch the term ‘Wetenschap’ covers the whole range of academic disciplines including the social sciences and the humanities. In my view ‘historiography of science’ should include this whole range as well. Yet, the field traditionally focuses on the history of the natural sciences (including the life sciences), medicine and mathematics, and the examples of historiography of science given in this thesis reflect this bias. One of the major challenges the profession faces is to integrate the history of the humanities and the history of the sciences into one ‘history of knowledge’. For arguments in support of this assertion see the Focus section in Isis 106-2 (2015).
The major change in perspective towards past science has yielded tremendous gains in scope, detail and general sophistication of the profession. Yet, what historiography of science has gained in all these respects, it has lost in analytical power. Gradually almost all prior analytical distinctions have been rejected, so as not to distort the past, and let it speak for itself. But it has become increasingly unclear in answer to what questions it should speak to us. We have become so open minded towards other belief systems, value systems, standards of evaluation, etc., that the danger is that our brains fall out.²

This manifests itself most clearly when it comes to assessments of past science. Increasingly people feel that the, in itself just, reaction against the unquestioned universalism of earlier historians of science has gone too far.¹ Even one of the main proponents of contextualism, Bruno Latour, has voiced worries that science studies are losing their critical functions.⁴ However, the problem is how to endow history of science with critical functions again without relinquishing the insights that have been won in the past few decades in the complex and many-faceted process that science is and without falling back on the, now obsolete, universalism of the older normative agenda.

This is a hard problem because prima facie it seems impossible to merge contextualist historiography, with its emphasis on contingencies, contrasts and the time-bound character of the justification of scientific knowledge, with a sense of continuity of scientific development and context-independence of norms that is required to carry out assessments of past science. For reasons to be discussed, in the often vehement debates on approaches to past science, it has proven difficult to occupy a middle ground between the two opposing points of view.⁵ Ronald Giere, for example, rhetorically asked that if we deny: “that there is any basis for the norms that transcends the society in its actual context, does this view not leave us open to a radical form of relativism?”⁶

At this point a parallel can be drawn with debates over the profession of history in the period of the Enlightenment. Enlightenment philosophes opposed two forms of historiography that were dominant at the time. One was the poetic or mythical (including Christian) history writing, which was too heavily based on faith and imagination instead of proof. The other was the old form of

² This pun stems from Alan Ross Anderson, as quoted in Kitcher (1992) p.54.
¹ See Alder (2002) and Jardine (2003).
⁵ Two issues made these debates vehement. The moral issue was: who pays the most respect to others when they have different views from ours? The authoritative issue was: who owns science, natural scientists, historians, philosophers or sociologists?
scholarship, the so-called ‘erudition’. What an erudite historian had to offer was a learned collection of notes, or fait divers, but he was lacking a clear method of organisation of his material.

The philosophes pleaded for a thorough change in the approach to history. They argued that historiography should become a critical science (‘science d’histoire’), through problem-oriented research. The presentation of solutions to these problems had to be in the form of an argument, the typical expression of this being the dissertation. The critical methods sustaining these arguments, and hence the interpretation of the past, should be derived from philosophy. Therefore historiography had to become philosophical if it was to become a respected science.

In the present context this surely is an overstatement. However, in my view, we must appreciate the point that a role for philosophy is indispensable in any approach to past science. This is especially true when it comes to securing a place for a sophisticated form of evaluative historiography. Historians of science, wary of philosophical interference, should not close the book at this point. The present endeavour is by no means an attempt to fence in historiography of science and turn historians into servants of philosophers. The general aim is to improve historiographical output and the thesis contains a number of suggestions to this end, hence historians of science form the primary audience to which this thesis is directed. Although the subject of this dissertation is the writing of history, philosophers of science can find valuable things here as well. After all, normative philosophical models have to square with historical reality in order to be worthy of pursuit. Hence a thorough discussion of perspectives on past science should be of their interest too.

2. Overview of individual chapters

Each chapter of this thesis contains results of its own, but together they add up to one argument. As far as I am aware, this dissertation provides the first systematic analysis to come to a sophisticated form of evaluative historiography. The argument consists of three parts: an historical part (chapters 1 and 2), an explorative part (chapters 2, 3 and 4) and a solution part (chapters 5, 6 and 7).

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7 Verschaffel (2002). There is thus an interesting self-referential aspect to this dissertation: it is about approaches to past science but at the same time, qua mode of presentation, it is a product of debates over approaches to past science earlier in history.

8 Earlier versions of chapter 4 and the first part of chapter 1 have been published as Karstens (2014a) and Karstens (2014b).
Chapter 1 contains a description and analysis of the decline of evaluative historiography. It starts with an overview of the aims and deeds of the first generation of professional historians of science. I argue that it is incorrect to set this historiography aside as naively Whiggish. Some of the aims and problems of the first generation continue to be relevant today, including, for example, the conceptualization of the relation of history of science to philosophy and to the natural sciences. Then the major change in approach to past science is described by looking at changes in vocabulary and in research topics. I discern a number of motivations and driving forces behind the transition. These can be captured in five main arguments against evaluative historiography (comprising a total of 14 arguments), which are theory-dependence, presentism, incommensurability, rule following and underdetermination. The arguments are further classified in a group questioning the very possibility and a group questioning the desirability of assessments of past science. The main task of the thesis is to disarm all these arguments. The possibility arguments require philosophical refutation, whereas desirability arguments require a beckoning perspective. I end the chapter with a list of arguments in favour of evaluative historiography. Under the right conditions evaluations can enhance historical understanding, provide the means to conceptualize scientific progress and provide criteria of selection, which help to endow history of science with critical functions again.

Chapter 2 contains a discussion of the principle of symmetry. This is guiding principle in historical interpretation, which can itself be interpreted in different ways. The discussion really is about what to recognize as topics of investigation and what as explanatory resources. Different formulations of the symmetry principle involve different conceptions of the topic-resource interface. I discuss the arguments in favour of the two most important formulations of the symmetry principle, namely the strong programme in the Sociology of Scientific Knowledge and the generalized version as adopted in posthumanism. Both involve a change in perception of knowledge from justified belief to authorized belief. Nonetheless, as is often not well recognized, the two approaches are really distinct and should not be lumped together. While it is hard to refute both symmetrical approaches by argument, we can point to a number of undesirable consequences for historiography of science that they yield. The currently en vogue posthumanism contains a number of good points, but historians of science today insufficiently realize the problematic aspects of this approach. I end the chapter by suggesting a ‘new relationalism’, which contains a different account of symmetry breaking, by looking at shifting relations between determining factors in past science. This is the first step
towards finding a new basis for evaluative historiography. It embraces a number of ideas stemming from symmetrical analysis of past science, but the approach I defend is in the end heterogeneous rather than symmetrical.

Chapter 3 is devoted to another interpretative tool, namely the principle of charity. Charitable interpretation involves seeking agreement with others, when no other clues are available. I defend the claim that the principle of charity is fundamental in understanding others. It works better than alternatives, such as the principle of humanity. Hence the principle of charity is constitutive of historical interpretation provided it is understood correctly. Interestingly, interpretations of charity range from its being a species of imperialism to a device in service of relativism. Neither of the two is correct. Charitable interpretation should be applied to the level of cognitive functions, intentional attitudes, etc. Expectations of rational behaviour should be gauged with respect to the historical circumstances. Finally, agreement first does not mean agreement last. Charitable interpretation triggers an interpretative circle, or a dialogue with the past, which may even lead to adjustments in our own concepts. It is a matter of continuously comparing alternative interpretations, and selecting the most suitable ones. All this is illustrated by a number of mistaken and correct applications of charitable interpretation in historiography of science.

In chapter 4 conceptualizations of the notion of error are discussed. I distinguish between two major conceptualizations, namely ‘errors as obstacles’ and ‘errors as failures’. ‘Errors as obstacles’ must be related to approaches to past science, which assume a demarcation between internal and external factors. ‘Errors as failures’ must be related to symmetrical approaches to past science. Both are problematic: the former because insufficient attention is paid to historical context and the latter because the analysis of failure is too much attached to particular contexts. Both have a clear story to tell when it comes to prospective error but not when it comes to the notion of retrospective error, arguably one of the most interesting aspects of the phenomenon! The strange conclusion is that history of science is without a good conceptualization of the phenomenon of error. In the final part of the chapter I indicate directions that need to be explored in order to fill this gap. First, the study of the phenomenon of error should be made part of a general philosophy of experiment in order to include the many levels on which errors can occur in scientific research. Second, I argue that we need to replace ‘removing errors’ as the main driving force of science with the idea that removing uncertainty actually is science’s main driving force. This makes assessments in the first place bear on persons instead
of theories and this creates the room to work with a wider theory of learning in science, which includes, for example, the phenomenon of the fertile error and the new analytical notion of ‘going amiss’.

In chapter 5 we take stock and consider what the exploration of concepts and principles of historical interpretation has brought so far with respect to the aim of disarming the five arguments against evaluative historiography. Some progress has been achieved but other things still need to be developed within the context of what I will call ‘extended naturalism’. The extended naturalism we seek must incorporate those desiderata, including most importantly a formulation of the concept of rationality and an articulation of a proper diachronic ‘zoom’ on the past. Another important result of chapter 5 is that common assumptions behind the positivist, or formalist, project in history and philosophy of science and the post-positivist, or naturalist, project are identified. It is these common assumptions that are responsible for the deadlock in the debates over approaches to past science. I argue that we need to overcome them and the natural way to do so precisely fits the desiderata we formulated earlier in the chapter. This gives a strong indication that we are on the right course.

In chapter 6 two candidates for extended naturalism are examined, namely normative naturalism and evolutionary epistemology. Both these approaches are primarily naturalist, but also have an evaluative dimension. They contain a number of good points, such as the focus on concrete problem situations, an evaluative approach in terms of virtues, a comparative evaluation procedure and the idea that strategies, methods and norms are themselves open to empirical testing. Yet, as a whole, both forms of extended naturalism fall short. Normative naturalism because it needs to assume demarcation between rational and social factors in order to maintain its normative thrust, and evolutionary epistemology because of its uni-directionality. It cannot account for science as a meshwork of diverging and merging paths of investigation and this is unfortunate. The solution to these problems is to expand the comparative horizon. Chapter 6 ends with notes on comparative historiography of science, which unfortunately never achieved the respect that it in my opinion deserves.

In chapter 7 I develop my own version of extended naturalism by putting forward an evaluative platform. The platform must be seen as a toolkit of analytical concepts with which the past can be approached. Moreover the platform serves as the ground for comparative evaluation. I believe that we have to accept that there are no absolute standards of evaluation. It follows that comparative evaluation is all there is and that when it comes to assessments of past science we should go comparative ‘all the way down’. The platform consists
of rational factors in terms of a set of virtues. These are defined in the thinnest way possible, namely through making a distinction between type and occurrence of the virtues, by not assuming a priori hierarchical relations among the virtues and by not demanding that all virtues need to be considered in every instance of theory choice. These soft constraints allow for a significant degree of pluralism but still provide sufficiently strong parameters for assessments of past science. Next to this the platform consists of a diachronic view on the past in terms of a collection of research programmes. I argue that we can make productive use of the benefit of hindsight in this way. Moreover, allowing ourselves to consider past episodes as phases in the development of a research programme takes the sting out the argument from underdetermination. Finally, I consider how anachronisms and present-day knowledge can be used as tools of explanation. Both are defensible, but only when applied in the same comparative circle of interpretation as defended in chapter 3. Chapter 7 effectively presents history of science with a research programme as much more can still be learned of typical patterns of virtue preferences and also of the role uncertainty plays in science.

In the conclusion I review how the five main arguments against evaluative historiography have been disarmed and list which shifts in perspective and conceptual innovations have been needed to achieve that. Among these are a focus on the typical as an intermediate level between the universal and the particular, a relationalist stance, the perspective of uncertainty and a thoroughly comparative approach to evaluation.

3. Revolution?

Earlier in this introduction a comparison was made to the philosophes of the Enlightenment, who opposed unsatisfying forms of historiography. A common interpretation is that the ideas of the Enlightenment may not have caused the French Revolution, but that these ideas nonetheless had an enormous impact on the new forms of government, which were established thereafter. The slogan of the new form of government became ‘liberté, égalité, fraternité’. This slogan corresponds surprisingly well to chapters 2-4. Symmetry: accounting for scientific products with reference to the same type of factors, resembles égalité. Charity: seeking agreement with others, amounts to fraternité. And considering that the original meaning of ‘errare’ is to wander freely, error corresponds to liberté. Without possibilities to go wrong there is no freedom of action. Hence freedom of choice always entails the risk of failure.
Zammito compared ‘recalcitrant logicists’ to “returning aristocrats of France after the Great Revolution, they have learned nothing and forgotten nothing.”
Indeed, we will not be calling here for a return to the old positivist project in order to facilitate evaluative historiography. Yet, do the shifts in perspective and the conceptual innovations call for a revolution in the study of past science? And considering that the original slogan was ‘liberté, égalité, fraternité ou la mort’ (see figure 1), is historiography of science indeed destined to perish if it does not follow in these footsteps?

![Image of a placard text from 1793](image)

Figure 1. Placard text from 1793.

Probably not. I have avoided the pitfall of opposing any approach to past science outright. Instead a golden mean was sought, combining as many positive aspects of *prima facie* incompatible approaches as possible. It follows that I think that a lot of the historiography of science that is produced today is valuable. Yet, a sense of crisis is nonetheless present in the field and has to be averted. In what follows I hope to convince historians of science, who believe that assessments of past science stand in the way of understanding the past, that the opposition between judging and understanding is incorrect and that there actually exists a fruitful niche for evaluative historiography. If historians are willing to do research along the lines suggested in this thesis, I am sure that this in the future will lead to exciting new interpretations, insights and unexpected discoveries.

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10 It is interesting that the Greek word ‘krisis’ actually meant decision or judgement. Gradually the meaning of the term has shifted from the moment of decision to the process leading up to the decision. In modern times ‘krisis’ got attached to the sociopolitical sphere with the latter meaning. The original meaning of crisis as judgement survives in our word criticism. See Koselleck and Richter (2006).