Pluralism within Parameters : towards a mature evaluative historiography of science

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Towards a Mature Evaluative Historiography of Science

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PLURALISM WITHIN PARAMETERS

Towards a Mature Evaluative Historiography of Science

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Introduction

1. The problem

Current historiography of science is for the most part a non-evaluative discipline.[1] 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.[3] 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-facetted 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

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[2] This pun stems from Alan Ross Anderson, as quoted in Kitcher (1992) p.54.
[3] See Alder (2002) and Jardine (2003).
[4] See Latour (2004).
[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?
[6] Giere (1985) p.341.

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.[7] 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.[8] 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).

[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 is 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, egalité, fraternité'. This slogan corresponds surprisingly well to chapters 2-4. Symmetry: accounting for scientific products with reference to the same type of factors, resembles *egalité*. 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."[9] 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, egalité, fraternité ou la mort' (see figure 1), is historiography of science indeed destined to perish if it does not follow in these footsteps?

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.[10] 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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[9] Zammito (2004) p.122.
[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 socio-political sphere with the latter meaning. The original meaning of crisis as judgement survives in our word criticism. See Koselleck and Richter (2006).

Chapter 1 The Decline of Evaluative Historiography

1. Introduction

This chapter offers a general introduction to the phenomenon of the disappearance of evaluations of past science in terms of empirical adequacy, validity, rationality and progress, in professional historiography of science. The chapter is organized as follows. First the aims of the discipline before the decline of evaluative historiography set in are discussed. Then there follows an overview of the major changes in the approach to past science, which can be illustrated by choices of new research subjects and the emergence of a novel conceptual apparatus used in historical explanation.

Behind these changes a set of motivations and a set of philosophical arguments can be discerned. The motivations stem from feelings of discomfort with modernity. As science is perceived as a cornerstone of modernity, one has to question it in order to undermine the modernist 'project'. Next to these motivations, a total of fourteen arguments can be compressed into five main arguments. Most of these arguments will sound familiar. What is perhaps new is bringing them together and also bringing them to bear explicitly on the writing of history of science. I cluster the main arguments in two groups. There is one group of arguments questioning the desirability, and another undermining the very possibility of evaluative historiography.

The main goal of this thesis is to disarm these arguments. In the process of disarmament we will discover what kind of evaluative historiography is feasible. The last section of this chapter is devoted to arguments in favour of this project. In light of the objections raised against evaluative historiography, which taken together make a formidable impression, we need strong motivation to take issue with them. The gains that lie in stock for historiography of science, which are summed up in the final section, provide this motivation.

2. Aims of the field in the first phase of institutionalization

The first signs of the institutionalization of history of science as an academic discipline were the first international conference on the history of science held adjacent to the World Exhibition in Paris in 1900, the start of the journal *Isis* in

1912, and the founding of the History of Science Society in 1924. This journal and the society still occupy a prominent position in the field today. The period also saw the first chairs, textbooks and specialized courses in history of science come about. To be sure, works on the history of science were written before the 20th century. As a matter of fact historians operating in the first half of the 20th century reacted to the philosophically informed views on the historical development of science of for example Comte, Whewell, Mach, and Duhem.[11] Nothing ever starts in a void. Yet in the 19th century nothing resembling a modern academic specialization came about.[12] For this reason I have chosen the early 20th century as a starting point of this overview of the development of the field.

The central figure in the first phase of the institutionalization of history of science was George Sarton (1884-1956).[13] Sarton was of Belgian origin but gained recognition as a professor at Harvard University. Sarton taught history of science there for many decades. Among his students were important later scholars in science studies, such as I.B. Cohen and Robert K. Merton. Sarton was also instrumental in Alexandre Koyré's move to the US, where he became highly influential. Sarton set up the journal *Isis* and was its editor-in-chief for many years. He was also involved in the creation of the History of Science Society. Sarton's role in the institutionalization of the discipline is widely recognized. The arguments and motivations he gave for a thorough study of the history of science by way of a specialized academic discipline form an interrelated whole that is worthwhile to unravel. Sarton's views will also be compared to the views of his contemporaries; this comparison yields a good picture of the prevailing ideas with respect to the study of past science during the first phase of its professionalization.

Sarton saw science as the only human activity in which progress had been achieved. According to him the progressive force of science had also had profound effects on society, especially in modern times. It was through science that the improvement of living conditions became possible, and science also

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[11] See Sarton (1952) and Kragh (1987). Contributions to historiography of science by Whewell, Duhem and Mach are discussed in Cohen (1994) pp. 24-53.

[12] For example, no specialist training for students was available. None of the best-known historians of the early period such as George Sarton, Charles Singer, Alexandre Koyré, Lynn Thorndike, Hélène Metzger, Edwin Burtt, Eduard Dijksterhuis, Anneliese Maier, Walter Pagel, Reijer Hooykaas or even Thomas Kuhn was educated as a specialist in history of science, or even in history in the case of most of these.

[13] Valuable insights in the first phase of institutionalization can be found in Pyenson and Verbruggen (2009).

showed the way to improve the organization of society. Furthermore Sarton saw the striving for pure knowledge as a moral quest. Good scientific research was a disinterested search for the truth and this attitude brought about the most outstanding achievements the human mind was capable of.

For all these reasons Sarton argued that the scientific enterprise had to be dealt with with great care. If science fell into the wrong hands, or if it were practiced in the wrong way, this could only be harmful to society. A mistaken approach to science was not just a symptom of a bad regime: it could well be conducive to wrong political systems.[14] Another danger was that scientists could start to overrate themselves and the importance of their contributions. Such hubris needed a check at all times. In pre-modern days people were kept in check by a clear social hierarchy: the church or the nobility. In modern times such social hierarchies, and the institutions connected to them, have only a marginal hold on people. Thus other forms of (institutional) control were needed.

Now Sarton firmly believed that modern historical consciousness could replace the older forms of social control if this consciousness were embedded in a spirit of 'new humanism'. The old humanism had known three categories: the natural, the human and the superhuman. The superhuman was thought to be the highest category, the natural to be the lowest. It followed that the study of man was also more important than the study of nature. This needed to be changed, according to Sarton. In his 'new humanism' the superhuman category disappeared and the study of man gained equal status with the study of nature. Only the combination of study of both fields could save humanity from the so-called technocrats: scientists who had become specialists in their own fields, who had no respect for the humanities and a total lack of appreciation of the unity of science. According to Sarton the role for history of science in bringing this new humanism about was crucial: "Between the old humanist and the scientist, there is but one bridge, the history of science, and the construction of that bridge is the main cultural need of our time."[15]

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[14] Sarton opposed both communist and fascist systems. After the war he saw his opinions confirmed when the horrifying scientific experiments of the Nazis became known. He also went as far as to say that Hegel was not well versed in science, because he had declared in his dissertation that the number of planets could not exceed seven *on principle*, and thus it was not a coincidence that his dialectic philosophy led via Marx to communist ideologies: Sarton (1952) 2nd lecture.

[15] Sarton (1931) p.72. The plea for 'new humanism' is much less quoted but essentially similar to Snow (1959). Even the political concern with totalitarian systems is similar:

Why was the history of science needed for this? Why not just concentrate on the products of modern science? Sarton answered this question by pointing to the complementary tasks of science and historiography of science. For him there was no principled difference between these fields. Both science and the study of its history worked towards the same goals, namely the gain of knowledge. It is only that the tasks of historians and scientists were different. Whereas the scientist investigates nature and comes up with experimental and theoretical results, the historian should act as a critic of the products of the scientist, as art critics value the work of artists.[16] Continuous criticism is the most important check on the dangers involved in the growth of knowledge and technology. On top of debates held within the scientific community, the historian was in a position to deliver such permanent criticism because he or she possessed the scholarship to place the products of the scientist and the debates held in scientific communities in a larger perspective and evaluate new contributions to science in light of the foregoing tradition. In this way historical understanding and current scientific research met, and only a professional historiography of science could provide the required bridge between them.

Apart from being a critic, the historian also had to act as a guardian. One of the main tasks of historiography of science was to establish the good tradition and do away with things that do not belong to it such as superstitions, undeserved privileges, (wilful) error, etc. This good tradition was not a story of immutability. Tradition for Sarton was a *dynamic* force, not an endless repetition of the same behaviour. He saw the good tradition as a sequence of the right steps. Only when this good tradition was safely protected could new discoveries and new claims to knowledge be assessed properly. The historian thus had to be evaluative with respect to past and present but not in the simplistic sense that everything in the past that contradicted present day knowledge should be considered bad science. Science progresses towards the truth but this road is difficult and almost every scientific theory so far has proven to be subject to revision. The *methods* by which this constant revision is possible are thus of central importance to the whole endeavour.

It was equally important that the historian highlighted the human dimension in this process. Historians had to concentrate on the non-linear development of science. Gathering knowledge about the world was a difficult process, which required a lot of effort. In the long run, with the benefit of hindsight, it is

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Sarton wrote against fascism and Snow against communism. The similarity is also noted by Cohen (1988). In *The Two Cultures* there is however no reference to Sarton at all.

[16] Sarton (1952) p.14.

possible to see patterns and logical sequences but when focusing on shorter time spans great struggles can be seen, hard work, wandering down wrong paths, periods of puzzlement and conflicts with others, victories and losses, etc. For Sarton it was one of the historian's central tasks to highlight these struggles in narratives about the past. 'New humanism' thus also meant that the human effort, that unravelling nature's mysteries required, gained the highest respect:

> "The New Humanist is of all men the one who is most conscious of his traditions and of the traditions of mankind. He admires the wonders of science but the greatest wonder of all he reflects is that man revealed them."[17]

Finally historians of science should become, perhaps somewhat paradoxically, specialists in generalization, and form long-term views lending unity to all scientific efforts. The general picture should act as an antidote to today's delusions and create the right attitude to science by yielding effects of moderation, patience and most importantly humility. Scientists are after the truth and their theories are converging towards it but they need to bear in mind that their ideas are continuously revised. Knowledge of the history of science can help to critically evaluate present scientific ideas.

In education the historian had to teach what science was about, its function and methods, its psychological and sociological implications, its deep humanity and its importance for the purification of thought and the integration of culture. In order to do all this a historian of science had to be an expert in history, have a good command of the state of the art in the sciences of both his or her own day and the past, be able to interpret past sources well (Sarton was also an empiricist), and also be a good writer and an able teacher.

Taken together, and especially in an age of rapid discipline formation, the task was obviously impossible for a single scholar to perform. This is precisely the reason why Sarton put so much effort in the institutionalization of the field. The unearthing of the sources and the interpretations of all episodes of the history of science for the most part still needed to commence. A great number of people was needed to perform such an abundance of detailed research projects. Good communication channels and an institutional platform for bringing all this knowledge together, passing it over to future generations and eventually building syntheses out of them, was therefore absolutely necessary. Otherwise historiography of science could never perform the important tasks Sarton had placed on it. And since these tasks were for him of crucial

[17] Ibidem p.13.

importance to the well-being of mankind as a whole, he devoted much energy to the legitimatization of the study of history of science as a separate discipline and the institutionalization of the field in the academic world.

According to Kragh, Sarton's programme was never carried out in practice.[18] There is however serious reason to doubt this. First the well-known historian of science A. Rupert Hall noticed a great influence of Sarton's ideas in Cambridge at the onset of professional historiography of science there:

"The broad notion of the literate scientific culture, at once rigorous and humane, agnostic and experimental, which Sarton called the New Humanism, had become widespread during the first half of the century."[19]

Apart from such direct references the similarities between Sarton's programmatic writings and the work of other historians such as Alexandre Koyré (1892-1964), E.A. Burtt (1892-1989) and E.J. Dijksterhuis (1892-1965) are striking. These historians did not write lengthy programmatic essays but were all occupied with establishing the good tradition, with the issue of humanism and with the philosophical dimension of science.

In the writings of both Koyré and Burtt we find the view of science as the most successful movement of thought history so far records. But in both there are also comments on the downside of the progress that modern science has brought, not unlike Max Weber's ideas on the disenchantment of the world. According to Burtt, the dominance of modern science in Western culture had led to a downgrading of the human spirit. It was the central task of philosophy "to reinstate man with his high spiritual claims"[20] rather than let him become a mere entity reducible to the atomic categories of modern science. The remedy for this, according to Burtt, was to reconnect science in each historical period to the philosophical or metaphysical ideas that reigned supreme in these periods.[21] The quest for scientific knowledge could be properly understood only in connection to these philosophical schemes. Hence the title of Burtt's main work, *The Metaphysical Foundations of Modern Physical Science* (1924). The fear of dehumanization is expressed in other terms than in the work of Sarton, who considered the scientific process dangerous, not in itself, but only if the control

[18] Kragh (1987) p.19.
[19] Rupert Hall (1984).
[20] Burtt (1954) p.25.
[21] Burtt was opposing logical positivists here who declared all kinds of metaphysical issues to be philosophical pseudo-questions. An interesting contextualization of this attitude can be found in Galison (1990).

of science fell into the wrong hands. Yet the remedy both came up with was strikingly similar: a firm connection between the sciences and the humanities was needed in order to benefit most from the advancement in scientific knowledge.

Koyré's ideas can easily be compared to the views of Burtt and Sarton. Like Burtt, Koyré too argued for the importance of studying philosophical schemes of the past. For him general mental frameworks could not be separated from scientific research: in every period scientific thought must be related to the 'thinking cap' prevalent in the period. In effect this can be seen as the start of contextualism in historiography of science.[22] What made the 17th-century breakthrough in science possible for Koyré was a change from Aristotelianism to Neo-Platonism. The latter brought with it the idea that reality was to be captured in mathematical terms. From a world of 'more or less' people started to live in a universe of precision. It was chiefly Galileo who brought this revolution about. For Koyré, Galileo's approach to science was basically the right one.[23]

The praise for Neo-Platonism was shared by Dijksterhuis, who launched the idea of a mechanization of the world picture, which can also be read as a *mathematization* of the world picture.[24] A considerable difference of opinion with Koyré was that Dijksterhuis did not consider reality as fundamentally mathematical. He saw mathematics as a way of describing that gets us as close to reality as we can. Yet they both had a decidedly anti-positivist attitude, as can be surmised from Koyré's slogan "good physics is done *a priori*." Duhem had still considered mathematics as of second-order importance but in the hands of Dijksterhuis and Koyré it became the central feature of modern science. They downplayed the role of experiments, which they saw as the verifications of hypotheses. Thus there was a great difference with Sarton who very much wanted to align history of science to the positivist project in philosophy.[25] Yet what is most important for the present purposes is to see that they all tried, in very different ways to be sure, to relate the study of past science in one way or

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[22] As pointed out in Cohen (1994). The idea of 'thinking cap' was further developed by Thomas Kuhn into the notion of paradigm.
[23] See Koyré (1978).
[24] Dijksterhuis (1950).
[25] In a letter to Cecil H. Desch dated 18 April 1926 Sarton wrote: "It is because of Comte, that I became a historian of science–and at the beginning of my scientific life I was tremendously influenced by the Positivist ideal. When I started the publication of Isis in 1912, I was still largely dominated by Comte's thought." Quote taken from Dibner (1984).

another to philosophy, thereby finding a way to distinguish the good tradition in science from the bad.

Cohen has summed up Koyré's achievements in the following way:

"Let there be no mistake about it: Koyré had a most powerful message. It had all the strengths of a unitary account in which, through the magnetic action of the core conception, a huge number of hitherto unrelated historical facts were now arranged, like so many iron filings, along neat lines of force."[26]

That core conception was the Scientific Revolution, a term coined by Koyré. In Sartonian spirit Koyré's unitary account combined the search for the right place of the human aspect in the development of science, the aim to distinguish the good tradition from the bad, and an attempt to secure a fundamental place for philosophy in historiography of science. The similarities in outlook on the goals and aims of the new profession are striking. The execution of them led to a number of challenges that, as will be argued, have remained pressing, especially for the study of past science in our day.

3. Challenges to the profession

Gradually historians started to doubt the value of sketching 'big pictures' of the history of science because these appeared to be centred too much on the present instead of the past. Therefore, stories of the good tradition, spanning long periods of time, possibly held together with unifying concepts, are nowadays invariably set aside as 'Whiggism'. In general one speaks of Whig history when the historical process is accounted for with present outcomes in mind.[27] This lends the historical process a form of necessity it did not have and for this reason presentist schemes for the organization of historical narratives are mostly rejected. Next to presentism three other Whiggish 'sins' can be associated with history of science, namely judgementalism, triumphalism and internalism.

All these four aspects of Whiggism apply to the historiography of science that was produced until roughly the 1960s. It was presentist in the sense that historians sought to establish the string of ideas that had led to the present-day

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[26] Cohen (1994) p.79.

[27] In the words of Herbert Butterfield: "[The Whig interpretation of history] ... is the tendency in many historians to write on the side of Protestants and Whigs, to praise revolutions provided they have been successful, to emphasize certain principles of progress in the past and to produce a story which is the ratification if not the glorification of the present." Butterfield (1931).

state of scientific knowledge. It was also presentist in its objectives: to keep contemporary developments of science in check by historical awareness. It was judgemental in the sense that the past was studied with an idea of the correct way of doing science in mind. It was triumphalist, as science was seen as potentially beneficial to society and achievements in science, especially during and after the Scientific Revolution, were seen as the highest achievements of mankind. It was internalist in the sense that an influence of the so-called 'external' factors, such as social, economic, political and cultural factors, on the course of science was hardly recognized. Although a start of contextualism can be witnessed, this contextualism applies to the wider mental, spiritual or philosophical context only, and certainly not to the socio-cultural context.

It comes as no surprise then that historians who started to put emphasis on an analysis of the socio-cultural context, set aside the work of their predecessors as Whiggish. A recent example is Steven Shapin, who sees in Sarton no more than a triumphalist. He explains this triumphalism by Sarton's institutional aims. The high achievements of the past were a good selling point for the study of history. When historiography of science had become a recognized discipline in the university system, Shapin argues, there was no longer any need for exaggeration and a process of 'lowering the tone' could begin.[28] Perhaps there is some truth in this analysis but in general I believe it can be harmful to brush off the older historiography of science as Whiggish with the implication that the work, as well as the incentives behind this work, can be considered irrelevant. In this way one loses sight of important motivations to engage in the study of past science and equally important deliberations on the discipline's aims and the prospects of, and problems in, achieving these aims.

The first generation of professional historians of science certainly was not naively Whiggish. There was keen appreciation of the struggles of past scientists and their modes of thinking. Not everything that must be considered wrong from the standards of present-day science was simply rejected as uninteresting or as bad science. Being an important link in the chain towards the present was the most important criterion to assess the science of the past. Progress was also not seen as a linear, gradual and smooth process, as an extreme Whig account of past science would have it. Instead, sharp breaks were recognized by the early group of historians of science. Developments in physics in their own time made them acutely aware that even the most well-established theories, in this case of Newton, could be radically overthrown. This in fact spawned historical attention to questions of continuity and change. Burtt for

[28] Shapin (2010) pp.3-14.

example questioned Duhem's continuity thesis in the development of science from the 13th century to the 20th, Sarton reflected on the notion of tradition which he came to interpret as a dynamic force and Koyré came up with his idea of regular overall changes in mentality. All these ideas shifted the debate from a black-and-white discussion of continuity versus discontinuity to a more sophisticated discussion on relative (dis)continuity. Differences of opinion among historians of science existed about the measure and importance of such relative (dis)continuities. The overall view of science as a progressive force was not challenged in these debates. Yet scientific progress was not seen as a smooth line of development towards the present and also not as something with only beneficiary effects to society.[29]

Decomposing the concept of Whig history into four aspects, presentism, judgementalism, progressivism/triumphalism and internalism, may be the right strategy to allow for a more balanced approach to 'Whiggism'. I believe that it is possible to formulate interpretations of these four aspects without turning them into a 'Whiggish sin'. I side with other scholars in science studies, who have argued that such a balanced approach is needed. Elzinga, for example, wondered whether the standard reproach of Whiggism has always been fortunate: "The reproach has become a shibboleth, which is placed in position against anyone who claims that science is in progress at all."[30] Similarly Hon et al. (2009) have argued that the fear of being Whiggish has been holding historians back in the study of errors in past science, thereby leaving a vast territory of interesting historical questions unaddressed.[31]

In this context it is important to recognize that the term Whig history, with reference to the historiography of science, came into use only from the 1970s onwards.[32] This suggests that the usage of the term has played a role in the legitimatization of new approaches to past science (see section 4 below). It is not that criticism of the earlier historiography was unjust, but dismissing former approaches as naively Whiggish appears *also* to have been a rhetorical strategy. This strategy leaves all kinds of questions unaddressed. If triumphalism was

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[29] This is not to deny that extremely Whiggish histories of science exist. My claim is however that this writing has hardly occurred in professional historiography of science. Extreme Whig history is most often produced by practicing scientists turning to the history of their field or in popular science books. A clear example of the former is Chomsky (1966), an example of the latter is Bryson (2003).

[30] Elzinga (1987) p.267.

[31] Hon, Schickore and Steinle eds. (2009) p.3.

[32] In Jardine (2003) the first usages of the term Whig history in relation to science are traced.

needed to legitimize the field, the question why the historiography of science was needed as a separate academic field is still open. Why were the concerns with humanism so strongly present in many scholars? And if the present was thought so important why was a study of the history of science needed? Why not just devote all effort to present-day science? Only by zooming in on the 'unitary' accounts of scholars like Sarton and Koyré can we find answers to such questions. The uptake is that a number of challenges to the profession that can be extracted from this, have lost none of their relevance today. These challenges have an important feature in common: they are all about striking a right balance between generalism and specialism.

The first of three challenges is the tension between the universalist aspirations of historiography of science and the increase in specialization in the natural sciences. How to satisfy the aim of maintaining a general overview of the whole development of science if it becomes so enormously complex? Even groups of historians cannot keep up with the specialism required to understand what is going on in all the separate disciplines and sub-disciplines.[33] The issue of unity between past and present is at stake here as well as the issue of unity between science and history of science, which especially Sarton envisaged as complementary endeavours.

A gap in expertise was already felt pressing before World War II. The continuing relevance of the problem can however be seen in the so-called 'science wars' in the 1990s. Historians had taken a radical solution to the problem and declared the universal programme to be mistaken. The solution thus was to break off the study of past science from present-day science, for example by forbidding the use of present-day scientific knowledge in historical explanation. As a consequence historians or sociologists of science were blamed by natural scientists for not having a clue what they were talking about in terms of the content of scientific theories. On the other hand natural scientists were blamed by historians for ignoring the impact of contextual factors on the course of science. Both camps argued that the other camp was, through fundamental ignorance, not in a position to offer serious critique, analysis or interpretation of past and present science. The way the science wars were conducted strikes one as not very fruitful and the debate in the end was quite inconclusive.[34]

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[33] As Floris Cohen once pointed out to me, lack of scientific background is perhaps the most profound reason why 'mainstream' historians *en masse* leave out science as an important determining factor of the history of mankind.

[34] In the collection of essays in both Koertge ed. (1998) and Segerstråle ed. (2000) an attempt is made to strike a balance after the storm had died down. The former is still quite belligerent, while the explorations to move beyond the 'wars' in the latter remain

Sarton's worst fear of a separation between the sciences and the humanities appeared to have come about!

The second challenge involves the tension between the generalist attitude of the historian and the need to stay close to the sources. For Sarton generalization over detailed case studies, i.e. the creation of larger historical syntheses, was justified only after work on the primary sources had been done. Since in later generations the number of relevant factors to consider expanded dramatically (see also section 4 below) the number of sources to take into account expanded as well.[35] A decidedly empirical attitude then blocks the way to setting up a grand historical narrative about science. Sarton could still maintain a relatively simple conception of the good tradition and how to account for it, but such a thing is no longer possible today. The question the field has yet to answer satisfactorily is what the proper level of generalization is and how to connect this to detailed studies of source material tied to historical localities. In sociology this problem is known as the problem of micro and macro.[36]

Thirdly, the question is how to envisage the relation between historical study of science, with its emphasis on the specific and concrete, and philosophical study of science, with its emphasis on the general and abstract? During the first period of professionalization the ties between the fields were quite strong. Sarton sought a connection to positivism; others sought to connect past science to its metaphysical backgrounds and held the turn to Neo-Platonism in high esteem. Moreover all historians were involved in the philosophical project of setting up criteria for what good science consists of and this model of good science was surely meant to be context independent. Philosophical thinking led to overarching normative models of science. Historical research was done in order to demonstrate or test such philosophical models. Today most historians abhor such historiography and the consequence is that philosophy is kept at a distance.

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inconclusive. Perhaps for this reason it is argued in Maienschein (2009) that, especially for the issue of the conceptualization of the relation between science and the study of its history, the work of Sarton is of continuing relevance.

[35] The order of magnitude was also enlarged by the very number of scientific disciplines to consider. Dijksterhuis for example wanted to expand the horizon to other fields than physics alone. This came into conflict with maintaining the unitary account of the development of science over a longer period of time. As Cohen writes: "The dilemma was certainly not one that Dijksterhuis alone had to confront. The more one wishes to do justice to all the sciences of the 17th century, the more one tends to lose a clear view of what it was that distinguished early modern science from its predecessors." Cohen (1994) p. 72.

[36] See Wiley (1988).

On all these three issues the balance has clearly shifted towards specialism (see next section). Therefore any attempt to again include more generalist perspectives on past science in historiography, has to face the three challenges of striking a balance between generalism and specialism given above.

4. Changes to the profession

After World War II attention for the study of the history of science grew considerably. In the US chairs were created at the insistence of not only university policy makers but also general politicians.[37] Yet according to Kuhn professionalization in historical scholarship proceeded slowly. In a tribute paper to commemorate Sarton, Kuhn offered some valuable insights into the status of the field of the historiography of science in the 1950s. In hindsight he felt the field offered an amateurish sight. There were regular meetings of the History of Science Society but these were relatively small conferences, sometimes even held at the home of one of the members of the society. Moreover Kuhn himself was appointed lecturer in history of science while having had no training in that field whatsoever. He observed that such a thing would be impossible in the 1980s. Kuhn did not express any nostalgic feelings about this period and claimed that, in spite of all the efforts before, the professionalization of the discipline started only when his generation set to work.[38]

This much-needed increase in historical sophistication occurred against the background of changing perspectives on the benefits of science for mankind. The feeling of a loss of control led to anti-science sentiments. Science was now also seen as standing at the root of many negative things such as dehumanization, alienation, pollution and atomic warfare. Perhaps the gain in scientific knowledge did not automatically lead to improvements in human life. Perhaps scientific progress was a myth.[39] Because it put science on a high pedestal, making it something sacred and inaccessible, the universalist ideology was seen to be part of the problem and not of the solution, as the older

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[37] James B. Conant was one of the main driving forces behind this process as mentioned in Westman (1994). Kuhn also mentioned the profound impact of scientific findings on the course of the war as a motivation to increase attention for history of science. A useful survey is Dauben, Gleason and Smith (2009).

[38] Kuhn (1982).

[39] Von Wright (1993). While technological mastery of nature and accumulation of knowledge are undeniable according to Von Wright, it is doubtful whether this had led to a better life in terms of morality and feelings of well-being. A mistaken belief in progress manifested through technological, rational and bureaucratic systems could lead to degrading modern man to no more than a wheel in a mechanic system.

generation of historians of science had thought. Evaluative historiography was perceived as belonging to an unqualified progressive and modernist perception of the development of science. It is in this context that we must place Latour's consequent attacks on what he calls 'The Modernist Settlement'. To address the fear of loss of control it was necessary to lower the tone of science.

The concern with improving society, that was clearly present from the beginning of the process of professionalization, remained, but became articulated in a completely different way. Collins and Pinch (1993) for example use the Golem as an image for science and technology. A Golem is a forceful, human-like creature that stems from Jewish mythology. It is created by man, but can get out of control because it does not know its own strength. To tame it and channel it in the right direction requires constant attention.[40] It is important to realize that the contextualist project in science studies is not a simple manifestation of the anti-scientific attitude. Rather it is an attempt to challenge the conception of unqualified progress, which is however still motivated by a desire to improve society. Only a deeper understanding of the processes of knowledge formation would provide the scholarship needed to keep science in check and control its possible harmful effects on society.

New approaches to past science invariably started to stress the contingent aspects of the historical process. The main idea became to understand past science on its own terms. Kuhn's notion of the paradigm demonstrates the shift in attention well.[41] On its most common interpretation a scientific paradigm is a 'closed' system consisting of theories, methods of research and standards of evaluation, but also of a set of values, principles and background assumptions. Especially this set is informed by the social and cultural factors dominant in a given period and/or place. All aspects together determine what normal science is within the paradigm. When a shift occurs to a new paradigm Kuhn argued that no cumulative process of increase of knowledge should be assumed, as the perspective on things, even on the most fundamental concepts, like time and space, could change dramatically. Thus the next paradigm should be understood on its own terms again, and so on.[42]

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[40] In film and literature scientists from Dr. Frankenstein to Dr. Strangelove often appear as figures that produce unmanageable monsters, the force of which may even surprises themselves. This *topos* can be traced further back, all the way to Prometheus: see Holton (1993).

[41] Kuhn (1962).

[42] This message was radicalized in Feyerabend (1975). For comparable analytical concepts such as Robert K. Merton's sociology of institutions or Stephen Toulmin's approach to scientific disciplines in terms of ecological 'niches', see below.

Older historians of science had been aware that reality was not easily mirrored in scientific theories but they did not imagine that the relationship could be so complex, with a multitude of contextual factors operating on it.[43] Yet even Kuhn's idea of science as moving from paradigm to paradigm can nowadays appear a bit old-fashioned. A paradigm for Kuhn could last very long, for example the Newtonian paradigm that held sway from roughly 1700 to 1900. Moreover, within a period of normal science the focus on scientific ideas is still quite dominant. Sociologists and historians after Kuhn found all this too schematic and too abstract.[44] They argued that historical research should focus on local circumstances and the concrete interaction of people and things. Ideas were not just floating in the air: their emergence and acceptance (or rejection) should be understood in terms of these interactions.

This development can be interpreted as a process of increasing naturalization of the study of past science. The term 'naturalization' refers here to the explanation of past science in causal terms. Description of relations in terms of cause and effect can be applied to the interaction of man and nature, to social interaction, to psychological processing and to the interplay of factors determining the historical process.[45] The idea is that the acceptance and rejection of theories about nature can be fully captured in causal terms. It only depends on the specific approach to past science which form of causal interaction is given the most dominant explanatory force.

The naturalist approach is opposed to the formal approach to the study of science. In the formal approach the aim is to establish a logical succession in the historical process of theory replacement. This yields context-independent norms of rationality. Further the goal is to identify justificatory relations in the total body of scientific knowledge by means of formal logic. The formal approach has close ties to philosophy and has been increasingly rejected in favour of naturalist projects in history of science. The use of analytical concepts and models in the explication of past science has increasingly been replaced in favour of a descriptive approach of following the actors, perhaps in the end not

[43] In thinking about science the whole mirror metaphor was cast aside in favour of constructivist points of view. See especially Rorty (1979).
[44] Note that Kuhn himself changed his position, but in the *opposite* direction. On the problem of incommensurability of paradigms he proposed that with respect to a number of virtues, such as simplicity, accuracy and scope, inter-paradigmatic comparisons were possible thus offering an alternative standard of progress. Thinking about scientific rationality in terms of virtues is an important idea, which will be explored in chapter 7.
[45] For naturalism in relation to the study of (past) science see above all Kitcher (1992).

far remote from the old Rankean spirit of 'bloss zeigen wie es eigentlich gewesen.'[46]

Together with institutional expansion there was an expansion of the field in terms of research topics. This can be read off from collections of papers presented at annual meetings of the History of Science Society 1957 and 1973, and a proceedings of an international congress held in 1961.[47] In 1957 papers on the following topics were presented: origins of classical mechanics, the discovery of energy conservation, the concept of electric charge, and the development of ideas on the structure of metals. Modest forms of contextualization are present in the volume, such as the relation of theoretical scholarship and craftsmanship, the relation between art and science in the Renaissance, the relation between science and politics during the period of the French Revolution, the maturation of biology in the 19th century and its relation to social theory. Another topic was how to teach the history of science, and which place should it get in the university curricula? This topic was on the agenda in the 1961 volume as well.

The 1961 volume witnessed an increase in theoretical contributions. Problems in the *sociology* of science were addressed by Kuhn, Hall, Polanyi and Toulmin. Problems in *historiography* of science were also discussed, such as the role of *a priori* assumptions in historical explanation,[48] and how to deal with forgotten or lost sources, i.e. with the incompleteness of source material. Topics further range from scientific thinking in Antiquity to the making of modern science. As in 1957 the main focus is on the fields of physics and biology. However in 1961 the contributions are ordered somewhat more chronologically and there clearly is much more theoretical interest and discussion about the direction of the field.

In the 1973 volume contains papers on Antiquity, Asia, alchemy, the role of industrialization (Wedgwood) and the ideological context of man's place in nature. Apart from physics and biology much attention is paid to the history of chemistry and geology. Very striking is however the increase in theoretical and methodological papers of this volume. Needham's comparative approach is critically discussed. But there is also a debate between Hesse and Rattansi on the use of evaluations in history of science, addressing the relation between philosophy of science and history of science. The very fact that this is a topic for discussion shows that evaluative historiography had ceased to be self-evident.

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[46] The same observation can be found in Kuukkanen (2012).

[47] Clagett ed. (1959), Crombie ed. (1963), Teich and Young eds. (1973).

[48] Note that in this context Guerlac first used the term Whig history in relation to history of science. Koyré's reaction to Guerlac's paper is interesting because it contains rejection of a number of Whiggish attitudes.

Especially Hesse's contribution is interesting because it is one of the first articles that seriously problematizes the role of evaluations in historiography of science.[49] Hesse aims to strike a balance between internalism and externalism.[50] According to her, science should be perceived as rational thought unfolding according to its own inner logic. Science does not operate in a vacuum but it can, in the end, be detached from external factors operating on its course. The historian's task is to clarify both logical and methodological relations and for this he or she needs to make use of evaluative categories.

All these considerations still stand quite firmly in the tradition of the history of ideas. A real turn in perspective only came about from the second half of the 1970s onwards when profound changes in vocabulary and in research agenda can be witnessed. We see a dramatic increase in the use of concepts such as context, culture, discourse, practice, instruments, trust, narrative, interests, internal and external factors, controversy, tacit knowledge and representation. For example 'context' has ca. 50 hits in the journal *Isis* until 1940, ca. 400 hits until 1970 and ca. 5089 hits until 2012. The same goes for 'practice', which has ca. 350 hits until 1940, 850 hits until 1970 and 4188 hits until 2012. These figures are significant even if we take into account that the publication rate and the sheer volume of the issues of *Isis* have expanded as well. Other analytical concepts were really innovations. 'Paradigm' for example has hits only from the 1950s onwards, 'self-fashioning' has no hits before 1990.[51]

In a useful survey Golinski has drawn up the most important lines of research that must be connected to this change in vocabulary.[52] The following themes dominate this agenda (I have added the turns): scientific and experimental practice (practical turn), focus on material circumstances: instruments and other objects (material turn), focus on linguistic practice, discourse analysis, communication, representation (linguistic turn), attention to the places of research (geographical turn), the institutionalization process of universities, scientific disciplines, etc., the interaction of science and the public and the self-fashioning of scientists. To this list a number of socially oriented topics (social turn) such as relations of trust and authority among scientists, relations between science and politics, the interwovenness of economic and scientific developments, etc., can be added.

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[49] Hesse (1973).
[50] For these views see also Hesse (1970).
[51] This is based on results of the search engine of Jstor. For the *British Journal for the History of Science*, started in 1950, a quick increase in the use of the same concepts can also be witnessed.
[52] Golinski (2005).

I think Golinski is right that this list of topics has become what mainstream historiography of science is occupied with. As a Belgian historian recently put it:

> "It is widely accepted among historians of science that the production of knowledge is first and foremost a localized process…, the localized setting plays a crucial role in understanding its conceptual and epistemic features."[53]

The focus is on understanding the way knowledge claims in particular, and the institution of science as whole, function in specific societies or specific historical contexts. The outlook on science as a unique, united and pure (i.e. timeless) endeavour has been replaced by perceiving science as a human activity, no more special than other human activities, disunited, both in time and place, and never pure, because always firmly tied to the specifics of the localities in which knowledge is produced.

5. A peculiar process of maturation

It is a bit strange that the dominant approach in historiography of science has come to resemble a form of Rankean historicism, the model general professional historiography started out with at the beginning of the 19th century, but that has long become obsolete.[54] How can we account for this? I discern two main driving forces behind the process of naturalization of the study of past science. Following Daston's idea that 'all epistemology is born in fear', these two driving forces can both be seen as being born in fear.[55] The first force I call the striving for liberation. By liberation I mean the freeing from dogmas, norms and standards, Eurocentrism, Westernization, elitism, etc. There is a moral component in this. In presenting the development of science as a highly contingent process, space is created to treat all actions and motives of past historical actors with equal respect.[56] Further, taking a distance from the modernist progressive project creates room for a critical engagement with this project, which staunch adherents of modernism are not willing to undertake. All this relates to a fear of objectivism: the dehumanizing force of science that destroys essential aspects of man's life. This was a thing that already bothered Burtt and Koyré. Perhaps it is ultimately a romantic fear, as one of the points of

[53] Van Paemel (2011).
[54] See Iggers (2005).
[55] Daston (2005).
[56] Nanda (1998) p. 287.

Romanticism was to free the individual from the mechanistic and deterministic schemes that Enlightenment thinking had produced.

The second driving force I will call the striving for exactness. An empiricist attitude of being as exact as possible in explications of past science shows itself in the naturalist approach, which focuses on concrete causal relations. Pointing towards 'influence' of persons, ideas or movements is not enough. These must be made concrete in patterns of interaction. This approach has gradually gained the upper hand in historiography of science. Grand narratives or large-scale comparisons are eschewed because they are regarded as speculative. This is the scientific side of the abolishment of *a priori* analytical concepts, or philosophically informed interpretive models of past science. Historians do not want to be reproached for being unscientific and therefore they insist on the most exact proof they can get. This fear of being unscientific relates to a more general fear of subjectivism. Once progressive ideals and transcendental standards are given up, there is nothing to go by, and hence the door is open for subjective speculation. Only an insistence on proof, in terms of concrete causal interactions, can keep this door to subjectivity closed.

The two fears are clearly different: one leads to a striving for less determination whereas the other leads to a striving for more determination. Yet, both lead to the same localist and descriptivist direction in historiography of science.[57] This becomes understandable if the striving for liberation is interpreted as an avoidance of prescriptivism. To let the past speak without fitting it into all kinds of straitjackets is not incompatible with an empiricist attitude of deriving theories from facts and being wary of too much theoretical speculation.

A conscious estrangement from philosophy and the natural sciences has occurred in connection to these developments. Historians of science no longer wanted to work in service of these disciplines and claimed independence instead. This however has proven more easily said than done. First, the social sciences have proven apt to serve naturalist projects in history of science. Interpretative models from the social sciences have started to exert a strong influence on the field and cannot easily be passed by. Second, while historicism has always served as a clear identity marker of historiography, we must wonder

[57] It is true that the network model of Bruno Latour offers a global perspective in which localities are connected to each other. Yet this is allowed only if these localities can be related through concrete interaction of actors. Therefore the global in Latour remains local no matter how big the network eventually becomes. For a more detailed discussion of this approach to past science, see the next chapter.

whether a return to a naïve historicism is not too high a price to claim autonomy for historiography of science.[58]

I am drawn to the conclusion that, in spite of the lofty ethical and scientific ideals, the desire to gain complete independence for historiography of science is mistaken. In my view, it belongs to the peculiarities of historiography of science that it really *is* a discipline that stands on a bridge between the natural sciences and the humanities. The subject matter historians of science have to deal with; the products of the sciences, together with the humanistic methods of studying and interpreting the past dictate this. Trying to gain independence by cutting off the bridge from the banks is therefore self-destructive. The challenge to gain independence is met, not by cutting ties with philosophy and the natural sciences but by reconceptualizing the relations between these fields. Not surprisingly this challenge is closely connected to the three challenges of striking the right balance between generalism and specialism.

The list of benefits the various 'turns' have brought to the study of past science is substantial. Many hitherto neglected aspects of science have gradually been brought into focus and as a consequence an altogether different picture of science, as a highly complex and many-facetted endeavour, has emerged. With respect to the evaluation of contributions to past science the burden has shifted fully from the present to the past. Claims to knowledge ought to be judged by standards operative in the context in which they were put forward and not by standards formulated in the present. As past standards of evaluation are not subject to critical debate, this has led to a high degree of epistemological relativism in historiography of science today. This relativism is supported by a number of philosophical arguments that will be considered carefully in section 6.

[58] It is therefore not surprising that attempts to declare independence have not been very convincing. Forman (1991) is a late cry to finally get rid of Whiggism. Jardine (2001) calls for an end to 'theoretical anarchy' by choosing one model of interpretation, namely the ideas of Norbert Elias on the civilization process. In this view historians of science should cooperate with other cultural historians in bringing about full understanding of this process. Yet, Elias was a sociologist and the dominant model then would still be sociological. Chang (2009) calls for an independent judgementalism for history of science which sounds attractive but he does not make clear what this judgementalism should be based on.

6. Arguments against evaluative historiography

6.1 Theory dependence

The problem of theory-ladenness is that there can be no direct, or neutral, observation of empirical facts. Humans approach nature with theoretical presuppositions and thus observation is always mediated. These presuppositions are both of a perceptual and of a conceptual kind. Our perception is influenced by presuppositions on a very basic cognitive level; the entities we perceive are mediated through higher- order theories, or conceptual systems. It follows that pure inductivism cannot work because pure inductivism presupposes that all theories are derived in bottom-up fashion from hard empirical facts. If we have to accept theory-ladenness of observation, which I think we are compelled to do, then in establishing the facts, other theories are already in use and hence new theories will always be dependent on older ones. Such considerations have led to a theory dependence tradition in philosophy of science that extends well beyond the problem of theory-ladenness of observation only.[59] I will first sketch the development of this tradition and then discuss the problems for evaluative historiography that follow from it.

A sketch of the theory dependence tradition in philosophy must commence with Francis Bacon. Bacon distinguished between four Idols: Idols of the Cave, Tribe, Theatre and Market Place.[60] The Cave refers to conditioning on a personal level through upbringing, schooling, life-experience, etc. The Tribe refers to conditioning due to limits set by general human cognitive capacities such as perceptual limitations, over-patterning, etc. The Theatre refers to conditioning through dogmas that are inherited from philosophy and theology. The Market Place refers to conditioning through language. We do not choose the language we speak, yet the language yields the categories with which we necessarily have to classify the world around us. In Bacon's view these Idols were the cause of false belief. He thought that surpassing the Idols was possible through the empirical study of nature. By performing experiments nature could be subjected to investigation, without the four Idols distorting the observational results, and through inductive reasoning theories could be built upon these observations.

[59] Chalmers (1999). See also Brewer and Lambert (2001).

[60] Bacon (1620).

Later philosophers started to question Bacon's optimism. Both Hume and Kant for example expressed serious doubts about the validity and absolute certainty of inductive reasoning. Yet the main flavour of empiricism was kept alive in the programme of logical empiricism. Logical empiricists thought they could circumvent Bacon's Idols through a formulation of a neutral form of language use. Logical empiricists viewed scientific theories as collections of statements. The key concept in their analysis of these statements was that of translation. First, perceptual data are translated into a neutral observation language (the verification criterion) and then from the neutral observation language, empirical facts are translated into higher-order theories. There is an acknowledgement in the logical empiricist project that our investigation of nature is not a straightforward matter and requires a minimum of theory. This theory is provided by assumptions about language and a set of logical principles. As these are formal tools, logical empiricism is one of the clearest expressions of the formal tradition in thinking about science, and has exerted considerable influence on the course of modern philosophy of science.

Logical empiricism was criticized for not being able to deliver an incontestable basic vocabulary, as well as for failing to deliver the needed translation mechanisms. In various ways philosophers tried to amend the project, while keeping the formalist flavour of it alive. Reichenbach, for example, made an influential proposal to distinguish between a context of discovery and a context of justification. While it was not possible to formalize the process of discovery, the process of justification of already formulated theories could be undertaken systematically. Philosophers of science, according to Reichenbach, therefore had to direct their attention to the context of justification only.

Popper formulated an influential deductivist, instead of inductivist, procedure of testing hypotheses in the context of justification. Replacing the verification criterion with a falsification criterion meant a reduction in the ambitions of philosophy. The project of vesting certain knowledge on logical foundations could no longer be carried out. No matter how many critical tests a theory is able to withstand, a theory is never completely certain, as falsification may in principle always be possible. That science is a rational endeavour is in Popper's framework safeguarded only by formal criteria such as the demand that theories should be well formulated, so as to allow for critical testing, and the formal testing procedure itself, which is based on propositional logic.

In the work of Reichenbach and Popper we see a reduction in the ambitions of the original logical positivist programme but the aim is still to interpret and qualify both the process and the products of science by formal criteria, which are taken to be context independent. The process of 'going bigger' in philosophy really starts with questioning the absoluteness of the standards of rationality which govern procedures of justification of knowledge. This shifted attention to the systems, or structures, which sustain such procedures. The scholar who should probably get the credit for being the first to expand philosophy of science in this direction is Ludwig Fleck, although his work initially was not recognized very well.[61]

In his own medical research Fleck had observed that access to empirical facts was not straightforward. Because natural phenomena are not directly clear one cannot apply a translation procedure to sense data because one does not know yet what these data represent. According to Fleck this meant that so-called facts are not perceived in isolation but start to make sense only in relation to other things. He borrowed ideas from Gestalt psychology in order to explain such holistic perceptions in science. The 'wholes' are perceived all of a sudden and provide the framework for further exploration. This perception is imbibed by the goals and problems scientists are working on and the conceptual framework they operate with. According to Fleck, these provide the social and cultural context in which new knowledge *by necessity* has to be formulated. He invented new terms such as 'Denkstil' and 'Denkkollektiv' to capture these irreducible social aspects of science.

In Fleck's work the efficacy of standards of theory appraisal is connected to social and cultural contexts. Similar ideas can be found in the work of Toulmin (disciplines), Merton (institutions), Kuhn (paradigms) and Lakatos (research programmes). Toulmin for example argued that standards of appraisal were embedded in disciplinary contexts, which he interpreted by analogy with the theory of evolution. Merton set up a sociology of institutions. He argued that science functions as an institution, comparable to other institutions in society. The genesis and structure of these institutions must therefore be studied in order to understand the development of science. Kuhn came up with the notion of paradigm. A paradigm is comprised of theories, methods, standards of appraisal, core assumptions, and particular values. All these elements together make up a paradigm within which normal scientific investigation occurs.

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[61] Fleck's main publication, *Die Entstehung und Entwicklung einer wissenschaftliche Tatsache*, appeared in 1935 but met with a late reception due to the Nazi regime and World War II. Fleck survived Auschwitz but did not return to philosophy afterwards. Both Kuhn and Latour have declared their indebtedness to Fleck.

The shift in focus from a set of context-independent standards to standards pertaining to specific frames of reference was also motivated by holistic theories of meaning. In these theories the meaning of individual terms, including scientific terms, cannot be established in isolation from other terms. In Quine's theory, for example, there is a web of belief in which the strength of a belief is a function of how well the belief is entrenched in the network. According to Quine this also holds for seemingly unshakable formal logical rules, such as the law of the excluded middle. These only *appear* to hold irrespective of any context, but their acceptability may change, for example when the connections in the network become seriously modified. In this kind of holism, standards of appraisal of scientific theories are therefore always embedded in particular networks of belief.

In science studies scholars seek to determine the meaning of knowledge claims and evaluate their content. In order to do this, ever larger frames of reference had to be considered. At first these frames of reference consisted of a (formal) language or a model of rationality. Later larger units of organization such as institutions, disciplines and paradigms were found necessary in order to properly explain theory acceptance and theory rejection.

All these approaches can be described as contextualist, yet until the 1970s all of them made use of a distinction between internal and external factors. What is internal to science is, in one way or another, independent of socio-cultural context. According to Shapin any distinction between internal and external realms of science is evaluative because it involves a view of what proper science is.[62] The most radical step in going bigger in the theory dependence tradition has been to fully give up the distinction between internal and external factors.[63] The justification of claims to knowledge after giving up the distinction becomes fully dependent on contextual factors, that is, the social, cultural and political factors, operative in a given historical context. All rational norms, including evaluative standards, become dependent on these variables. 'Context' may no longer be an appropriate word to use because strictly speaking no external context to science is recognized anymore. This approach leads to a predominance of socio-cultural factors in the explanation of theory choice and has influenced historiography of science deeply, as we have seen in section 4

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[62] Shapin (1992).

[63] For a more extensive treatment of the distinction between internal and external factors and giving up this distinction by postulating a principle of symmetry I refer the reader to chapter 2.

above. It is especially from this most radical move in the theory dependence tradition that the first set of arguments against evaluative historiography flow.

The first argument is that if every evaluative procedure is dependent on a time-bound conceptual framework then this also holds for present-day standards of evaluation. Therefore neutral assessments of past science cannot be given and therefore they should be avoided as much as possible. The grounds for evaluation can be questioned in another manner, which gives a second argument. In order to assess past claims to knowledge historians need to rely on present-day science. Given the highly specialized nature of present-day science it is questionable whether historians of science have sufficient insight in the content of present-day theories. If this is a necessity then misinterpretation of present-day knowledge is to be expected and this cannot be helpful to historical interpretation.

A third argument questions what is gained by 'purifying' past claims to knowledge by lifting them out of context. It is not clear how this helps in deepening our understanding of past science, which is what historians should be after. The project of understanding specific meanings attached to words and deeds including intentions, motivations, ways of argumentation, etc., of past scientists should focus on factors and standards of evaluation operative in past contexts and relate theory choice to these contextual parameters. Evaluations from an extra-contextual point of view stand in the way of such a project.

Finally, the goal of deepening our understanding of the past is not helped by mixing it with our own bias. Even though this is unavoidable to some extent, one can aim to be as neutral as possible. It certainly does not help to carve up the past according to some idea what the proper way of doing science is. This closes our eyes for practices of investigating nature that are very distinct from our practice and have to be incorporated in historiography of science. An evaluative attitude based on a platform of assumptions in the present stands in the way of such open-mindedness.

6.2 Presentism

Among historians of science a strong sensitivity towards presentist historiography has emerged, mostly in opposition to Whig history. The arguments against evaluative historiography stemming from presentism are not far remote from the arguments ranked here under theory dependence in 6.1. Yet the arguments in 6.2 exhibit a number of special features which merit separate treatment.

The first argument is that using the present as a touchstone for the past leads to constant rewriting of the past.[64] As the present is in constant change, the selection of what is found relevant in the past changes accordingly. Even if we grant that access to modern scientific theories is relatively unproblematic, the evaluative historian would still be relying on a body of knowledge that in all probability has only temporary status, as present-day claims to knowledge will almost certainly be adjusted or refuted in the future. Furthermore, the point is not just that the content of scientific theories changes, but also that the evaluative standards themselves change.[65] If such standards are used to judge past contributions to science then the history of science has to be rewritten with every change in evaluative standards. There is something odd about this because the past itself does not change anymore. It logically follows that interpretations of the past become more stable when they are disconnected from present-day evaluative categories.

Secondly, presentist historiography leads to finalistic or teleological historiography. This practice is circular because it explains developments towards outcomes, while using these very outcomes as explanatory tools. Furthermore, finalistic historiography lends the historical process a form of necessity it most probably did not have. It often leads to ancestor hunting: the search for origins, pioneers or anticipations of things that for the historical actors in question were not 'ancestors' of later developments at all.

What we get with this type of historiography is hagiography because seeing things first, long before the others, required brilliant scientists. This kind of hagiography is misplaced because it obscures important aspects of past science. Of course a number of past scientists were gifted people. But when looking at the past this way it is easily forgotten that 'the genius' rarely operates in isolation as he or she stands in contact with contemporaries and with scientific heritage. The genius' contribution to science therefore mostly is deeply entrenched in the ideas of others.

The argument can be extended to protests against the use of anachronisms in historical explication. A projection of present-day categories onto the past, like 'people spoke about phlogiston but what they really meant was oxygen', not only leads to mistaken prototyping, but also to distorting the meaning of the concept of phlogiston. As the historian's primary task is to retrieve what this

[64] This point is made in Bowler (1988), Schuster (1995) and Kiss (1999).
[65] A clear illustration of this is provided by Daston and Galison (2007). They demonstrate that since 1800 at least three models of objectivity have been dominant in science.

concept meant to the historical actors, the proper conduct is to avoid the use of anachronisms altogether and confine oneself to such 'actor's categories' in historical explanation. Contextualization is the remedy against all sins of Whiggism. To sum up the argument: evaluative historiography leads to finding prototypes to present forms of science that did not exist. The danger is that this leads to serious misinterpretations of words and deeds of past actors. Using hindsight in this way is thus harmful to historical interpretation and should be avoided.[66]

Thirdly, evaluations of past science must be based on present-day expert knowledge. It follows that such evaluations are scientific assessments and not really historical assessments. If evaluative practice becomes completely dependent on current scientific insights then it is hard to see what the point of *historical* investigation and evaluation is. Which effect can this have, other than passing unfair judgements on past actors for things they could not possibly have known? If historians only seek confirmation and justification for present-day knowledge then why study the past at all?

Finally, presentism can lead to bias of historians against persons, countries, periods, etc., that is unjustified. The effect may be to overlook, consciously skip or seriously misrepresent these categories. An example is Noam Chomsky's account of the history of linguistics.[67] According to Chomsky, this field started out in the right direction in the 17th and 18th centuries with investigations into formal grammar. The 19th century in his account represents a serious backlash, as linguistics was captured by the historical comparative approach to language study. This approach yielded no insight in the formal structures of language, the relation of language to reasoning and the general cognitive capacities involved in language use. Only with Saussure, and later Chomsky himself, the right thread was picked up again and the tradition of 'rationalist thought' could proceed.

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[66] An interesting note to this argument is Chang's observation that judgemental historiography often boils down to a history of winners, called Tory history in Fuller (2000) to contrast it with genuine Whig history. Chang's idea is that historians uncritically copy accounts of the victors of past scientific controversies, which leads to misrepresentation of the losers (Chang 2009). In his case study he argues that both the phlogiston theory and Lavoisier were 'wrong' from a present-day perspective and thus should both be criticized from a Whig perspective. But in the majority of cases Lavoisier is hailed as an important step forward towards present-day chemistry while the phlogiston theory is set aside as an unfruitful theory. According to Chang this then is not really presentist judgementalism because it rests on ignorance of the true content of Lavoisier's and phlogiston theories.

[67] Chomsky (1966).

Chomsky pays no tribute at all to the important interpretation of language as an organism that dominated 19th-century linguistics, and from which the notion of structure later was derived, on which the work of both Saussure and Chomsky so heavily depends.[68] Selective bias on the part of the 'historian' has led here to a lack of sensitivity to relevant aspects of past science *even for* his own research programme. The conclusion must be that it is very dangerous to turn the present into a privileged period. Past periods have an intrinsic worth, on equal footing with the present, and should therefore be studied independently of later developments.

In short, presentist historiography means wrenching historical events out of their context and this offers no insight in how things were, how and why they changed, etc. Using present-day state-of-the art scientific knowledge and present-day evaluative standards makes one lose sight of the contingency of the historical process. Things not relevant from a presentist perspective must either be left out or else be judged negatively. In both ways this hampers historical understanding. The difficulties with presentism are aggravated by the fact that the present changes continuously which would make historiography of science a seriously unstable endeavour.

6.3 Rule following

In philosophy of science it is a common idea that science can be seen as a rational affair, and hence as qualitatively different from other human endeavours, provided the process of theory replacement can be captured in terms of systematic (or formal) procedures that hold irrespective of historical context. Major discussions among philosophers of science have been fought over the issue how to adequately articulate such rational procedures. Well-known proposals have been verificationism of logical positivism, Popper's falsificationism and Lakatos' methodology of scientific research programmes. Irrespective of the procedure one settles on, it is clear that any procedure of this type provides the grounds for evaluative historiography. Past theory choice that was made in conflict with the given rational procedure must be judged erroneous. As Lakatos has argued this can offer the historian valuable clues about the past, as the deviant behaviour must be explained with reference to context-dependent factors. Such external historiography then deepens our understanding of historical context. Still, Lakatos argues, internal

[68] Koerner (1975).

historiography has to follow the specified rational procedures instead of the way the actual past went.[69] Such historiography, with the clear distinction between what is internal and what is external to science, is grounded in a theory of what counts as rational scientific behaviour and is, on the basis of this meta-methodology, evaluative in nature.

Arguments of rule following turn against this type of historiography in a number of ways. The first argument is that it has not been proved that science progresses in a well-ordered and step-by-step fashion. In spite of many attempts, no one has been able to satisfactorily capture the dynamics of science with one normative meta-methodology. On the contrary, studying past episodes of theory change in detail reveals that science is a discontinuous enterprise in which many shifts of perception and changes in ideas occur. Perhaps the string of successive theories amounts to more than 'just one damn thing after another', but a specification of one rational procedure, with which it is possible to glue the string of theories together, has proven hard to find. Moreover, when the effect of postulating a rational meta-methodology is that large parts of past science must be declared irrational, we must wonder what the point of postulating such normativity is. Models of scientific rationality have to reflect actual decision making in order to be useful for historical interpretation. Following these considerations we have to accept that standards of rationality are not context independent. That is, their acceptance or rejection depends on specific socio-cultural factors. As these standards do not transcend their context of application it follows that there is no meaningful basis for normative comparison of theories from one context to another.

A second argument is a variation on the problem of induction.[70] If there is no clear mechanical, algorithmic or formal procedure that governs the step-by-step development of science then the correct next step cannot be inferred from previous steps. The problem is that in a given situation, where rival theories are in competition, we cannot judge whether the right choice was eventually made, because this cannot be inferred from previous steps in history. Without algorithmic rules for theory choice, so the argument runs, it follows that science is not governed by norms of rationality. And hence progress in science cannot be measured, since theory change must lead to cumulative explanation in order to be progressive, and the explanation of natural phenomena is evidently not a cumulative process.[71]

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[69] Lakatos (1970).
[70] At least it is so called in Lakatos (1970).
[71] In Laudan (1996) pp. 24-25 this argument is attributed to Kuhn.

A third argument against evaluative historiography flows from the consideration that standards of theory appraisal change from time to time. Worrall has argued that we need super-standards to judge whether such changes in evaluative standards were progressive.[72] He denies that such super-standards can be formulated and draws the conclusion that relativism must therefore be accepted. For Worrall there is no middle ground between absolutism and relativism. He argues that even if a normative rational reconstruction of the past were possible this must by necessity be a biased one. But if this is the case it makes no sense to create rational reconstructions of past science since the very goal of these reconstructions is to rid history of bias as much as possible.

6.4 Incommensurability

When things are said to be incommensurable this means that they cannot be compared according to some common standard of measurement. In the last section we have seen that this plays a role on the level of standards of rationality. This indeed is just a variant of incommensurability. But here we restrict the notion to the incommensurability of the meaning of individual terms and, in a wider sense, to the incommensurability of distinct conceptual schemes.

Feyerabend has argued for incommensurability of subsequent scientific theories as follows:

> "what happens is rather a complete replacement of the ontology of T* by the ontology of T, and a corresponding change in the meanings of all descriptive terms of T*, provided these terms are still employed."[73]

In any descriptive theory of reference, sense determines reference. If changes in descriptive components occur then this means that the reference of the term associated with the change in descriptive components changes accordingly. If the reference of the same term in different periods of time is not invariant we cannot compare the two terms meaningfully because they refer to completely different things. People in different periods may for example use the same terms, such as 'earth' or 'atom', but attach totally different meanings to them. 'Earth', for example, has referred to a flat surface but also to a sphere. The problem may become even more apparent when different terms are used altogether, such as phlogiston and oxygen (see above).

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[72] Worrall (1988).

[73] Feyerabend (1962) p.59.

The argument from incommensurability says that we have to accept that changes in reference frequently occur in science. This means that we cannot evaluate whether a successor theory improves over a predecessor because the theories talk about different things. Feyerabend and Kuhn have repeatedly pointed out that the problem runs so deep because of holism. The meaning of individual terms is dependent on their relation to other terms. Were only theoretical terms susceptible to meaning change, the rest of the conceptual framework could provide a stable background and possibly leave room for assessments of the change in theoretical terms. Alas, this is not the case according to Feyerabend and Kuhn, as all terms are susceptible to change. We cannot judge which set of terms forms a better description of the actual state of affairs in the world because these conceptual schemes come in wholes. The problem is that there is no 'third' stance from which intra-schematic comparison is possible. We lack a neutral translation manual outside any conceptual scheme, including our own. Hence there is no comparative ground to assess the relative worth of distinct conceptual schemes.

The argument from incommensurability has the clearest effect on diachronic historiography. In a Kuhnian framework, for example, one can speak of accumulation of knowledge only within a paradigm but not from paradigm to paradigm. During periods of revolutionary science, in which paradigm shifts occur, fundamental notions change considerably and incommensurability between these schemes manifests itself. Hence conceptual schemes following each other in time cannot be qualitatively compared.[74]

There is no easy way out of the problem of incommensurability. What it requires is a statement of some stable comparative ground. However access to a base of theory-neutral facts is impossible due to the problem of theory-ladenness. And access to a set of evaluative standards is not available either, as we have seen in the discussion on the argument of rule following. In philosophy of language the causal theory of reference (Kripke, Putnam) has been the main theory invented to deal with the problem of incommensurability, but it is not without problems itself.[75]

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[74] Accepting the incommensurability thesis can also have an effect on synchronic historiography as participants in a controversy can be taken as essentially talking past one another.

[75] There is no space to go into these solutions here. Evans (1982) argues that the causal theory still has difficulties with the crucial issue of reference change. For an elaborate recent discussion see also Kuukkanen (2008), chapter 2. Other solutions to the problem are given in various description theories (amongst others Searle). The principle of charity was initially formulated to deal with the problem of stability of reference when

6.5 Underdetermination

The argument from underdetermination stems from approaches to the study of science that do not accept a distinction between social and rational factors. It is one of the most forceful arguments behind the strong programme in the sociology of scientific knowledge, which will be explored in detail in the next chapter. The argument runs as follows. At every junction in the history of science choices had to be made between alternative theories. Controversies came to closure and we need to explain how. The thesis of underdetermination says that in the majority of cases the debates could not have been settled with reference to empirical information because all parties agreed about the data. They differed in *accounting for* the data. Empirical evidence in general allows for such differences in interpretation and thus underdetermines theory choice.[76] If this is true then empirical evidence is clearly insufficient for the evaluation of theory choice.

However there are other means of evaluating the choices made in the past. One can explain settlements of controversies by reference to rationality, validity, or by reference to epistemic categories such as simplicity, fruitfulness, etc. Such explanations have however been criticized on the grounds that they are circular. These qualifications have all been established *ex post facto*, after the winner of the controversy has been selected. *In* the controversy competing theories seemed equally rational, valid, simple, etc., and hence these criteria cannot have decided theory choice. It is circular to account for theory choice in past science this way because outcomes of controversies are used in the explanation how these very outcomes came about.

If all these options of explaining closure of past controversies are not available, we still need to find a way to explain how controversies ended. Adherents to the strong programme argue that we have to look at social factors. These can play a role both on the micro-level (relations of trust, authority, persuasion, etc.) and on the macro-level (common values, systems of power, etc.).[77] If all this has to

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changes in descriptive elements occur. This principle is important for historical interpretation; it is discussed in chapter 3.

[76] In Quine's terms the problem of underdetermination is also one of translation. As more than one translation from data to theory is possible it is really because of the indeterminacy of translation from data to theory that underdeterminaton comes about.

[77] Note that the argument of the experimenter's regress is very similar. The regress in accepting expertise being dependent on expert judgement can only be stopped if at some point social factors are invoked to explain why some person, some device, some method of interpretation or some theory acquired expert authority.

be accepted then it is clear that there is simply no room left for assessments of past science.

7. Two sorts of argument: desirability and possibility

Much of the remainder of the thesis will be devoted to finding replies to the five arguments given in the previous section. In order to obtain more grip on them, it is useful to classify the arguments into two groups: one group of arguments questioning the *possibility* and the other group questioning the *desirability* of making assessments.

The arguments from incommensurability, rule following and, in part, theory dependence question the possibility of establishing the grounds for evaluative historiography. As historians lack access to transcendental standards of rationality and objectivity, to current scientific expertise, to an independent translation procedure between conceptual schemes, or to a base of independent empirical facts, these grounds cannot be given. As evaluative historiography requires some stable extra-historical platform, and this platform cannot be provided, assessments of past science cannot be meaningfully carried out.

The problems set by underdetermination, presentism and, again in part, theory dependence show that evaluative historiography leads to incomplete, or even worse, mistaken explanations of past science. Granted that evaluative historiography is possible these arguments are designed to demonstrate that it is undesirable to approach the past with evaluative categories as a basis for historical explanation.

The arguments questioning the desirability of assessments of past science follow from changes in research programme in science studies. Evaluations of past science have been dropped as a consequence of these changes. The arguments questioning the ability to carry out evaluative historiography have on the other hand *forced* a search for alternative ways to study past science. Yet the effect on historiographical practice of both groups of arguments is the same. They both lead to localist and descriptivist historiography, which was illustrated in section 4 above.

Whether this localism is considered to be a type of objectivism, which was connected above to the striving for exactness, and which appears to be more closely linked to the possibility arguments, or as a way of avoiding prescriptivism, which was connected above to the striving for liberation, which appears to be more closely linked to arguments questioning desirability, does not directly matter for the historiographical output that is being produced. However if we want to regain a place for evaluations of past science in historiography it does matter which type of argument we are addressing. The arguments questioning the desirability of evaluative historiography require *less* fundamental counter-argumentation than the arguments questioning the grounds from which evaluative historiography can be undertaken.

In the next chapter we will for example find that attempts at philosophical refutation of the symmetry principle have remained inconclusive. This is not surprising if we consider that the argument from underdetermination is the main argument for symmetrical study of past science, and this is an argument of the desirability type. The chapter thereafter on the principle of charity contains a philosophical discussion, which in the end results in a defence of the claim that a weighted principle charity principle is constitutive of historical interpretation. This is also not surprising because the discussion on charitable interpretation is mainly relevant to the arguments from the possibility group, such as incommensurability and rule following, and these arguments require thorough refutation. The desirability arguments, on the other hand, have to be met by offering perspectives and ideas that show why including evaluations of past science make history of science a stronger profession. An overview of these ideas is provided in the last section of this chapter.

8. The gains of evaluative historiography

Understanding and evaluation often appear as opposite categories. It has for example been argued that any attempt to understand motivations behind the atrocious deeds of the Nazi regime lead to relativizing the brutality of their acts. Bringing about deeper historical understanding is thus blocked from an evaluative stance. In historiography of science nowadays an evaluative attitude is invariably seen as leading to distorted historical understanding. I believe that the profession deprives itself of important tools to reveal what is hidden in historical contexts with this attitude. Moreover a number of such insights cannot be gained in ways other than through an evaluative approach.

A clear example is provided by Galileo who predicted a relation between air resistance, speed and frequency of oscillation of pendulums, which does not stand up to present-day empirical testing. McAllister now perceptively points out that it is upon such findings that Galilean experiments are interpreted as:

"didactic thought-demonstrations manufactured for their persuasive power after the completion of the relevant theory rather than as true sources for Galileo of raw data. Our replication of this historical mechanism has led us to opinions of our own on the text's veracity and thence to a new interpretation of the role of experimental evidence in Galileo. An evaluation of past science has here visibly provided the basis for much of the current literature on a major figure of the scientific revolution."[78]

McAllister (1986) provides more examples of how evaluative historiography can be beneficial to historical understanding. Aristotle made a number of strange claims about human bodies, for example that women have fewer teeth than men. This is strange considering the organisms as we know them and it suggests that Aristotle relied on textual evidence, such as philosophical doctrine or reports of others, rather than on his own observation. This yields socio-cultural clues for historical interpretation that can be based only on an evaluation of the truth of Aristotle's claims. Another example is the Needham question: why did modern science emerge in the West in the 16th and 17th centuries rather than in China, which was arguably more advanced during the Middle Ages, both scientifically and technically? Whatever the reasons, the possibility of posing this question depends upon the belief that past states of science can be normatively compared.[79]

Another example is given by a puzzling number of measurements of the ratio of the electron's mass and charge carried out by J.J. Thomson in the late 1890s. About this Weinberg writes:

"he persistently emphasized measurements that gave results at the high end of the range. The historical record alone would not allow us to decide whether this was because these results tended to confirm his first measurement, or because these were actually more careful measurements. Why not use the clue that the second alternative is unlikely because the large value that was favoured by Thomson is almost twice what we know today as the correct value?"[80]

Historians can only guess at the results unless present-day knowledge is used. This does not involve judging past scientists for not seeing things the way we do: all it requires is to accept that the phenomena that Thomson was investigating are similar to those that were investigated a century later.[81]

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78 McAllister (1986) p.329.

79 This is one of reasons why comparative historiography is so scarce in history of science today. See especially the final section of chapter 6.

80 Weinberg (1996).

81 Tosh (2006) and Tosh (2007) contain the same argument. Proper use of modern knowledge may require cooperation between historians and scientists. Scientists can benefit from this cooperation too as historical scholarship can be of aid in present-day

The above examples have illustrated how assessments of past science can be useful tools for historical explanation. This is not the only gain evaluative historiography promises. As Alder (2002) and Jardine (2003) have pointed out, historiography of science has lost its critical functions over the past few decades. This has led to at least three problems: undermining the credibility of present-day science, undermining the relevance of the study of past science for present-day science and undermining the notion of scientific progress. Let me address these problems in turn and suggest how an evaluative approach to past science can help restore the critical functions of the profession.

Historians of science continuously stress the contingent aspects of knowledge formation. Typically, for each problem, a number of solutions were in competition, and experts, as a rule, do not agree on those solutions. This provides ammunition to the questioning of the expertise of scientists nowadays. For every expert opinion a contrasting opinion can be found, with the implication that these opinions, when needed, can be ignored. The same effect is yielded by the argument from rule following. If there are many, in principle equivalent, belief systems, then variety of belief must be accepted. This raises questions about the authority of scientific belief systems over other belief systems. In a society that is so dependent on modern science and technology it can become dangerous when we lack the means to clearly distinguish dilettantism from serious alternative points of view.[82] The credibility of science, both as an endeavour and in terms of theoretical content, can be safeguarded if satisfactory means to assess scientific expertise can be provided.

A non-critical engagement with past science runs the risk of losing contact with present-day science. Historical scholarship can be relevant to present-day science in the following ways. It can provide insight in the historical genesis of differences of opinion that exist today, and thus be of aid in present-day scientific controversies. It can help critically evaluate new ideas by providing a check on originality and functioning as an antidote to delusion and hubris. It can place new contributions to science in existing traditions. It can teach a general academic attitude of patience and moderation. And finally it can act as a source of inspiration.[83] To exert these functions some connection between past

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research, for example by re-opening forgotten lines of research. This has been defended, in true Sartonian spirit, in Chang (1999). See also the discussion on restaging of experiments in chapter 7.

[82] What we lack in science studies is a good way to communicate the role of uncertainty in science to the public. In my view this requires rethinking the concept of uncertainty and its relation to the notions of truth and error. I address this topic in chapter 4.

[83] These points are mentioned in Sarton (1952) and Elffers (1991).

and present needs to be established. Yet the arguments against evaluative historiography all disconnect the study of past science explicitly from present-day scientific research.

Finally, a non-critical engagement with past science closes the door to assessments of scientific progress. It is a peculiar phenomenon that historiography of science lacks a good theory of qualitative improvement in science.[84] Scientists are often motivated by improving on their predecessors or being better than their contemporaries. As they aim to correct errors of others, they are also well aware that their own hypotheses can be mistaken. An example is Einstein who scolded himself for having made the biggest mistake of his life in putting forward a cosmological constant to keep his model of the universe stable. Einstein thought this was bad science because he could not provide any other physical reason for introducing the constant.[85]

Max Weber, in his famous lecture 'Wissenschaft als Beruf' (science as a vocation), asked why anyone would devote his or her life to science when one knows that one's contributions will inevitably be rejected and replaced by others.[86] If historical actors were aware that they could be wrong and if, in the self-image of many scientists, playing the game of science is about getting things right, some form of evaluation is required to bring this about in historiography. In general it is plausible to say that because of the widely accepted fallibility of humans, a quality check on their work is needed. As this can often be done only with the benefit of hindsight, performing this check should be a task of historiography.

Other arguments in favour of evaluative historiography stem from considerations of the aim of providing neutral scholarship. It is not difficult to see that any form of historiography contains a degree of presentism, for the simple reason that the historian can never fully shake off his or her own rootedness in place and time. Although those opposed to evaluative historiography realize this, there is a clear aim to write historiography in as neutral a fashion as possible. This can for example be seen in the arguments from theory dependence and presentism.

This attitude involves the myth of the 'given' past, which we only need to describe, and hence all it requires is a descriptivist historiographical strategy. It is however an idée fixe that the historian can restrict himself to just describing

[84] This is the general topic of chapter 4.
[85] Ray (1990) argues that introducing the constant may not have been such a mistake at all as the constant was a solution to a long-standing problem and continues to be in use.
[86] Weber (2002).

the past 'as it happened'. Historical work must involve criteria of selection because otherwise the task would be to describe all aspects of the past, which is not only impossible because of the size of the work but also pointless because it would amount to nothing more than a repetition of past events on paper without offering any explication of these events. Without selective criteria, what would be the motivation to study the past in the first place? Of course the need to work with selective criteria does not directly entail that these criteria are evaluative. But as these criteria have to be formulated in the present, it does considerably weaken the arguments from presentism. The problem of lack of selective criteria is a serious one. The development in approaches to past science driven by a force of liberation has led to erasing all kinds of selective and interpretive criteria.[87] Rudwick for example complains about the effects on historiography this has had: "the political, economic, social and cultural dimensions have little historical significance if their analysis neglects the precise claims to knowledge and epistemic goals that were the ostensible raison d'être of the scientific work."[88] Indeed, it becomes unclear what all these dimensions have to say if the epistemic goals of science do not occupy centre stage. Arguably, in order to give the epistemic goals centre stage in historiography of science, and establish past claims to knowledge with precision, we need to rely on evaluative categories.

In conclusion, evaluative historiography can contribute to improve the output of historiography of science in three ways: it yields tools to enhance historical understanding, it provides the means to conceptualize scientific progress and it provides criteria of selection which makes history of science a relevant discipline endowed with critical functions, not only for studying the past, but for present purposes as well.

In historiography of science today it is unclear which means of interpretation one is allowed to use, how historical episodes must be connected to each other and how more general or comparative historical questions can be put on the agenda. Thomas Kuhn observed that "particularly in periods of acknowledged crisis, scientists have turned to philosophical analysis as a device for unlocking the riddles of their field."[89] This appears to hold for historiography of science today in which many riddles are waiting to be unlocked. As we have seen above, historians in the past few decades have made an effort of disconnecting their discipline from 'parent' disciplines such as philosophy of science and the natural

[87] See the next chapter.
[88] Rudwick (2005) p.4.
[89] Kuhn (1962) p.88.

sciences. This is seen here as a mistaken course of action. It is one of the peculiarities of history of science that ties with philosophy and the sciences cannot be broken. Of course such a remark does not call for a return to post-war historiography. Instead, the challenge is to reconceptualise the relations between these fields of study.

As will be made clear in the chapters to come, the role of philosophy in any approach to the study of past science is indispensable. To satisfy the need for an improvement of the analytical framework of history of science one also has to turn to philosophy. This may be a difficult message to swallow for historians of science who have had enough of philosophers of science telling historians what to do. This is certainly not the aim of the present investigation, which firmly acknowledges the high level of scholarship and the many insights in scientific practice the naturalist turn has brought. This development has yielded an enormous expansion in research topics and in consideration of determining factors, which has resulted in a much better understanding of the multifarious and complex thing that science is.

Yet the negative effect has been that what has been won in descriptive power has been lost in terms of analytical power. The aim of the present investigation thus is to provide history of science with a stronger set of tools of interpretation than it now has. In response to the arguments against evaluative historiography it will gradually become clear what this toolset consists of. Openness to the messages of this thesis does however require shrugging off stereotyped images of philosophy of science and of Whiggism in the community of historians of science. The gains in store are important enough.

Chapter 2 The Principle of Symmetry

1. Symmetries and asymmetries in approaches to past science

The principle of symmetry became manifest in science studies with the advent of the strong programme in the Sociology of Scientific Knowledge (henceforth SSK) in the 1970s. Yet upon reflection we can see that every approach to past science relies on the assumption of some form of symmetry. To see why, we must first make a distinction between topics and resources. Topics are the things that stand in need of explanation. In this thesis the central topic is theory choice, or from a diachronic perspective, theory change. Supposing that, in the majority of cases, alternative explanations for a particular natural phenomenon or a particular research problem were in competition, the question historiography of science needs to answer is what made the balance tip in favour of one of these explanations, and consequently, how to evaluate the choices that were made?

With the topic stable, approaches to past science differ in terms of the resources they recognize to carry out the explanation. Any mode of explaining past science has variant and invariant aspects. What remains invariant is the symmetrical aspect of the explanation. Thinking about symmetry then is a way to think about the topic-resource interface in historical explanation. Decisions made with respect to these categories determine, so to speak, the working space of the historian of science, both in terms of research agenda and in terms of ways of approaching the main research questions.

The term symmetry is derived from the Greek word ‘symmetros’ which has the literal meaning ‘to measure together’, i.e. symmetry is some form of common measurement. It can be observed in three ways. The first is mirror symmetry, that is, reflection via an (imaginary) line. The second is rotational symmetry, that is, with respect to a fixed point of perspective. And the third is invariance under transformation: a system retains certain characteristics after transformation. In physics, symmetry is mostly defined as invariance under transformation. A clear example is the law of conservation of energy. No matter how many transformations occur in a closed system, the total amount of energy remains constant.[90]

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[90] For more background on the concept see Hon and Goldstein (2008) and Du Sautoy (2008).

Many approaches to the history of science work with a notion of symmetry as invariance under transformation. In Whig history of science, for example, all past science is considered as working up to the present. No matter how many transformations occurred in the past, the present is the stable element in Whiggish explanations of past science. Another form of invariance is the assumption that the structure of the world is stable. The history of theory replacement can then be interpreted as converging upon this structure. Nature itself rejects mistaken hypotheses, for example in response to experiments, and this prompts the search for alternative explanations. One can explain past science this way only on the assumption that the structure of the world is invariant.

Another important kind of invariance that approaches to past science rely on is given by the assumption of transcendent norms of rationality. In such approaches the proposed norms of rationality are context independent and hence are supposed to hold in every time and place. Progress in science is ultimately ensured not by historical actors, technological advancements or theoretical breakthroughs, but by norms of rationality that govern these.

When choices made in the past do not conform to the selected model of rationality one has to explain these choices with reference to specific factors operative in local contexts, often referred to in short as social factors. Persistent belief in what we now see as erroneous theories can for example be explained with reference to religious or philosophical doctrine, to authority relations, to cultural values or to social interests. The acceptance of one type of belief, correct belief, or at least belief in the right direction, is explained by rational factors and another type of belief, incorrect belief or belief hampering scientific development, is explained by social factors. These explanations of past science are said to be asymmetrical because not all beliefs are accounted for with reference to the same type of factors. Note however that there is a symmetrical aspect in the explanation, namely the assumption that the norms of rationality are historically invariant.

It is important to understand this way of explaining past science because it is against this form of asymmetrical explanation that sociologists and historians in the 1970s, and beyond, have reacted. There are approaches to past science that, at first appearance, come close to a fully symmetrical explanation of past science, because they allow for a considerable degree of contextualism. On closer inspection we can however see that these approaches, in one way or another, still rely on a transcendent norm of rationality. A first example is provided by Merton's sociology of institutions. Merton proposed to interpret science as an institution, functioning like other institutions in society. We can therefore apply the same sociological means to study the functioning of the institution of science, as we use in the study of the functioning of other institutions. However, in Merton's theory social factors define only the context in which science is practiced, but not the content of science. For example, he argued that the Reformation created a climate that was very suitable for the development of modern science. Merton claimed that, had the whole of Europe remained Catholic, mankind would have arrived at the same scientific breakthroughs that have occurred in actual history, only at a much slower pace.

That this would be the case is due to a number of core values that define the character of science according to Merton, namely commun(al)ism, universalism, disinterestedness and organized scepticism. Living up to these values ensures the rationality of the scientific process. The pace of development is due to contextual factors, but these factors ultimately do not touch the content of scientific theories. Note that this approach may also imply an evaluative assessment of historical contexts, as some of these contexts are more favourable to scientific development than others.

Next to Merton's sociology of knowledge we can also have a look at Lakatos' methodology of scientific research programmes. In this methodology a research programme is interpreted as a collection of theories. A research programme can be progressive in two ways: theoretically and empirically. Theoretical progress is achieved by better predictions; empirical progress is given by the confirmation of novel predictions, which is a sign of increase in empirical adequacy. It is irrational not to choose for successor theories within the programme that promise theoretical and/or empirical progress. Lakatos' methodology allows for temporary regressions or stagnations of the research programme, but these cannot last long. It is rational to pursue a progressive research programme, and within research programmes it is rational to choose progressive theories. When choices made in the past conform to these norms of rationality they belong to internal history, when not they must be explained with reference to social factors and belong to external history. A rational reconstruction of past science, i.e. its internal history, may deviate from the actual course of history and replace irrational choices by rational ones. We can see that in this approach assessments of rational behaviour are dependent on the chosen programme and on the particular theories in question, i.e. they are dependent on context, but evaluations of progress and regress through assessments of predictive accuracy and empirical adequacy are still the same for all research programmes, and hence also context-independent.

Finally, even Kuhn's model of science as paradigm alternation, rightfully seen as more contextualist than Lakatos', works with the idea of an internal realm and a surrounding context. What we call science can be called so only because it exhibits patterns of paradigm alternation via periods of normal science and revolutionary science. With this model Kuhn was able to capture two forms of criticism, institutionalized (during periods of normal science) and fundamental (during periods of revolutionary science). A flavour of meta-methodology is therefore kept alive in Kuhn's model, as scientific development has to conform to this structure.

It is true that the constituents of paradigms are all taken from a particular historical context. Even what counts as a fundamental critique is informed by contextual parameters. But, already in *Structure*, Kuhn argued that a scientific community accepts paradigm shifts only when they promise an increase in problem-solving capacity.[91] The fact that fundamental critique is directed at the previous paradigm ensures that the alternating paradigms are not wholly unrelated. Later in his career, Kuhn argued that inter-paradigmatic comparison is possible on a number of epistemic virtues such as simplicity, consistency and fruitfulness. This is not possible in symmetrical study of past science because both the preference for, and the very understanding of, these epistemic virtues are dependent on historical context. Hence they cannot play a fundamental role in the explanation of theory acceptance and rejection in science.[92] It is therefore incorrect to interpret Kuhn's philosophy of science as another variant of the social study of science, as is often done. The difference will stand out more clearly in comparison to the principle of symmetry of the strong programme.[93]

2. The strong programme

In the previous section I argued that all approaches to past science rely on some form of symmetry. Yet only with SSK symmetry became a methodological principle. The key text is Bloor (1976), which starts with the observation that in

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[91] This view is not the same as Laudan's definition of scientific progress in terms of problem-solving capacity. For Kuhn, with every paradigm shift some problems become irrelevant and hence some problem-solving capacity is lost in the process. Laudan does not think along these lines, see below and chapters 4 and 6.

[92] See also the debate between Laudan and Bloor on this issue below. Thinking about rationality in terms of the pursuit of epistemic virtues is essentially the right way of looking at it. But this idea can be interpreted in many ways. In chapter 7 I develop my own view on this matter.

[93] Kuhn distanced himself from the symmetrical approaches to science coming 'after' him. One of the most constructive examples is Kuhn (1991).

approaches to science based on demarcation between internal and external factors it is the natural thing to arrive at correct theories. ‘Right’ actions then appear to carry their own motives, irrespective of historical context. Following Merton, Bloor argued that correct theories require as much explanation as incorrect ones.

“The sociology of knowledge came into being with the signal hypothesis that even truths were to be held socially accountable, were to be related to the historical society in which they emerged.”[94]

Merton however did not draw the radical consequence from this, which is to move beyond demarcation between internal and external factors. His sociology of knowledge is a ‘weak’ programme. It does grant an important role to social factors in science but it does not turn social factors into the overriding determining type of factors *tout court*. When this happens, we speak of the strong programme in the sociology of scientific knowledge. It is with the postulation of the principle of symmetry that everything becomes ‘sociology’, which is why this principle represents such an important watershed in the study of past science.

The strong programme rests on four principles: causality, impartiality, reflexivity and symmetry.[95] Scholars should focus on the causal processes that engender belief formation and theory acceptance in a community. In carrying out this naturalist programme, scholars should be impartial to the content of scientific theories. The whole approach must further be reflexively applied to SSK itself. As SSK makes the study of science a branch of sociology it is part of science. The reflexivity principle ensures that the principles of the strong programme are applied to itself and hence confirms its scientific status.

What gives the programme its most significant bite is the symmetry principle. Bloor formulated the principle as follows: “the same type of cause would explain say true and false belief.”[96] This means that the same type of causal factors must

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[94] Merton (1973) p. 11.

[95] Cf. Bloor (1976). In what follows we will mainly focus on the strong programme and the so-called Edinburgh school. Other members of this school were Barnes, Pickering and Shapin. The Bath school (Collins, Evans, Pinch) with its empirical programme of relativism (EPR) emerged around the same time, see Collins (1981a). Many other scholars such as Caneva, Wise, Kusch, etc., can also be associated with SSK. There are differences between approaches but these are not differences in principle, only in emphasis. EPR is for example more micro oriented and has a clear focus on the theme of expertise while the strong programme is more historically oriented, hence best fitting the present focus on historiography of science.

[96] Bloor (1976) p.5.

be used to explain all rejection *and* acceptance of claims to knowledge. Since social factors are taken to be decisive in all cases of theory choice, there is no special place for rationality anymore in the explication of past science. As Barnes put it:

"Needless to say all forms of relativism are anathema to rationalists, who insist that there is a crucial divide between rationally and irrationally held beliefs and that the incidence of the two different kind of beliefs must be explained in radically different ways."[97]

If this difference disappears the qualification of what counts as rational has to be explained as well. This is what generally happens when a symmetry is posited: something from the set of resources shifts to the set of topics.[98]

With the assumption of the symmetry principle the natural question is: how do we get at outcomes in science at all? Full symmetry with respect to all properties of a system would amount to a standstill. Only the breaking of symmetry yields outcomes. The attractiveness of an approach to past science that assumes a lot of symmetry is that *a priori* assumptions about science are decreased to a minimum. Yet the more symmetry, the higher the demand for a satisfactory breaking mechanism becomes.

In SSK the breaking of symmetry is explained in an ingenious way. SSK insists on symmetry only on *type* level. On occurrence level, social factors manifest themselves in many different ways, for example, in relations of trust and authority, through disciplinary training or in processes of inclusion and exclusion. On macro-level we can think about political systems, the economy, societal structures, the role of cultural values, etc.[99] In every situation of theory choice detailed analysis is required of all relevant factors, in terms of their causal efficacy on theory choice, and in terms of the interaction between them. On the level of actual occurrence there will thus be a different story to tell from case to case. The task of the historian of science is to investigate such particulars.

It is perhaps good to avoid a number of misinterpretations of the SSK approach to the study of science. First, it is *not* another form of externalism. As the internal-external distinction is given up, there is nothing to be external to anymore.[100] Secondly, it is *not* the case that nature and the activities of

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[97] Barnes (1992) p.135.

[98] For other illustrations of this effect see the generalized symmetry principles discussed below.

[99] Bloor (1981) p.203 suggests that where macro-social factors are not present the micro factors invariably take over.

[100] Shapin (1992) addresses the community of historians of science on the consequences of the strong programme for their profession and makes this point very clear.

individual scientists play no role anymore in the course of science. The physical world does provide the phenomena that scientists study and individual scientists come up with theories explaining these phenomena. Although the phenomena set some limits on the possibilities for interpretation, a wide variety of interpretations is still possible.

However, individual interpretations do not yet amount to knowledge. What the strong programme is offering is a different way of looking at the justification of belief, and hence a different way of defining what counts as knowledge. If knowledge is defined traditionally, as justified true belief, then nature could play a role as truth provider and the justification for having a belief could be attached to individuals. In the strong programme however, knowledge is equated with *authorized* belief. An individual scientist can do research, come up with experimental results and theorize about these. But when it comes to sustaining such knowledge claims he or she has to enter the social sphere and engage in a debate with other people. Therefore, when it comes to the question of acceptance or rejection of theories, social factors outweigh all other factors. There is thus a hierarchy of factors in science and the social ones are always dominant when it comes to theory choice.

Thirdly, SSK is *not* anti-science. It just offers a different view on what science is and how it functions, with the aim of deepening our understanding of knowledge formation processes. As it rests heavily on sociology, a scientific discipline, it aspires to be scientific itself. When it comes to the structure of nature Bloor has even claimed to be a realist. But he combines this ontological realism with epistemological perspectivism.[101] Many perspectives can be projected on nature as "nature will always have to be filtered, simplified, selectively sampled and cleverly interpreted to bring it within *our* grasp."[102]

It is this perspectivism that leads to epistemological relativism because the strong programme does not accept independent criteria to assess whether one perspective is better than another. As Bloor has put it: "all cultures are equally near to nature."[103] Hence, all perspectives developed on nature should be understood in their own right. It is from this epistemological relativism that a

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[101] "We take for granted that trees and rocks, as well as electrons and bacilli, have long been stable items amongst the furniture of the universe. They are just there providing a stable backdrop to the more volatile happenings on the human stage, where ideas change and theories come and go." Bloor (1999) p.86. There is a fundamental difference with posthumanism in this respect, see section 5 below.

[102] Bloor (1999) p.90.

[103] Bloor (1999) p.88.

direct line to the localist and non-evaluative character of present-day historiography of science can be drawn.

As said in chapter 1, support for the strong programme comes from the argument from underdetermination. According to SSK, in scientific controversies there can be no conflict over empirical evidence. Either conflicting parties agree on the stack of empirical evidence or when not, this is because they recognize different standards of measurement, different instrument calibrations or different standards of interpretation of experimental results. Hence it is a conflict on standards and not on the evidence.[104] As scientific controversies are eventually closed, theory choice must be settled by means other than empirical evidence. To solve this problem of underdetermination SSK draws the conclusion that we are forced to look at social factors in order to find out how controversies were settled. If this holds for all choices made in the past, the whole endeavour we call science is of a deeply social kind.

Other options to solve the problem of underdetermination are rejected on the grounds of circularity. We cannot use criteria of rationality, progress or success to tell the winners from the losers because these evaluations got attached to the winners only *after* they became victorious. It is circular to use resulting outcomes in the explanation of a process leading up to these results. Moreover, if it were possible to decide who is right and who is wrong by referring to standards of rationality or success, then there would not be a controversy in the first place.[105] Pickering has put the point as follows:

"If one is interested in how a scientific world-view is constructed, reference to its finished form is circularly self-defeating; the explanation of a genuine decision cannot be found in a statement of what that decision was."[106]

Collins pointed out that the circularity argument is a logical consequence of accepting the principle of symmetry:

"The tenet of symmetry tells us something about the content of our explanations. The same types of explanation will be applied to all 'qualities' of scientific endeavour. Explanations of the true will be like explanations of the false, and similarly for the rational and irrational, and the successful and unsuccessful and, we may suppose, for the

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[104] Clear examples are Pickering (1984) and Collins (2004). Note that if two parties agreed upon a difference in evidential support for their theories there would be no controversy worth mentioning.
[105] Jerkert (2006).
[106] Pickering (1984) p.404.

apparently progressive and the degenerative. It follows that there are things that cannot form part of an explanation belonging to the radical programme. Knowledge cannot be explained by reference to what is true, rational, successful or progressive. If such categories were allowed into explanations then the explanation of, say true, knowledge would not be of the same type as the explanation of false knowledge."[107]

Thus the principle of symmetry ensures both neutrality and the avoidance of circularity of explanation. The only way out of the underdetermination problem is to turn to social factors to account for theory choice.

What the objections against circularity basically amount to is saying that notions such as truth, rationality, objectivity and measure of success have no absolute character. The efficacy of these notions depends on historical context and must be socially accounted for. We have seen that traditional notions of right and wrong and the definition of knowledge must change accordingly. Scientific knowledge is never absolute. Knowledge claims must be judged according to the function they have in specific social circumstances and the way they attach to specific networks of belief. Perhaps a scientific theory can perform similar functions in another context but this is unlikely because there are too many specific factors in play. Therefore Bloor concludes:

"All knowledge is relative to the local situation of the thinkers who produce it: the ideas and conjectures that they are capable of producing; the problems that bother them; the interplay of assumption and criticism in their milieu; their purposes and aims; the experiences they have and the standards and meanings they apply."[108]

The turn to sociology involves a serious challenge to philosophy as the primary discipline to study science. SSK is deeply embedded in a tradition of social study of knowledge that started with Durkheim, *The Elementary Forms of Religious Life* (1912). For Durkheim classifications of the world flow from the human need for organization. The evolutionary argument is that we need social organization because otherwise our species cannot survive. Science then must be seen as part of the social organizations man has created for this purpose. According to Durkheim, both philosophy and later science were born out of the most elementary form of human organization, which in his view is religion, which he interpreted as follows:

"Before all religion is a system of ideas with which individuals represent to themselves the society of which they are members, and the obscure but intimate relations they have with it."[109]

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[107] Collins (1981a) p. 217.
[108] Bloor (1976) p.142.
[109] Durkheim (1912) p.8.

Society for Durkheim was something *sui generis*, that is, it has characteristics that cannot be reduced to other things. Because of this, the notion of society or 'the social' can have explanatory force with respect to other things.

Bloor extrapolated these views towards the study of the formation of scientific knowledge and the structures in which claims to knowledge come about. Following Durkheim, Bloor saw society as the primary locus of human existence. It is the well from which religion, philosophy and science have sprung. 'The social' presents itself as a middle ground between full (or individual) subjectivism on the one hand and absolutism on the other. Moreover, this focus helps to sidestep a number of persistent philosophical problems, because science can be studied via the discipline of sociology.

Still it would be a mistake to think that SSK has fully disconnected the study of science from philosophy. The strong programme in SSK is inspired by the work of various philosophers, of which the later Wittgenstein is the most important.[110] Bloor has emphasized that it is the conventional character of language, in which all social conventions are codified, that makes the "profound involvement of society a pervasive and inescapable feature of knowledge."[111] The conventional character of linguistic categories was captured by Wittgenstein in the concept of the language game and the idea that the meaning of linguistic expressions becomes manifest in the use of these expressions in particular situations.

People speaking a language can be seen as playing a game according to a set of rules. Which rules to follow is not a given necessity, it is a conventional matter. Such conventions are not dictated by individuals but come about in processes of social negotiation. Next to geographically separated nations, or historically separated localities, these ideas can also be applied to the study of particular societies. Social groups in one society differ because they play different language games. They use different sets of words and settle on different sets of rules. This perspective can be applied to opposing parties in a scientific controversy, when these parties represent different social groups.[112]

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110 Bloor wrote two books on the philosophy of Wittgenstein. See Bloor (1983) and Bloor (1997).
111 Bloor (1981) p.211.
112 This is what happens in the by now classic exemplar of the strong programme applied to history of science: Shapin and Schaffer (1985), in which rationalism and empiricism are depicted as different 'life forms', which is another Wittgensteinian notion. I discuss this work in the next chapter in relation to the principle of charity.

According to the 'meaning is use' doctrine, words do not have fixed semantic content. Instead the meaning of words can become clear only when the role they play in the language game is specified. This requires a localist study of concepts, which is known as finitism. Finitism holds that no concept has a fixed meaning, i.e. meaning is finite with respect to a particular context. There are always circumstances, causes and potential problems that stand between previous applications of a concept and the next application of it. Thus in every situation, when circumstances shift, meaning is created afresh and this happens through social negotiation.[113] For Bloor this holds equally for the empirical, mathematical and theoretical concepts used in science.

The views on language as a game, and on meaning as use, significantly undermine the logical positivist project of securing certain knowledge, because that project was based on the assumption of a neutral observation language and context-independent translation mechanisms to higher-order linguistic expressions. This does not fit with the conventionalism of Wittgenstein's views on language. As a consequence it also questions the whole formalist tradition in modern philosophy of science that the logical positivist project sparked off.[114] Interestingly, however, the linguistic turn in philosophy continues to have a deep influence. Even in the arguments of those who distance themselves from formalist approaches to science, the focus on language is still of central importance.

SSK may have claimed authority over philosophy in the explication of science. Yet, in spite of the rhetoric it has never been 'philosophy out' completely. As the strong programme is grounded on philosophical insights itself, only specific strands of philosophy are rejected while other philosophical views have come to replace these. I believe that any distinct approach to past science is based on a philosophical view on what science is and how it should be studied. It is therefore important to recognize the deeper philosophical views behind approaches to science, such as SSK, because historians of science, either consciously or subconsciously, inevitably come to operate with them.

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113 Finitism is close in outlook to the work of Quine and Rorty as well. In various places Bloor has subscribed to Quinean holism and to the coherentist view on justification associated with it, for example in Bloor (1981). Finitism bears many similarities to Rorty (1979). According to Rorty the task of epistemology was not to provide the ultimate foundations of knowledge but to study how knowledge operates in specific contexts.

114 See chapter 1, section 6.1.

3. 'Refutations' of the strong programme

The different perspective on knowledge SSK had to offer proved counterintuitive to many scholars in science studies. Consequently, it provoked harsh rejections and these led to fierce controversies. The fierceness is not surprising, if we consider that a struggle for authority in the science studies was at stake. Who was in the best position to offer interpretations and critiques of science? The philosopher, the sociologist, the historian or the scientist? This struggle was fought in a high moral tone over values such as fairness, honesty, respect and intellectual rigour.[115] Even among symmetrists an 'impartiality contest' was fought. The most radical symmetrists presented themselves as heroic democrats and levellers, calling others 'chicken' or telling them to 'go home'.[116]

In this section I argue that attempts to refute the strong programme have not succeeded. In order to demonstrate this I will have a close look at the Laudan-Bloor debate, the so-called Captives debate, and the 'Pinnick' controversy. The attacking strategy, aimed at complete refutation of the strong programme, is very often accompanied by simplifications and misrepresentations of the symmetrist position. This is for example the case in Tosh (2006) and Tosh (2007). Tosh misses the point that SSK offers an altogether different perspective on knowledge and the justification process. He also skips over the fundamental distinction between the principle of symmetry and generalized variants of the symmetry principle (see section 5 below).[117] Blindness for subtle, but nonetheless essential, distinctions and nuances, is a consequence of the belligerent strategy.

This strategy should in my view be avoided.[118] It is better to walk the distance with the strong programme and then ask whether the programme leads to undesirable restrictions on explaining past science. In section 4 I argue that this is indeed the case. The chapter from then on starts to work towards a treatment of determining factors in past science that is less restrictive, and hence more desirable, because it allows historians of science to investigate a wider variety of historical questions.

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115 Pels (1996).
116 Some of these debates are collected in Pickering ed. (1992). See also Pels (1996).
117 This is also pointed out in a reply by Kochan (2010).
118 Misrepresentations have also affected the unfruitful science wars in the 1990s, see chapter 1.

An early attempt at refutation of the strong programme can be found in Laudan (1981a).[119] Laudan delivered four main points of critique. First, he attacked the strong programme for relying on *a priori* assumptions. He saw no empirical proof for the dominant role granted to social factors in determining theory choice. Second, he accused Bloor of making a simple-minded distinction between teleologists on the one hand and (pure) empiricists on the other hand. Since both of these positions are obviously untenable, the strong programme came to appear as the only alternative. With a more fine-grained representation of positions in philosophy of science this would not have been possible. Third, Laudan argued that rationality is not a non-explanatory concept, as Bloor appeared to assume. Finally, Laudan conceded that symmetry works with respect to truth and success, but not with respect to rationality. Bloor had made the mistake to argue only against Lakatos' project of rational reconstruction, thereby ignoring other interpretations of the rationality concept, such as Laudan's own. Laudan acknowledges that some degree of relativism is unavoidable. Yet, what Laudan could not accept was the 'omnibus' relativism that is a consequence of the application of the principle of symmetry.

Bloor was not impressed by these charges. In his reply he referred to a number of case studies in order to show that the strong programme stood on firm empirical grounds.[120] To the argument that he had performed 'bad philosophy' Bloor remained indifferent. For him every philosophy of science granting a special place to the notion of rationality, in one way or another, would have to succumb to the symmetry principle. The classification he had offered was not of primary relevance to this argument.

The problem with the rationality concept is not that it does not explain anything, but it is the *self-explanatory character* of the concept. According to Bloor, a preference for simplicity of explanation, for example, is context dependent in two ways. First, the very preference for this virtue over others is a contextual matter and second, what actually counts as being 'simple' is not invariant across history. For Bloor it is therefore mistaken to believe that

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[119] The reply is Bloor (1981).

[120] In chapter one we have pointed out the influence of SSK on historiography of science. In 1981 Bloor could refer in this respect to Forman (1971), Farley and Geison (1974), Shapin (1975), Turner (1974), Frankel (1976), Hanvood (1976), MacKenzie (1978), Barnes and Shapin eds., (1979), Wallis ed., (1979). Later important books were Pickering (1984), Shapin and Schaffer (1985), Collins (1985), Biagioli (1993), Shapin (1994) and Shapin (1996). These are historical studies only and it is only a selection. There is much more, including many sociological case studies.

rational standards do not require any further explanation. In his view the operation of rational standards is fully dependent on social context.[121]

Finally to the argument that symmetry works with respect to truth and success, but not with respect to rationality, properly understood, Bloor replied that the circularity argument applies to any evaluative category. Against the charge that if this is indeed true, it will be impossible to tell anymore which cognitive features define science, Bloor simply replied that this is, in his view, indeed an empirical question!

In all fairness it should be said that Bloor seriously misconstrued Laudan's position in various places. It is not the case that Laudan wanted to rule out social factors, it is also not the case that Laudan interpreted rationality as a 'self-propelling phenomenon', and Laudan is also not a proponent of the history of ideas.[122] Laudan has in fact defended quite a moderate notion of progress based on the increase of problem-solving effectiveness.[123] In this theory it is always rational to accept theories that solve the most problems. It is however a contextual matter which problems scientists select to work on, and how they weight solutions to problems, if these pull in different directions of theory choice.

Thus Laudan's philosophy of science is highly context sensitive but it is true that he does make a distinction between social and rational factors and maintain that if irrational choices occur, they should be explained with reference to social factors only. Notwithstanding the misinterpretations of Laudan's theory, Bloor could never go along with this. In my view there seems to be no definitive argument that forces a choice between Bloor's descriptive naturalism and Laudan's normative naturalism. It is more or less a matter of perspective which position deserves support.

Another example of an attempt at refutation of the strong programme can be found in Pinnick's review of Shapin and Schaffer (1985), from which ensued a harsh discussion.[124] Pinnick argued that *Leviathan and the Air-Pump* was a clear example of bad historiography. The debate between Hobbes and Boyle is

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[121] Following Durkheim, Bloor did accept that a form of minimal rationality, required for survival, is present in any human society. But he hastened to add that such a concept is hardly interesting for the explication of theory choice in past science because it is not specified enough. For more discussion on this point see chapter 6 on naturalistic projects in philosophy of science.

[122] Bloor (1981) p.201, p. 206 and p.210.

[123] Laudan (1977). Laudan's approach to science falls under normative naturalism. For a discussion of normative naturalism see chapter 6.

[124] Pinnick (1998). The debate between them was published in *Social Studies of Science*.

presented as a central one in the 17th century. For her, no justification for the plausibility of this claim is however offered. According to Pinnick doubts about the experimental method were widespread and shared by everyone. The Hobbes-Boyle controversy is thus at best exemplary for the age. Shapin and Schaffer had also made selective use of source material. They deliberately included material that made Hobbes and Boyle look like complete adversaries and excluded material that made a more nuanced picture possible. According to Pinnick, the dichotomy between Hobbes and Boyle is an *artefact* of selective filtration of the historical evidence. Finally, she argued that the modern thesis concerning the impossibility of performing crucial experiments is read into Hobbes' objections to empiricism, which she saw a clear example of the bad practice of anachronistic torturing of history.

In reply Shapin and Schaffer stated that they pointed out many similarities between Hobbes and Boyle. They argued that *upon* this record they construed a sophisticated account of the debate through which the differences between the two could be appreciated more clearly. Pinnick had failed to see this because she dogmatically wanted to see only similarities. In reply Pinnick accused Shapin and Schaffer of being dogmatic about the *a priori* thesis of social causation, which in her view has only the effect of distorting historical reality. No wonder that Shapin and Schaffer followed this with another harsh reply, but luckily the 'debate' ended here.

There is quite an interesting point involved in the discussion, namely how to weigh the various differences and similarities between contestants in past scientific controversies. Moreover, should we focus on one controversy in isolation or place it in a wider framework? Yet the tone in which the debate was carried out did not allow for a fruitful exchange on such crucial matters. This, in my view, is mainly due to Pinnick's original intention of refuting SSK, by way of beating down one of its most profound examples from historiography of science.

In the so-called Captives debate Scott, Richards and Martin pointed out that "an epistemologically symmetrical analysis of a controversy is almost always more useful to the side with less scientific credibility or cognitive authority."[125] The requirement to be impartial actually contains a hidden value judgement, namely that all participants in any controversy are always equally credible. Symmetrists are therefore always 'captured' by the least credible parties. The impartiality principle is therefore incoherent in itself. In a short reaction Collins simply embraced this criticism and pointed out that he liked to side with 'the

[125] Scott, Richards and Martin (1990) p.490.

underdogs and the bad guys.'[126] He argued that this stance is necessary to take away the self-evident character of credibility and authority attached to scientific theories. This again strikes me as an issue that cannot be settled by argument.[127]

The discussion on the strong programme is relevant to the arguments from presentism and underdetermination, given in chapter 1. These question the desirability of evaluative historiography. It is no wonder then that attempts at refutation of the strong programme have failed.[128] It makes me think of a remark Popper once made:

> "I regard conventionalism as a system, which is self-contained and defensible. Attempts to detect inconsistencies in it are not likely to succeed. Yet in spite of all this I find it quite unacceptable."[129]

If there is something to find unacceptable about the strong programme, and I believe it has some unwelcome consequences for the study of past science, which are discussed in the next section, this requires one to oppose it with a more desirable approach to past science, because it avoids the unwelcome consequences.

4. The strong programme: undesirable consequences for historiography of science

In this section I list the, in my view, undesirable consequences of the strong programme for historiography of science. Next to this I point out that, when going along with the strong programme, two inconsistencies occur that the programme cannot get rid of.

One of the most profound effects of the strong programme is that it restricts historical investigation to local contexts only. The more local and specific our view of knowledge becomes, the harder it is to see how it travels. And yet knowledge does travel: how should we account for that? This has been recognized as a problem of delocalization, which was formulated by Peter Galison as follows:

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[126] Collins (1991).
[127] Yet there is something about the impartiality issue that is problematic for SSK as the principle of impartiality does appear to sit rather uncomfortably with the symmetry principle on meta-level (see below).
[128] At least they have not stopped symmetrical study of past science at all, see Golinksi (2005). Even the issue why scientists continue to perceive themselves as impartial truth-seekers has been addressed. In Mulkay and Gilbert (1982) it is for example argued this perception is the result of social constructive process of identity building.
[129] Popper (1968) p.82.

"If the original production of scientific knowledge is so reflective of local conditions – whether they are craft techniques or religious views, material objects or forms of teamwork, how does *de*localization take place?"[130]

The problem of delocalization has also been addressed as the problem of construction of knowledge (Golinski) or as the problem of the movement of local knowledge (Secord).[131] Earlier Rouse had something similar in mind with his problem of theoretical decontextualisation.[132]

SSK can refer to negotiation that takes place when one local context comes into contact with another (for example through military conflict, the expansion of an empire or travelling long distances). It is however unclear how to understand this interaction within the SSK framework. Should we see the zone of interaction as a new type of context? Or is it part of both the two originally distinct contexts? And if knowledge from one context turns out to be relevant in another context, why is this the case? Arguably, it has often happened that accepted knowledge in one context turned out to have profound effects in another context, while the content of this knowledge was not changed very much in the course of adaptation. If this is accepted we come close to saying that ideas can have a determining effect on social circumstances. At least SSK has to admit that the acceptance of one idea is not dependent on one specific set of social factors, as both the interacting contexts come to accept it, and they are not exactly similar in terms of social factors. It is difficult to explain such interactions within the SSK framework because they problematize the very notion of context. Yet SSK explanation of past science relies heavily on clearly defined contexts because only within such boundaries can the role of social factors in the determination of theory choice be adequately specified.

We can see the problematic effects this approach to past science has on the study of scientific controversies. First, SSK demands that a definite closure is reached at every 'junction' in the history of science in which a controversy was played out. Secondly, in order to make the story of competing interests work; the interests of the conflicting parties must be represented as full oppositions. Hence differences between contestants tend to be stressed more than similarities.[133] According to Pels (1996) the symmetry postulate has the effect of

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[130] Galison (1997). Repeated in Galison (2008), problems 7 and 8.
[131] Golinksi (2005) p.33 and p.133, Secord (2004) p.660.
[132] Rouse (1987) p.112.
[133] The point that should have come across in Pinnick's critique on Shapin and Schaffer. It is also made in Pels (1996) and Schickore (2009).

over-schematization and hence obscures and misplaces the more interesting similarities and differences between contestants in a controversy.

The word ‘closure’ is probably already misleading. Settlements of controversies often have a temporary character. Closure is often partial, letting other things rest. Later on they often stand open for revision. And conflicts can be fought over again when the incentive to do so arises. These intricacies become visible only when the horizon of the historian is widened. Yet the localism of the SSK approach blocks access to a diachronically wider perspective on past science.

Such a perspective would also put the problem of underdetermination in another light. Martin Rudwick, in his account of the Devonian controversy in the history of geology, convincingly argued that at some point in time, it was no longer possible to refuse to join the consensus over the Devonian system. The two dissenters that kept doing so violated norms of good scientific conduct.[134] Allan Franklin presented the same argument in his account about the history of the idea of gravitational waves. According to him it was proved, beyond reasonable doubt that the original ‘detection’ of the waves by Weber was due to a misinterpretation of the experimental results. The ones who thought so checked and double-checked their findings, forwarded their results to others for critical examination, and tried various ways of interpreting the results (using differences in scale, methods of calculation, etc.). The fact that Weber did not do all this but still stubbornly kept maintaining his earlier results, must, according to Franklin, simply be qualified as irrational behaviour.[135]

Against the interpretation of Rudwick however, Collins and Pinch maintained that the dissenters had a genuine position to defend.[136] Equally, against Franklin, Collins maintained that Weber had the right to follow his own method of interpretation even if no one else did so anymore.[137] I am inclined to side with Rudwick and Franklin on this issue. Their historical narratives consist of diachronic sequences of interrelated developmental steps. If we perceive past science as a collection of research programmes, gradually unfolding in the course of time, we do not need to demand full closure of controversies at every step in the development, and this opens up the possibility of dealing with the

[134] Rudwick (1985).
[135] Franklin (1998a).
[136] Pinch (1986), Collins (1987).
[137] See Collins (2004) on gravitational waves.

problem of underdetermination in another way and avoid the consequences SSK has drawn from it.[138]

Also stemming from SSK's localism is a ban on comparative historiography of science. With the strong programme it is, for example, difficult to account for the simultaneous occurrence of similar discoveries or claims to knowledge in distinct localities. The specific social circumstances in these situations cannot have been the same, so why do different social structures sustain the same claims to knowledge? Take for example the interesting case of Galileo and Descartes (together with Beeckman), who both arrived independently at a, by current standards, mistaken formula of free fall. Both initially thought that the speed of the object was proportional to the distance covered. Only Galileo managed to correct this into elapsed time.[139]

Koyré ascribed the double occurrence of the same error to the reigning 'thinking cap' of impetus physics, which had influenced both Galileo and Descartes. In SSK this explanation would require substantiation in terms of widespread correspondence of social structures and cultural factors, shared by a group of European scholars. But if we go this way, a historical context is no longer geographically identifiable. Again the crucial notion of context, on which the strong programme depends, is problematized. Also, the question why Galileo managed to correct his earlier theory, becomes interesting in comparison to the others, who did not manage to do so. Tackling this issue in some part depends on a comparison between the two cases. Yet, comparative analysis of past science cannot be part of the strong programme as this programme insists on causal explanations. While it is true that comparative analysis can help to identify causal factors in history, the mode of analysis is not causal in itself.[140]

Next to the undesirable restrictions that SSK puts on historical explanation it runs into inconsistencies if one wants to live up to all of its principles at the same time. As many authors have pointed out, the neutrality/impartiality principle does not sit very well with the symmetry postulate on a meta-level.[141] As the reflexivity principle says that the strong programme must be applied to itself, symmetrists must be neutral with respect to other approaches to past science. This cannot be defended, while at the same time claiming that the

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[138] In chapter 7 I develop this diachronic view on the history of science in more detail.
[139] Koyré (1978) discusses the episode at length.
[140] I have not even mentioned comparison between *historically* distinct localities. This is surely out of reach of SSK and thereby leaves a host of historical interesting questions unaddressed.
[141] For example Pels (1995), Tosh (2006) and Schickore (2009).

strong programme is a better approach than other approaches to past science. Were symmetry the only principle to follow, this would not be much of a problem because it would allow for partisanship on a meta-level. However, in combination with the other two principles, impartiality and reflexivity, the strong programme cannot be coherently defended on the meta-level.

Another point of difficulty involves the question whether social factors are allowed in the explication of past science when the operation of these factors was not fully clear to the historical actors in question. There are many socio-cultural values, conventions, rules of conduct, etc., that are so self-evident to participants in a society that they are not consciously aware of how they influence their decision making. Why is it allowed to be presentist in this respect and use current sociological knowledge in the explication of past decision making, whereas this is not allowed for the best of our current insights from the natural sciences? There is something uneven about this. It is exactly at this point that approaches based on generalized principles of symmetry differ from the strong programme.

The localism of SSK leads to a number of problems for historiography of science. Among these are the study of the interaction between 'distinct' contexts, the over-schematic treatment of past controversies and the overly restrictive conclusions drawn from the problem of underdetermination. The lack of comparative ground makes it hard to account for simultaneous occurrences of theory choice in distinct contexts, and makes it equally hard to speak of qualitative improvement. Finally the assumptions of the programme are not consistent. The uneven balance in the use of present day knowledge, as the use of sociological knowledge is allowed but natural scientific knowledge is not, is unaccounted for. And the demand for neutrality cannot be maintained with the principles of symmetry and reflexivity at the meta-level. In order to remedy these problems scholars have generalized the initial principle of symmetry. In the next section I discuss how and in the section thereafter the main approach to past science based on generalized symmetry, namely posthumanism, is critically evaluated.

5. Generalized symmetry: posthumanism

A number of extensions of the original symmetry principle stand out in the literature. They all involve erasing boundaries between what were previously regarded as distinct categories. A clear example is giving up the boundary between science and technology. Pinch and Bijker, for example, argued that we should stop regarding technology as applied science, i.e. as spin-off of pure science. According to them the relations between theoretical claims and technology are so intricate, that we cannot even make a strict distinction between facts and artefacts.[142] Historians and sociologists have, in similar ways, questioned other distinctions such as mind and hand, theory and practice, and discovery and justification. Posing more symmetry is synonymous with erasing boundaries. It creates a less discriminated object of study for historians of science. Or, in other words, more things shift from the resource side and become topics of investigation.

Giving up these distinctions means moving beyond the original symmetry principle, because that principle was formulated only in relation to the acceptance of belief. Yet, in themselves the extensions of the principle do not challenge the mode of explaining past science of the strong programme. They can easily find a place in, and often strengthen, the programme of explaining the course of science with reference to socio-cultural factors.[143]

A real shift in thinking came about with the so-called posthumanist approaches to science. The turn towards posthumanism has also influenced research in history of science deeply, and this continues to be so to the present day. For both these reasons posthumanism requires an elaborate discussion. Posthumanists think that SSK had made a step in the right direction. With the introduction of the principle of symmetry they undid science from its universality and its 'holiness', making an empirical study of science possible. Yet the mistake of SSK had been to put 'the social' in the place of the old universality ideal, as another mythical entity. Posthumanists argue that SSK works with an *a priori* preference for social factors over other factors. Hence, despite the insistence on a principle of symmetry, a deep asymmetry between the natural and the social has remained in the strong programme. In order to make a truly empirical study of science possible the boundary between the social and the natural must be erased, that is, the principle of symmetry must be generalized.[144]

For posthumanists the natural world and the social world grow up together. No social structure (possibly given by sociology) and no natural structure (possibly given by the natural sciences) can play a role in the explanation of past science, because these structures stand in need of explanation themselves.

[142] Pinch and Bijker (1984).
[143] This also holds for the programme of reflexivity that was developed to combat the problem of coherence on the meta-level. See Woolgar (1988) and Ashmore (1988).
[144] The first use of the term 'generalized principle of symmetry' in this sense is probably (Callon 1986). An important section on the generalization of the principle by one of its main proponents is Latour (1993) pp. 94-96.

Structures in the world are the result of an interaction process of agents (also called actants or actors), which can be *both* human and non-human. In SSK humans occupy central stage because of the dominance of social factors, which is a human category. With the principle of symmetry generalized, non-human agents acquire an important role too as one of the determining factors in science, hence the term 'posthumanism'.

In order to understand this approach to science I first focus on the theory of one of its best-known proponents: Bruno Latour.[145] Before the world is classified in social structures and natural structures, including the institution we call science and the knowledge claims that are defended in it, 'things' have not taken shape yet. Terminology is lacking here, but as something needs to be there in order to interact, Latour prefers to speak of quasi-objects. All the quasi-objects together make up everything there is in the world. They enter into processes of interaction. Latour speaks of actors who are constantly mediating with other actors in order to achieve networks of alliances. Hence he called his theory Actor Network Theory (henceforth ANT).

Processes of mediation repeat themselves continuously. In the process more stable structures, or networks of associations, gradually emerge. These also include our present-day classifications of the natural and the social. Such classifications can however be upheld only because they are sustained by supporting networks. Networks can acquire relative stability over time. Yet, nothing in the networks is permanent as every stable situation can be destabilized through further processes of mediation. Historicity is all-pervading in ANT. Nothing can escape the torrent of history. This also holds for all analytical categories. Real historicization of science, and hence a fully empirical approach, must divorce itself from any form of a-temporality.[146]

Latour's most famous case study is on Louis Pasteur and his theory of microbes. According to Latour, Pasteur's theory beat its competitors (mainly Koch) because Pasteur was a shrewd negotiator who managed to create a strong network of alliances among fellow scientists, politicians *and* the entities in nature, which we now refer to as microbes, but which did not exist before the process of network building started.[147]

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[145] In Latour (1987) a programme for the study of science consisting of 7 rules of method and 6 principles is presented. Rules of method 3 and 4 together form the generalized symmetry principle. Note that corresponding ideas can be found in the work of Callon and Stengers.
[146] Latour (1999), chapter 4 'The Historicity of Things'.
[147] Latour (1988), Latour (1987) contains a number of other case studies such as the double helix theory of DNA.

Once relatively stable networks are in place, the process of interaction with actors acquires qualitatively different features. Actors will have to mediate in relation to the existing networks and networks of alliances can compete with each other, which introduces a new level of competition. Further, Latour argues that on the network level characteristics emerge that cannot be found on the actor level. For example, he asserts that the larger the network becomes the stronger the formal 'nucleus' needs to be to keep the network together. Mathematical or logical formalizations can play this role. In Latour's view these are not a measure of truth but represent a demand of strength.

Next to this Latour argued that scientific theories or scientific instruments must be in finished form in order to move through networks. In this context he introduced analytical notions such as 'immutable mobiles' and 'black-boxing'. There are quite a number of belligerent terms in Latour's vocabulary. He basically sees the selection of scientific theories as a survival of the fittest between competitive networks. The strongest network of alliances will win over its rivals in a scientific controversy. After the closure of a conflict the winning theory is 'black-boxed'. People tend to forget the contingent process that is behind the establishment of the theory. The theory becomes immutable (it is taken as a fact) and because of this it becomes mobile and helps to bind a network together.

ANT leads to a clear research agenda for the historian of science. Foremost, he or she must follow the interactions of actors and simply describe these, like an anthropological participant observer.[148] Because this is not fully possible for historical study, the historian must rely on the inscriptions actors have left behind and study how these have been used in processes of negotiation. Note that symmetry breaking is explained in ANT only through the concept of mediation. Actants are always the cause of scientific outcomes (type level) but their specific interactions differ from case to case (occurrence level). The fact that actants mediate means that they are capable of exerting power and offer resistance to pressure. A more profound analysis of the concept of agency, beyond this capacity for mediation, is not required according to Latour. He needs to work with a broad definition of agency in order to include all actors, both human *and* non-human.

While following the actors, historians of science must of course also study how networks are formed, grow (win over competition) and decline (lose to competition). Changes in science are synonymous with changes in networks.

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[148] This is exactly what is done in Latour and Woolgar (1979). Their approach bears similarities to Geertz's 'thick description'.

The degree of acceptance of a claim to knowledge is similar to the strength of the network that supports this claim. Historians of science can however re-open the black boxes by providing a detailed study of the interactions that have occurred that in the end resulted in the accepted theories. This gives the most detailed access to the contingent aspects of knowledge formation. It can also help to reveal important aspects of history that have become forgotten after black-boxing has occurred.

With the blurring of a clear distinction between what is natural and what is human, and with its focus on networks, Latour's ANT is part of one of the strongest currents in present-day philosophy of science, and beyond. Network thinking is nowadays very common in many areas, quite possibly owing to the end of the Cold War, through which the world is no longer split up in distinct compartments, and to technological developments that have integrated the world such as the internet, GPS and mobile communication.[149] The focus on networks involves a shift to a relationalist view of reality. For Latour the process of establishing relations is fundamental because things exist only via lists of associations. In this respect works from the continental tradition of philosophy appear to become relevant in the traditionally analytically dominated field of philosophy of science.[150]

Above we have seen that Latour used the notion of quasi-objects in order to refer to the something that is there to enter in processes of interaction, out of which natural and social structures emerge. This however is not just a matter of classification. Posthumanist philosophers have also addressed the issue of ontology, with the idea that natural objects and artefacts merge together in new types of objects. It is by no means clear how to capture these processes and what to call these new types of objects.[151] If we take posthumanists' ideas seriously

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[149] In Latour (1993) p.145 an explicit connection between the new study of science and the fall of the Berlin Wall is drawn. This however overstretches the point. Collins (1985) had already published a network perspective on science, although it is true that this differs from posthumanism on crucial points. For a view on history in terms of networks (webs) see McNeill and McNeill (2003). Barábasi (2002) contains interesting material with respect to network analysis in all areas of society. Other publications can easily be cited as well.

[150] Gilles Deleuze is an important source of inspiration in this respect see Braidotti (2011), (2013). Note however that in the analytical tradition some take relationals as the ultimate constituents of the universe. Muller (2013) for example states that entities are discernible not by properties but primarily by relations. This is also defended in variants of structural realism. See French and Ladyman (2003), Ladyman and Ross (2007), Esfeld and Lam (2008).

[151] See Haraway (1989) and Haraway (1991). Haraway proposes a new theory of truth which she calls co-respondence, instead of correspondence. Ideas on New Materialism

this means that answering the central problem in this thesis in some ways requires a departure from analytical philosophy and a venture into continental philosophy of science and feminist epistemology.

Returning to posthumanist models for the study of science, I would like to compare Latour's ANT with Pickering's idea of the mangle of practice. For Pickering all determining factors of history enter together in a mangle. In a process of interaction things come about, including institutional academic structures and scientific theories. Ultimately only agents can be responsible for changes. Pickering studies the interaction of agents via the concepts of resistance and accommodation. Like Latour he advocates an empirical strategy of following the actors. We should describe what they do, 'in the thick of things'. Knowledge for Pickering is neither a construction, because reality plays an important role, nor a revelation of reality, because reality is *active* and not passively waiting to be discovered. He speaks of ontological transformations, hybrids, mediation and emergence. His model of science is very dynamic as no equilibrium ever is a perfect adaptation to the environment and will always be challenged to change. Pickering therefore purposefully uses evolutionary concepts to articulate his posthumanist views on science.

There are a lot of resemblances between Latour and Pickering. Latour's ANT can also be read as an evolutionary theory, with its constant trials of strength.[152] They both centre on actors and allow a determining role for human and non-human actors in the course of science. Differences between Latour and Pickering are therefore differences in emphasis, not in principle. Latour has developed a number of analytical notions, which can be used as tools of description, when it comes to the study of networks, which are missing in Pickering. Pickering however pays more attention to the notion of agency. Where Latour does not make a clear distinction between humans and non-humans, Pickering defends the view that only humans possess intentionality. The agency of non-humans mostly manifests itself as resistance to human intentionality. There is thus a clear asymmetry between humans and non-humans in Pickering's model as different mediating powers are ascribed to them. This asymmetry can however also be detected in Latour when he states

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are collected in Dolphijn and Van der Tuin (2012). Daston and Galison (2007) suggest that we may be on the brink of a new sense of objectivity, which they tentatively call the nanofacture, inspired by developments in nanoscience. The nanofacture is about presentation (actively interfering in nature) instead of representation. Perhaps we also need ontologies on other levels such as an ontology of processes in order to capture the hybridization of natural entities and human artefacts.

[152] See also chapter 6 on evolutionary approaches to science.

that humans are to be seen as the 'weavers of morphisms'. The freedom they possess is the capacity to sort combinations of hybrids. Non-humans do not possess this freedom, at least not in a comparable degree.[153] Finally Pickering is inclined to lean towards the mystical aspects of the mangle metaphor and to perceive everything as a great flow of being, without any (essential) distinctions. With Latour this is much less the case.[154]

Posthumanism met with a delayed reception in history of science, but is currently very much *en vogue*. This is not due to deep philosophical reflection but the result of an increasing feeling of discontent with the prevalent localism in the field.[155] An important keynote lecture at a meeting of the History of Science Society in Halifax in 2004 by Jim Secord can be seen as the kick-off of all kinds of research projects in the circulation of knowledge. The aim of these projects is to study how knowledge travels from one locality to another. Inter-local contact is often represented as connections in networks. Historians study how actors were connected in these networks and how they interacted with one another. For this they make use of 'trans' words like transfer, translation, transition, transaction, transcription, transformation, etc., (but *not* transcendence!). In their explications historians also frequently make use of notions introduced by Latour, such as 'inscriptions' or 'immutable mobiles'.[156]

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[153] Latour (1993) p.141.

[154] In later works Latour appeared to call for a departure from ANT towards even more symmetry. In Latour (1998) he wrote: "There are four things that do not work with actor-network theory; the word actor, the word network, the word theory and the hyphen! Four nails in the coffin." We should just start thinking in a flux or a flow in which no distinctions can be made at all. However, in other places Latour clearly sticks to ANT, see for example his 2005 book titled *Re-assembling the Social. An Introduction to Actor-Network Theory*. If Latour has changed his position at all, I believe this change has not been significant. In any case the pure flow thinking is just too mystical to be of help for the study of past science.

[155] See section 4.

[156] Of the early examples Latour's *Pasteurization of France* (1988) has already been mentioned. In Pickering and Guzik eds. (2009) a number of historical case studies based on the mangle concept can be found. Another important book is Porter (1996). Relevant publications after Secord's lecture are Raj (2007), Roberts, Dear and Schaffer (2007), Cook (2007), Davids (2008), Raj et al. eds., (2009), Dupré and Lüthy eds. (2011), Roberts ed. (2011). One of the messages of these works is that circulation of scientific knowledge is indissoluble from economic traffic, processes of nation building, colonization, etc. The dominant focus is often on material culture following the slogan that 'books, not -isms pass hands'. Many conferences on the history of science are organized with a focus on circulation of knowledge: for example the 4th international congress of the European Society for the History of Science (2010) took as its theme 'The Circulation of Science and Technology' and the 4th Woudschoten conference of the History of Science in the Low Countries (2011) opted for 'Locations of Knowledge'.

Even though the focus of research and the use of posthumanist terminology are often not accompanied with a defence of generalized symmetry, such historical studies do rely, at least methodologically, on such a principle. It can be expected that the theme of circulation of knowledge, the analysis of past science in terms of networks and the ontological issues involving the 'new' materialism will continue to dominate the agenda in history and philosophy of science in the near future. Therefore it is important to reflect on the symmetry principle for historiography of science today.

The posthumanist approach to science offers a solution to two problems with the strong programme. First of all the network concept offers a way out of the problem of localism. With the network concept, the troublesome concept of context can be avoided. Posthumanists need not be clear on what exactly counts as a locality and what not. What is local, extra-local, or even global simply depends on how large the network is. When two distinct networks come to interact with one another a new network is, or new networks are, formed. Since posthumanism does not need the relative stability of social factors in the explanation of past science it is in a much better position to deal with such interactions.

Secondly, the social is undone from its mythical authority. Social factors remain important in the explication of past science but as one factor among others. On the one hand this has been achieved by further naturalizing the study of past science, that is the insistence on causal explanations of theory acceptance and rejection has become even stronger than in the strong programme. On the other hand we now have no access to sociological theory anymore as a resource of explanation. The number of resources has been decreased even further, compared to the strong programme. This has been a conscious aim with an empiricist research programme in mind, which Latour has put as follows: "The whole challenge of the exercise is to generate a maximum of differences by a minimum of means."[157]

Because the process of further naturalization has been a gradual one, commentators discussing the 'social' approach to past science often fail to distinguish clearly between the original claims of the strong programme and posthumanist approaches to science.[158] For a fair debate on approaches to past science, posthumanism must be seen as a real shift in thinking about science,

[157] Latour (1993) p.112.
[158] This is the case in Kitcher (1998), Sokal (1998), Tosh (2006) and Tosh (2007).

not just methodologically but also metaphysically.[159] As said above, the SSK approach represents a form of perspectivism. Bloor, for example, combines ontological realism with epistemological relativism. The world allows for many different perspectives, which can be called systems of classification, systems of belief or conceptual schemes. All these perspectives are engendered by humans and in the last instance natural factors do not play a decisive role in the acceptance of them. According to Bloor all perspectives are equally distant from nature.

The strange thing of perspectivism is that the world is there somewhere, as it really is, but we are never able to reach the actual state of affairs. The world allows for the projection of a large number of perspectives but there is an unbridgeable distance between all those webs of belief and the world they are supposed to represent. Latour has, in my view rightly, pointed out that something strange is going on here. He argues that with perspectivism we have lost direct contact with the world and one needs to move beyond it in order to regain this contact.

Interestingly a similar way of reasoning can be found in Davidson's famous attack on the 'third dogma' of empiricism.[160] In this paper Davidson supported Quine's earlier attack on the two dogmas of empiricism, namely reductionism and the analytic-synthetic distinction, but argued that Quine had mistakenly left a third dogma untouched. This was the dogma of the dualism between conceptual schemes and empirical content of sentences. Like Latour, Davidson argued that if we leave this dualism behind we can come to a theory of science that works with an idea of direct mediation between our conceptual schemes and the world, thereby regaining contact with that world. The similarity in views has been noted by Kremer:

> "The quickest way of expressing this commonality is to say that philosophers as diverse as William James and Friedrich Nietzsche, Donald Davidson and Jacques Derrida, Hilary Putnam and Bruno Latour, John Dewey and Michael Foucault—and Richard Rorty, of course—are anti-dualists. They are trying to replace the world pictures constructed with the aid of metaphysical dualisms inherited from the Greeks (essence and accident; substance and property; appearance and reality, etc.) with a picture of a flux of continually changing relations."[161]

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[159] It is for example significant that Latour and Woolgar changed the title of their 1979 book *Laboratory Life: The Social Construction of Scientific Facts* into *Laboratory Life: The Construction of Scientific Facts* for the second edition. Dropping the word 'social' was done purposefully to avoid association with SSK.
[160] Davidson (1973).
[161] Kremer (2007) p.71.

The commonality between Latour and Davidson is important for the current project, as it also visible in the discussion of the principle of charity in the next chapter.

Beyond dualism there is so to speak a single ontology of events. These are best captured via the notion of relations. It is only through establishing relations in a continuing process of mediation that structures in the world come about. To consider nature as being rich enough to allow for many different classifications, is already wrong from the posthumanist perspective because nature is not stable, instead its structures are shaped in continuous processes of interaction.

Latour has therefore described this position as relativist relativism:

"The relativist relativist, more modest but more empirical, points out what instruments and what chains serve to *create* asymmetries and equalities, hierarchies and differences."[162]

The more apt term, that he also uses, and that fits better with the work of other scholars, is simply relationalism.[163] The theory is dynamic and favours notions that express openness such as hybridization, emergence, adaptation, accommodation and meditation. Stability of networks is only temporary, as they are in constant flux. In contradistinction to SSK, posthumanism therefore represents a relational ontology of becoming. This is one of the main reasons why it leads to a different approach to past science, and should be dealt with accordingly.

6. Posthumanism: attractions and difficulties

Posthumanism has a number of attractions over the strong programme. The relationalist stance has made it possible to be undogmatic about determining factors in science. This allows for more flexibility with respect to both natural and social factors. The notion of local context is re-interpreted through the notion of the network. This removes conceptual difficulties for extra-contextual studies of past science. Finally the treatment of closure of controversies is less strict, as all forms of stability have only a temporary character in posthumanism.

Nonetheless, I believe that there are also serious difficulties with the posthumanist programme. A first problem is the confusion of levels of

[162] Latour (1993) p.114.
[163] Latour (1999a) p.161 speaks of relationism, but relationalism is a more current term.

analysis.[164] Sometimes the social and natural structures that have emerged are seen as products of our classificatory schemes. This has occurred in interaction with natural entities, such as 'microbes', but it is we who eventually introduce the concept of microbes to speak about the natural world. This is different from speaking about objects directly, relating to the issue how we should perceive objects that are hybrids of natural objects and cultural artefacts. A clear example of this confusion is Latour's third methodological rule from *Science in Action* (1987):

"Since the settlement of a controversy is the cause of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled."

This rule contains a shift between theories of nature (representation) and nature itself in just one sentence.

Latour also does not distinguish consistently between the perspective of the scientist and the meta-perspective of the sociologist or historian of science. The latter should investigate what scientists do, or have done, and generally refrain from getting involved in the direct investigation of nature. Pels (1995) suggests that Latour sometimes blurs this distinction because of a tendency to equate science with nature and politics with society. The study of science and politics can then also involve a direct study of nature and society. All this leads to confused mixtures of ontology, epistemology, sociology and ethics.

Reasons for this confusion can also be sought in the decrease of conceptual resources that the generalization of the symmetry principle has brought. Past delineating concepts such as knowledge, science, nature and society, have lost their explanatory value. The new conceptual alternatives are not always sufficiently clear, mangles and quasi-objects for example are vague notions, and the characteristics of agents and networks are not specified in much detail. Networks can in fact function as both explanans and explanandum. They are a topic of investigation for the historian of science but can also function as a resource in explaining how knowledge claims have become accepted or rejected.

Another problem with giving up resources for the explanation of past science is the ensuing lack of selective criteria. Every agent and all determining factors may become relevant to the study of past science. What about the meals that are served in the institutes in which scientists work? How do they travel from home to work? What about their family background? Etc. Once you start speculating

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[164] See also Pels (1995), Sokal (1998) and Bloor (1999).

in this manner everything seems to become relevant in 'the great dance of agency'. If everything is potentially relevant to the study of past science the aim must be to literally describe all past interactions that have occurred between humans and non-humans. Not only is this simply impossible, it would also be rather pointless because it would amount to an exact repetition of the past without offering an analysis of the historical development of science, which is what the goal of historiography of science must be. Of course description of historical contexts is a vital component of such analyses but it cannot be the overriding purpose of the whole enterprise.

The problem is that there are no clear criteria to select what is relevant and what to leave out of our historical accounts. Why couldn't we simply choose an approach to past science based on demarcation? For example, why not simply select the Mertonian values? If the majority of historians of science can be made to agree with this choice, then the network of historians of science has accepted that this is the best way to approach past science. On what grounds could the posthumanist criticize this?

What has become unclear in erasing almost all analytical boundaries is why we should involve in a strategy of following the actors. What purpose does that serve? In SSK, a clear concern with modern society stood at the basis of their programme. Modern science in their view could be beneficial to society but in order to control its negative effects, science had to be brought down from its universal pedestal. Can one in posthumanism still address the question why modern science is such a successful enterprise? Can one answer the question why knowledge travels from one locality to another? It seems that with the posthumanist approach we can answer only the 'how' questions and describe how things have come about.

What is won in posthumanism in descriptive scope is lost in analytical scope. It is striking that posthumanists often fall back on SSK-style explanations in terms of conflicts of interests, processes of negotiation, trials of strength, the effect of power relations and processes of standardization to address the 'why' questions. It then becomes unclear indeed how much the generalization of the symmetry principle has brought for the study of past science. Moreover the other problems that we have listed with SSK, such as the lack of comparative ground and the demand for full closure of controversies, remain equally troublesome in the posthumanist approach.

Last but not least, there is still a problem with the inclusion of rational factors in the set of determining factors in past science. Posthumanists are not inclined to grant these factors an important role. Especially striking is Latour's advice in *Science in Action*. Rule 7 says:

> "Before attributing any special quality to the mind or to the method of people, let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors."

The implication is that this does not occur. I believe that the exclusion of cognitive factors has to do with the fear that including them automatically leads to the attribution of 'special qualities of the mind.' And hence this would lead us all the way back to approaches of past science based on a demarcation between rational and social factors. This is why, as soon as cognitive factors are mentioned, symmetrists immediately shout 'halt!'. They suffer from what I would like to call a demarcation reflex. Unfortunately this reflex restricts assessments of past science to quantitative terms, namely in terms of the length, depth and degree of interconnectedness of networks. It blocks access to real evaluations of past science, which are of a qualitative kind.

So far we have discussed the two major approaches to past science based on two principles of symmetry. We have found that the application of these approaches to the study of past science is problematic. Generalization of the symmetry principle solved a number of the earlier problems we had with SSK (but not all), but created new problems by itself. Still, I am attracted to symmetrical study of past science because I think that an approach based on demarcation will inevitably lead to the application of rational norms when these should *not* be applied (see also chapter 4). We must however find a way to include cognitive factors in our set of determining factors.

At the end of this chapter a tentative proposal for symmetrical study of science, including a new idea to account for symmetry breaking, is presented with these goals in mind. With this proposal I believe that most of the problems with symmetry and generalized symmetry we have discussed above, can be answered. On the other hand the proposal still needs to be related to a platform for the study of past science that enables comparative evaluation, if it is to work as a basis for evaluative historiography (see chapters 6 and 7).

7. A proposal: new relationalism

Approaches based on demarcation use different sets of factors to explain correct and incorrect claims to knowledge. In more schematic formulation we thus have something like (1):

(1) Type factor A explains acceptance/ rejection of x, type factor B explains acceptance/ rejection of y, where x and y are scientific theories and x belongs to the right (course of) science while y does not.

Both approaches based on symmetry and approaches based on generalized symmetry can be captured with the same formal structure, which reads something like (2):

(2) Type factor A explains both x and y, where (a1, a2,.., an) explains x and (a1, a2,..., an) explains y. (a1, a2,.., an) stands for possible instantiations of type A. The correctness of x and y is not relevant to the explanation.

All knowledge claims must be explained with reference to the same type of factors. Symmetry breaking is explained with reference to differences in *instantiation* of these factors. In the strong programme the type of factors that do the explanation are social factors. Instantiations of these explain all instances of theory acceptance and theory rejection. In the case of posthumanism agency is the determining factor. On occurrence level the interactions of agents again differ from case to case. The deciding type of factor has changed, but the mode of explanation has not, as both approaches make use of a hierarchy of factors. In both approaches one type of factor is dominant over the others.

In my proposal the leading idea is that the determining factors in science such as natural, social, psychological, rational, and personal factors, as well as cognitive resources and technological possibilities, require no predetermined hierarchy. Their relative efficacy is an empirical matter that must be accounted for by means of historical research. The other idea is that theory choice can be accounted for by looking at changes in the combinations of the factors. The new relationalist explanation of past science must look like (3):

(3) Type factors A, B,…, Z explain both x and y. Differences in acceptance are explained by different relations among instantiations a1-an, b1-bn, …, z1-zn of the factor types A-Z.

It is in fact not exactly symmetry that is defended in this new relationalist approach but heterogeneity, as the acceptance and rejection of scientific claims to knowledge do not have to be explained with reference to the same type of factors in all cases. Yet *before* research all type of factors could possibly have played a role in the determination of theory choice. Only detailed historical investigation can reveal which factors played a role in a particular instance of theory choice and what the dominance relations between these factors were.

The idea to explain changes in the historical process with reference to changing relations between relatively constant factors resembles an approach of the German historian, Nipperdey. Society according to Nipperdey consists of many processes that all have their own mode of development. It is through the intersection of these processes that a historical context can be carved out. [165] According to Nipperdey it was possible to establish relatively constant elements of human life but that different relations between these elements occurred from time to time. It is through these differences that an account of the special characteristics of historical periods can be given.

In Nipperdey's model the same type of elements may occur in historical contexts but the combinations between these elements are always different. This idea is not yet present in (1), as it was introduced in the science studies by SSK, with the distinction between a constant type of factors and differing instantiations of the type variable. I believe this is still a fruitful idea. It is maintained in (3) and will also serve the purpose of setting up a platform for historiography of science in chapter 7.

At least two other major approaches to past science have been formulated that are both based on the recognition of a number of constant elements. These approaches account for differences between historical contexts in terms of differences in the combination between the constant elements, like a chemist whose different molecules are built from the same set of elements of the periodic system. A clear example of such an approach is offered by John Pickstone.[166] Pickstone thinks we can identify a number of fairly constant 'ways of knowing' such as world reading, calculation, analysis and experimentation. Science in any period of time is always a compound of these ways of knowing. In one historical period one way of knowing can become dominant over the others. The period from roughly 1780 to 1850, for example, is interpreted by Pickstone as an 'age of analysis', as in a lot of fields of study scholars were busy

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[165] Nipperdey (1976).
[166] Pickstone (2000) is the key publication. More recent articles are Pickstone (2011a) and Pickstone (2011b).

with the collection of data and ordering these data according to particular systems of classification. This does not imply that other ways of knowing were not present in that period, but only that analysis was the dominant way of approaching things.

This model must be clearly distinguished from the more static style approach put forward by Crombie.[167] Crombie argued that several research styles alternated in the history of science. He distinguished between deductive, experimental, analytical-hypothetical, taxonomic, statistical, and evolutionary styles. So far this resembles Pickstone's model. Crombie however offers a choice *between* styles in distinct periods, whereas Pickstone argues that, although styles indeed alternate, the other styles continue to be present too. The alternation is a matter of dominance relations and not of wholesale replacement. Pickstone's model therefore is more dynamic as it allows for many possible combinations between the ways of knowing or styles of reasoning respectively.

Another 'chemical' approach to the study of science was recently defended by Hoyningen-Huene.[168] According to Hoyningen-Huene science differs from other human activities by being more systematic. The difference is however gradual and must be studied on nine distinct levels. These levels are loosely defined and it depends on the concrete field of research how the specific forms of systematicity have actually taken shape. Again, in this approach a number of constant elements are indicated on type level. Distinctions follow by investigating instantiations *and* combinations between these elements on occurrence level.

The symmetry breaking proposed in (3) is especially designed to account for theory choice. This is not the case in the 'Ways of Knowing' model, which can capture changes in science only on a very general level. In the example of the 'age of analysis' the different modes of analysis have to be made domain specific to function as criteria for the acceptance and rejection of theories. As it stands however, the five ways of knowing are too generally formulated to play this role in historical explanation. Moreover it appears that Pickstone's model has to be supplied with another mode of explanation that can account for the causes of the changes in the relations between the five ways of knowing.

In (3) above the factors A-Z are selected because they are the determining factors in theory choice. Compared to either SSK or posthumanism more analytical guidance is demanded at type level as the list of factors that are possibly relevant to theory choice is much longer and requires specification,

[167] Crombie (1994). See also Kwa (2011).
[168] Hoyningen-Huene (2013).

albeit in minimal terms. *A priori* guidance of historical research on type level must be present, but loose at the same time. Only at occurrence level, that is, in concrete historical circumstances, do the determining factors acquire their explicit meaning and efficacy. One advantageous prospect of the model is that the door is opened to rational factors again *without* the need to posit a line of demarcation. How exactly to interpret this set of factors, and how rational factors would facilitate evaluative historiography in this framework, are still desiderata that need to be met in the chapters to come. If we give up on a demarcation between internal and external realms of science, as I think we should do, the question is whether this pre-empts all space for evaluations of past science. The challenge is to show that to posit a context of justification does not depend on a demarcation between internal and external factors.

In this chapter I hope to have demonstrated that symmetrists have not 'gone crazy' but that instead symmetrical approaches to past science rest on fairly cogent arguments that require serious attention. Although I have uttered dissatisfaction with the main symmetrical approaches to past science, a number of their key ideas, such as the type-occurrence distinction of SSK or the relationalism of posthumanism, should, in my opinion, be embraced and find a place in an approach to past science that aims to move beyond them, in order to allow for qualitative assessments of past science.

Chapter 3 The Principle of Charity and Historical Interpretation

1. Modern historical awareness and the hermeneutic tradition

Until well in early modern times people thought about the present as being essentially the same as the past. The world was governed by a system from Creation to Christ to the Last Judgement and all human activity could be interpreted in relation to that system. Moreover in the texts of Antiquity a lot of wisdom could be found that was applicable to the present. Thus Cicero's adage that history was the master of life, *historia est magistra vitae*, long made a natural impression. When the present is essentially the same as the past, direct lessons can be drawn from it for life in the present.

The ties between past, present and future gradually started to weaken due to a host of factors such as the Reformation, the European discovery of the Americas, the confrontation with other religions and cultures, the study of chronology, the start of modern philological text criticism and the Scientific Revolution. Slowly the trio Creation-Christ-Judgement was replaced by the trio Antiquity-Middle Ages-Modernity. Yet it took until the middle of the 18th century before a modern historical consciousness started to emerge. Modern historical consciousness starts with the experience of difference. In the modern conception, history is seen as an open-ended process in which change is the central notion.

The different conception of history led to a radical change in approach to the study of the past. As periods differed from the present, one of the tasks of historiography became to study the past on its own terms. Further the question became how the present came to be. What was the greater scheme behind historical developments and how could such a scheme be wedded to the study of particular aspects of distinct historical contexts? To take lessons from the study of the past continued to be important but it was no longer primary. The primary issue became how to gain understanding of the differences between past and present. If we conceive of the past as a foreign country how can we come to grasp the meaning of norms, values, rituals, customs, intentions, and other aspects of the mental and physical life world, which are alien to us?[169]

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[169] Cf. Hartley (1953): 'The past is a foreign country, they do things differently there.'

As soon as it was realized that the past does not speak directly to us, both philosophers and historians started to think about how to gain access to the world of the past and how to understand it on its own terms. This started a hermeneutic tradition in the study of history. The term hermeneutics is derived from Hermes, the messenger God. As the main source material of historians consists of written texts, hermeneutics offers ways to capture the message of the text, its meaning, the idea or intention of the author, the reception of the text by readers, the relation of the text to other aspects of the historical context, etc. From this the behaviour of historical actors can be interpreted, as purpose and meaning of behaviour can be inferred and relations to behaviour of others can be drawn. Schleiermacher was the first to propose a theory of hermeneutics, whereas Ranke and Droysen turned it into an approach to history known as historicism. Later important contributions to the hermeneutic tradition were delivered by Dilthey (*Verstehen* approach), the Neo-Kantians, Gadamer and Skinner. Postmodernist thinkers such as White, Derrida, Lyotard and Rorty can also be seen as part of the tradition, but one has to be careful here as in the 19th century historicism was supposed to produce objective historical knowledge, which is something postmodernists would deny to be possible.

From Dilthey onwards we get a sharp division in philosophy between continental philosophy and analytic philosophy. The former is focused on experience, following the stress in the hermeneutic tradition on 'nachfühlen', 'einfühlen', 'nacherleben', 'ahnen', etc. Husserl for example took psychology under the umbrella of phenomenology. In the analytic tradition psychology is seen as an experimental scientific discipline, a field separate from philosophy. The analytic tradition does not focus on direct experience but following the linguistic turn aims to present an objective and exact analysis of language. As hypotheses have to be formulated in sentences, the aim is also to provide an analysis of scientific knowledge by logical and linguistic means.[170] Modern analytic philosophy of science is born out of this tradition and this also holds for the principle of charity. The principle of charity is a methodological principle with which gaps in understanding others can be bridged. In what follows we will thus approach this question from the perspective of analytic philosophy, yet the

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170 The link between Dilthey and Carnap is a topic of current research. See Damböck (2012).

issue is firmly connected to the rise of modern historical consciousness and the hermeneutic tradition that followed in its wake.[171]

2. Introduction to the principle of charity

The principle of charity received widespread attention due to Quine (1960). Quine's use of the principle stems from two sources of inspiration: Carnap's principle of tolerance and Wilson's original use of a principle of charity. Carnap's principle of tolerance was born when he left the idea that all scientific hypotheses could be reduced to one observation language. Carnap (1937) argued that it is possible to describe reality in more than one language. The selection of the most apt language is guided by practical demands. Carnap equated the structure, or syntax, of the language with logic. He rejected Frege's logical monotheism. As long as syntax was recognizable, and as long as the logic was consistent, different forms of reasoning and expression had to be accepted. Carnap famously asserted that 'in logic there are no morals' and hence a principle of tolerance had to secure a tolerant attitude towards a diversity of logical systems.[172]

Wilson introduced the principle of charity in order to solve a problem in determining reference of proper names.[173] According to Wilson, a full description of all properties does not suffice to pick out referents. He gives the example of Caesar of whom we know that he crossed the Rubicon, invaded Italy and went on to take power in Rome. Wilson now asks us to imagine that Caesar took a different decision and decided not to cross the Rubicon and stay in Gaul. Would *this* Caesar still be the same person or not? According to Wilson our intuition is that he would, but if reference of proper names is dependent on descriptions of properties only, we are drawn to the conclusion that the Caesar who crosses the Rubicon is a different person from the Caesar who decided not to.

Properties are of course important in determining referents but what it is to be an individual cannot be wholly dependent on descriptive properties. We

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[171] Considerations in analytic philosophy are not often brought to bear on the study of history. Glock (2008) is an exception. He argues that more contact between these fields can be beneficial to both.

[172] For an extensive elaboration on Carnap (1937) see Ricketts (1994). Note that Carnap is not a relativist: whether a language accurately represents the world is still settled by an appeal to the verification principle.

[173] Wilson (1958).

need something extra to close the gap between identifiable properties and the determination of individual referents. For this Wilson proposed a principle of charity. If we have a set of descriptive properties and a set of individuals we may charitably assume that the individual who is the most likely match with the set of properties is the person referred to in using a name such as 'Caesar'. The set of descriptive properties should be used as a basis for comparison. If something changes in the set of descriptive properties the individual best fitting the properties might not have to change accordingly. We have nothing else to go on than a set of properties and yet reference is not completely dependent on those properties. Charity is thus needed in determining fitting relations and hence in fixing reference.

In Quine (1960) charity is also used in terms of a comparative fitting relation but there are crucial differences with Wilson. First Quine extends the application of the principle to the level of translation and second he lets the principle play a crucial role in the problem of *radical* translation. He asks us to imagine that we are confronted with an unknown tribe at a location where this tribe lives. We see a white rabbit jumping by and people of the tribe say "gavagai!". We cannot ask the members of the tribe what this utterance means because we do not know anything about their language. Many translations of 'gavagai' into English appear to be possible, among these for example 'something jumping', 'something white', 'a deceased forefather in the guise of a rabbit appears', and 'a rabbit'. We are thus facing a problem of indeterminacy of translation, which is another version of the problem of underdetermination.

Quine now argues that in simple cases we are allowed to choose the most straightforward agreement between our categories and the ones of the language of the people we are translating from. We thus charitably assume others speak the truth, hence with 'gavagai' they mean 'rabbit'. The principle of charity allows us to choose the most justified candidate out of a set of possible translations depending on a match with the categories of our own conceptual scheme. According to Quine this is the only way a procedure of radical translation can get started. It may later turn out that meanings differ but these differences can be established only on the basis of other forms of agreement, etc. Charity is thus a pre-condition for all successful translation.

In the work of Davidson we find a further elaboration of the principle of charity to the level of interpretation of both linguistic and non-linguistic behaviour.[174] This is the most relevant use of the principle for current purposes

[174] I am referring to Davidson (1973). A collection of this and other articles can be found in Davidson (2001).

because Davidson's considerations on the interpretation of others can be extended to the interpretation of the deeds of past actors too, following the idea of 'the past as a foreign country.'

Davidson observed that a sharp distinction between empirical content and conceptual scheme underlies the work of Carnap and Quine. Both are tolerant with respect to the number of conceptual schemes that can be set up. Yet in Carnap the verification principle works equally across languages and Quine's use of the principle of charity makes basic truths invariant across languages. The same can also be said about Wilson's set of referents. According to Davidson, independent access to sets of basic 'facts' is not available to us. This means that the principles of tolerance and charity of Carnap and Quine cannot do the work they are supposed to do.

The use of charity becomes more fundamental as a consequence. Davidson argues that in order to fix interpretations of linguistic behaviour we need to create a theory of meaning *and* a theory of belief of the speaker. Part of the theory of belief consists of a set of cognitive attitudes (intentions). As interpreters we also possess such theories. Ultimately the goal of interpretation is grasping the meaning of behaviour. Hence the theory of meaning, i.e. the attribution of semantic content to sentences, is the dependent variable. In a situation of radical interpretation we can therefore start to make sense of the other only by assuming agreement between our theory of belief and that of the speaker. This agreement is given by the principle of charity. It is not that with charity we can by comparison select the most appropriate theory of meaning, as these theories are not available yet and have to be built up in the process of interpretation. The use of charity in Davidson is more fundamental, since the principle is constitutive for setting up *any* theory of meaning.

With Davidson's use of the principle, agreement with others is no longer sought on the level of 'obvious' truths as with Quine. Application of charity does not mean: those we are interpreting have to have the same beliefs, opinions, etc., as we do. Instead we must assume agreement with those we are interpreting on the level of cognitive attitudes, such as attitudes towards truth, striving to implement some form of logical reasoning, communication of what is true and relevant to others, etc. In short, the application of charity involves interpreting others as rational beings:

"The object of interpretation is the explanation of actions, linguistic and nonlinguistic. Rationally to explain an action requires that it be so described as to seem reasonable. An action will seem reasonable only if it issues from desires and beliefs that are themselves reasonable. For these attitudes to seem reasonable they must accord with what you, the interpreter, take to be reasonable. Thus it is that rationalizing explanation requires charity, i.e., the ascription of attitudes the interpreter himself has."[175]

This also involves seeking agreement on the level of belief because too much difference between belief systems would blur the focus of interpretation.

We create agreement with others by considerations of simplicity, hunches about the effects of social conditioning, knowledge about explicable error and common sense.[176] An operation of 'fine tuning' towards specific context is thus required. As far as possible we have to take into account background knowledge of the speaker and possibly information of other linguistic and non-linguistic behaviour. A logical principle we think is valid, but that does not fit in with the network of beliefs of others, does not have to be expected, and we should avoid interpreting this absence as irrational. This does not pre-empt all verdicts of irrationality, as we may for example expect maximization of logical reasoning in relation to the whole web of beliefs of a given speaker.

The procedure of interpretation we get has a circular element. The interpretation starts with assumptions of agreement on beliefs and propositional attitudes; in short a theory of belief is set up. This creates the grounds for the interpretation of the meaning of behaviour. With this, a theory of meaning can be constructed. If needed, the theory of belief can be re-interpreted if the theory of meaning comes into conflict with it. Such adjustments take place in a step-by-step process until we arrive at both a theory of meaning and a theory of belief of the speaker. The process might also lead to adjustments of our own theories of meaning and belief. Note that there are no external standards to judge whether the agreement is really present or whether any of the theories corresponds to state of affairs in the world. To the first issue the reply is that as long as there is no information to the contrary we can continue to work on supposed agreements. To the second issue the reply is that the issue has been formulated incorrectly. This becomes understandable when we concentrate on Davidson's rejection of the third dogma of empiricism.

Following Quine, Davidson agreed with the rejection of the two 'dogmas' of empiricism: the analytic-synthetic distinction and the reduction of complex

[175] Davidson (1975) p.21.
[176] Davidson (1973) p.18.

sentences to simple sentences.[177] Giving up the first two dogmas meant giving up the idea that we can allocate empirical content uniquely sentence by sentence. This however left the idea of empirical content intact. All sentences may have empirical content and this content is explained by reference to the world. This dualism of conceptual schemes and the empirical content of sentences is the third dogma of empiricism and it has to be rejected as well according to Davidson.[178]

His argument for the rejection of this third dogma was directed against conceptual relativism. Kuhn and Feyerabend were Davidson's main targets, but the argument applies equally to Bloor's perspectivism.[179] These relativist theories are, according to Davidson, built on the duality of empirical content and conceptual scheme. The relativist view is that the *same* world is confronted by different conceptual schemes, yet the truth of sentences can be established only relative to the schemes. This is the combination between ontological realism and epistemological relativism we saw in the previous chapter. This combination produces the problem of incommensurability, as it is not clear on what grounds to compare different claims to knowledge made in the respective conceptual schemes. When accepting conceptual relativism, change of reference of scientific terms cannot be meaningfully judged, as translation between conceptual schemes fails.

A translation procedure based on charity in the manner of Quine offers no way out of the problem as the truth of simple observation statements cannot be gained independently of the rest of the theory of belief. Davidson agrees with the relativists that neither a fixed stock of meaning nor a theory-neutral reality can provide a ground for comparison of conceptual schemes.[180] Yet the mistake of the relativists is to assume a fixed world but to 'disconnect' this world from the conceptual schemes that can be projected on it. Davidson demonstrates the failure of this relativism by arguing that the relativist cannot make sense of untranslatability. Either there is total failure of translation, but this would simply be unrecognizable, or there is partial failure of translation but this involves a retreat from the extreme relativist position. Davidson's central

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[177] Quine (1980).

[178] Note that a conceptual scheme is always expressed in a language but more than one language can be the expression of the same conceptual scheme.

[179] Relations between Carnap and Kuhn have also been drawn: Earman (1993) and Irzik (1995). In Kuukkanen (2008) Kuhn is depicted as a neo-Kantian conceptual anti-realist, i.e. he does not accept convergence to reality, only increase in coherence in conceptual schemes. This is similar to Bloor's perspectivism.

[180] Davidson (1973) p.17.

argument is that partial failure of translation can be made sense of only against a vast background of agreement. This leads to a general insight of the charity discussion: differences can be made sense of only on the basis of similarities and in this sense agreement comes prior to disagreement. Something totally strange, absurd or bizarre can simply not be made sense of. Yet the experience of difference is crucial since if everything were the same nothing would be worthy of investigation.

Considering the problem of incommensurability, we must accept three things according to Davidson: (1) a holistic theory of meaning and belief, (2) the direct involvement of the world in the emergence of our concepts and vice versa, and (3) an interpretation procedure of other conceptual schemes that rests on charity, if applied on the right level, and with acceptance of the consequences of a degree of circularity and uncertainty.[181] For Davidson (2) does not involve relinquishing the notion of objective truth. On the contrary, he says it may help to regain unmediated touch with the way familiar objects manifest themselves and make our sentences true or untrue.[182]

Applied to the history of science, a Davidsonian principle of charity orders us to seek agreement with past historical actors on the level of rationality. Especially in the case of radical interpretation, i.e. in case we have nothing else to go on, the principle of charity is needed to fix interpretations of past behaviour. Yet also in case of conflicting interpretations, the principle of charity advises us to choose the most rationalizing interpretation, maximizing agreement between past and present. In this sense the comparative selection procedure is again the central aspect of charitable interpretation.

Such a principle of interpretation is needed because other options are not available to us. In case of conflicting interpretations, or in case of lack of interpretation altogether, we cannot make use of a fixed stock of meaning items or appeal to a theory-neutral reality. Without a principle of charity we would have to accept untranslatability between conceptual schemes and hence accept

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[181] It is interesting that Daniel Dennett has expressed similar viewpoints. Accepting the intentional stance is making normative claims because conditions flow from this stance on how people ought to behave. Further Dennett argues that semantic content cannot be explained purely in micro terms, i.e. be reduced to individual referents, nor does a pure macro explanation in terms of social functions suffice. Semantic content is not static but dynamic: it is relational, Dennett (2010). For Dennett the fact that we can make use of this relational information is because this ability is the result of natural (and cultural) selection. See also chapter 6.

[182] Davidson (1973) p. 20. For Davidson truth is a necessary condition for interpretation. Yet, according to the interpretation offered in Van den Akker (2008), he endorses a pragmatic theory of truth. I follow this interpretation here.

the consequence of incommensurability, namely conceptual relativism. This would then fundamentally undermine evaluative historiography. Still the question whether there aren't any other principles of interpretation that can do a similar job needs to be addressed. Two alternative candidates appear to be available: the principle of symmetry and the principle of humanity. I will end this section with a comparison between symmetrical interpretation, discussed in detail in the previous chapter, and charitable interpretation. Section 3 is devoted to a comparison between the 'wider' principle of humanity and the principle of charity.

Because charity orders us to seek agreement between past and present on a particular cognitive good, there is an assumption behind this that the cognitive good is similar in present and past times. Hence isn't this just another way of positing a symmetry? Yes, this is indeed the case, but any approach to past science has to work with symmetries and asymmetries. Positing a symmetry does therefore not automatically involve commitment to an elaborate principle of historical interpretation. It should be realized that insisting on charity is incompatible with both the principles of symmetry of SSK and posthumanism. The principles of symmetry underlying these programmes select determining factors that govern past science. Cognitive factors are deliberately excluded from the set of determining factors while charity, at least in the Davidsonian variant, operates *precisely* on these factors.

Further, interpretive principles are designed to solve problems of underdetermination, but they are applied on different levels. Symmetrical analysis is used because, so the argument runs, no other means to explain closures of scientific controversies are available. The principle of symmetry offers a perspective on the past involving a specification of determining factors. The principle of charity does not involve an elaborate perspective on the determining factors in science. It does force conditions on interpretation, but on a different level. Charity should be seen as a tool to handle specific problems of historical interpretation, i.e. to what extent to offer a rationalizing interpretation of past deeds and beliefs. Charity thus works as a selection mechanism between possible historical interpretations. Contrary to symmetry, charity is also evaluative in nature.

In spite of all these differences the new relationalism proposed in the previous chapter may offer possibilities to draw the two principles of interpretation closer together. The two appear to connect when cognitive factors are included in the total matrix of determining factors. In this way charitable interpretation can be subsumed under a general symmetrical approach to past science. Another way to look at the relation is to say that charity is a *precondition* to make the symmetry of new relationalism work because it tells us how to make use of rational factors in historical interpretation, namely via a comparative selection procedure between possible interpretations. The previous chapter has made clear that staying too close to historical context in the interpretation of the past backfires. The principle of charity however assumes *a priori* guidance to historical interpretation. The crucial issue therefore is to specify how much can be stated beforehand, or 'outside history', and how much can be left to historical context.

3. Charity or humanity?

Apart from the symmetry principle there is another principle of interpretation available, namely the principle of humanity. Where the symmetry principle was introduced for different purposes, the principle of humanity was designed to do exactly the same work as the principle of charity. Most philosophers insisting on a principle of humanity also accept a holistic theory of meaning and equally draw the conclusion that some interpretive principle is needed to make contact with distinct conceptual schemes, as no other means are available to do this work.[183] Yet they question the preference for rationalizing interpretations over irrationalizing interpretations that follows from the principle of charity. Why interpret a speaker's statements as rational and, in the case of any argument, consider its best possible interpretation? Why avoid attributing irrationality, errors, etc., when a coherent, rational interpretation of the statements is available? They argue that no good reason for this preference can be given.

Instead it is better to assume that others, including past actors, stand in the same position, depending on the concrete situation, as we do, with respect to going right and going wrong, to acting rationally or irrationally. The idea therefore is to subsume charitable interpretation under a wider principle of humanity that has equal expectations of correct and incorrect or rational and irrational behaviour. From this stance it would be possible to see when and where others went wrong, were irrational, etc., whereas charitable interpretation would just pardon everything and lead to serious

[183] Grandy (1973) uses the term 'Principle of Humanity'. McGinn (1977) followed this usage. Similar principles are a 'Principle of Universal Sympathy', Gellner (1973) and a 'Principle of Explicability', Henderson (1993).

misinterpretations.[184] Grandy for example wrote: “it is better to attribute to the speaker an explicable falsehood than a mysterious truth”.[185]

The attack against charity is often misdirected. On the Davidsonian interpretation agreement with those we are interpreting should be sought, not on the level of truth content of sentences, but on the level of cognitive goods such as intelligibility, rationality, etc. That is, it can be rational to defend a theory that only later turned out to be wrong.[186] Further, it is not the case that all principles of charity are insensitive to error, as is often suggested. Yet in spite of these confusions, the principle of humanity does have an intuitive appeal. On deeper inspection there are however serious difficulties with the implementation of the principle.

Grandy argued that in interpreting others we need to place ourselves fully in their position, that is, we must assume their total belief-desire system. We are then equally liable to go astray as the past actors were. The problem is from what angle to perceive the errors? The first option is to make use of standards of evaluation operative in the context of the ones we are interpreting, which for our purposes is the historical context. But this is problematic because when we have set ourselves fully in the position of the other, what is wrong or what is irrational cannot be seen anymore as we assume that in general people do not persist deliberately in being erroneous or irrational.

A second option is to connect to what we now think is correct and rational (our best theories of the world). Then we can attribute irrationalities and false belief to past actors. The danger of this procedure is that it quickly leads to a sociology of error, i.e. everything that falls out of present-day conceptions, categories and standards must be explained via causal analysis.[187] We have seen in the previous chapter that this is problematic because it leads to classifications of deviant behaviour from our point of view as irrational where this is sometimes not justified. The principle of humanity appears to force such judgements, whereas a well-formulated principle of charity can avoid them.

The final option for the ‘humanist’ would be to work with a less normative rationality concept and allow for rationalizing interpretations of past science

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184 Cf. the French expression ‘tout comprendre c’est tout pardonner’ which appears to stem from Buddha: to ‘understand all is to forgive all’.

185 Grandy (1973) p.445.

186 Thus Grandy (1973) was only directed against Quine. However in McGinn (1977) a critique against Davidson was formulated based on some of the points Grandy made earlier Illustration of this point with examples from historiography of science follows in the next section.

187 For an illustration of this effect, see also next section.

even if these deviate from current standards. Such a move would bring the two interpretation principles very close together. Dennett for example has argued that the difference between a charity principle and a humanity principle (which he calls the projection principle) is only a difference in emphasis, as they both insist on a special role of rationality in the interpretation of others.[188] The crucial point then is to make explicit with what rationality concept we are working, since this determines what kind of agreement with others we are seeking and how to be judgemental when differences occur. The concept should be historically sensitive in such a way that is does not lead to a sociology of error, as there are charity-based interpretations of past science which do.[189] The formulation of such a rationality concept will be a task undertaken in chapter 7.

There is another problem with the principle of humanity. Recall that the principle of charity was designed to solve specific problems of interpretation in situations where no clue for interpretation is present or a choice between conflicting interpretations has to be made which cannot be settled by others means. With charity one is advised to choose the most rational interpretation possible (depending of course on the rationality concept). With humanity it is still unclear what to do since the choice for interpretation is entirely left open, as we are forced to equally expect rational and irrational behaviour. The initial goal of using a principle of interpretation to solve the problem of underdetermination on this level cannot be met with the principle of humanity.

Grandy suggested solving this problem by combining the application of the principle of humanity with a theory of evidence, but this cannot work. First, the causal connection between evidence and the utterances we have to interpret, on which Grandy insists, runs into difficulties.[190] Second, the whole point of using interpretive principles is that evidence is not straightforwardly or independently given. Setting evidence apart from the rest of interpretation denies the assumption of holism. I am in agreement here with Malpas: “there is no

188 Dennett (1987).

189 An important dissertation in this respect especially attuned to historiography of science is Elffers (1991). She argues that only charitable interpretation based on a historically adequate rationality concept works for historiography of science. The strong point of it is that it can reveal factors that sometimes determined behaviour of past actors, which were not consciously known to them. This might require to know past actors better than they knew themselves: “every interpreter understands the author better than he understood himself. Every interpretation goes too far.” Guépin, as cited in Elffers (1991) p.111. Note that such an analysis is impossible with a strategy of just following the actors.

190 Lemos (2007) p. 41 shows that Gettier problems remain, precisely the type of problem Grandy wants to get rid of.

independent evidence on which interpretation can be based - the evidence of interpretation is itself the product of interpretation."[191]

McGinn has argued that if holism is accepted then charity follows because there is no other way to break into distinct conceptual schemes involving an intertwinement of meaning, empirical content, belief and cognitive attitudes. He goes on to reject the premise and then follows Grandy in opting for a principle of humanity. In my view, holism needs to be accepted. The challenge is to connect distinct webs of beliefs to our web of beliefs, leading to a greater understanding of both. This challenge can be met only by drawing the right relations between past and present. I believe that a sophisticated evaluative historiography yields useful tools to accomplish that. However, contrary to McGinn, I do not think that on the acceptance of holism there is only the option of applying a principle of charity left to avoid conceptual relativism. This combination is defended by Davidson (1973), Malpas (1988), Elffers (1991) and Fitzgerald (2008). However defences of a combination between holism and a principle of humanity can be found as well, for example in Gellner (1973) and Henderson (1993). If the discussion charity vs. humanity were to be solved via a choice for holism, it would not be such a pressing issue.

The discussion above has highlighted a number of difficulties with the principle of humanity. A final consideration is needed to strengthen the case for charity-based interpretation. It is often not well understood that charitable interpretation is only a first approximation. It is the start of a cyclical interpretation process, which can lead to changes in the initial interpretation. Thus charitable interpretation procedures are not insensitive to error and irrationality. The best way to view the procedure is as a dialogue between two conceptual schemes. One has to start the dialogue in seeking agreement with things in one's own network, otherwise no grounds for interaction can be established. Yet, on the basis of later information, it may turn out that earlier assumed forms of agreement were incorrect and hence the connections between the two networks require updating. In the Davidsonian framework we start with assumptions of the other's theory of belief. This theory may come into conflict with the theory of meaning. If so, then we need to make an adjustment somewhere. This goes back and forth and eventually the process ends in a balance between two networks, which can always change again when new information makes this necessary. Note that in the interpretation of, for example a particular utterance by a speaker, theories of belief and meaning need

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[191] Malpas (1988) p.22.

to be combined with *both* knowledge about other behaviour of the speaker and knowledge about the context in which the behaviour took place.

There is another reason why charity-based interpretation is not insensitive to error, given by Davidson: "Charity prompts the interpreter to maximize the intelligibility of the speaker, not the sameness of belief. This entails that interpretation must take into account probable errors due to bad positioning, deficient sensory apparatus, and differences in background knowledge."[192] Thus there is room for explicable error from the very start of the interpretation process. Fitzgerald therefore argues that Davidson's version of the charity principle is in fact not so much different from the principle of humanity.

This analysis can be supported by once more quoting Davidson:

> "The Principle of Coherence prompts the interpreter to discover a degree of logical consistency in the thought of the speaker; the Principle of Correspondence prompts the interpreter to take the speaker to be responding to the same features of the world that he (the interpreter) would be responding to under similar circumstances. Both principles can be (and have been) called Principles of Charity: one principle endows the speaker with a modicum of logic, the other endows him with a degree of what the interpreter takes to be true belief about the world."[193]

Davidson thus commits himself to optimizing the intelligibility of a speaker against a background theory of their beliefs: this is similar to the demand of the principle of humanity to put ourselves in the shoes of those we are interpreting.

Principles of interpretation are needed to close a gap between what is obvious and what is not obvious. We can observe verbal and non-verbal behaviour, but we cannot directly observe the meaning or the mental processes underlying this behaviour. In order to gain access to these we need interpretive principles. On the assumption of holism two main approaches, charity-based and humanity-based, purport to do the job. In the analysis above these approaches have been brought close together, yet a choice for a charity-based interpretation has been preferred. Charitable interpretation is much less rigid than is often supposed and can deal with errors and disagreement. The advantage of making use of charity first is that it offers a clear choice of interpretation when no other clues are present. Further the principle of charity offers better grounds to bring about a real connection between our and past conceptual schemes. The choice for charity-based interpretation over humanity-based interpretation does however not end the discussion on the principle of charity. Charity can be applied in

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[192] Davidson, as quoted in Fitzgerald (2008) p.19.
[193] Davidson, as quoted in Fitzgerald (2008) p.20.

different ways and we need to spell out what the most useful way for the study of past science is. This discussion is best continued by looking at concrete examples of charitable interpretation from science studies.

4. Examples of charitable interpretation in science studies

The principle of charity applied to the history of science requires interpreting words and deeds of past actors as rational. In the case of an argument over interpretation, which cannot be settled by other means, the strongest, most intelligible, most rational, etc. interpretation must be favoured. Even when accepting this, diverging interpretations of the principle are possible. Thagard and Nisbett (1983) distinguish five of these:

1. Do not assume *a priori* that people are irrational.
2. Do not give any special prior favour to the interpretation that people are irrational.
3. Do not judge people to be irrational unless you have an empirically justified account of what they are doing when they violate normative standards.
4. Interpret people as irrational only given overwhelming evidence.
5. Never interpret people as irrational.

(1) is the most judgemental application of the principle, as initial withholding of judgements of irrationality can quickly be overturned. (5) on the other hand is the most liberal usage: rationalizing interpretations will never be revised. Usage of charity in history of science has often found itself in one of these extremes. With (1), charitable interpretation can lead to two things. Either past attitudes and beliefs are translated in such a manner that it looks as if past actors were thinking as we do, only using other terminology. An example of this will be discussed below. Or past attitudes and beliefs that deviate from present-day attitudes and beliefs must be deemed irrational. This usage can be connected to Whiggish approaches to past science, for an example see also below. Both these ways of applying charity are sometimes critically described as 'imperialist'. As Hacking forcefully put it applying charity in this way may not lead to the respect people are waiting for:

" 'Charity' and 'humanity' have long been in the missionary vanguard of globalizing commerce. Our 'native' may be wondering whether philosophical B52s and strategic hamlets are in the offing if he wont sit up and speak like the English.... If the native does not share most of our beliefs and wants, he is not engaged in human discourse, and is at best sub-human (the native has heard that one before too.)"[194]

An example is Priestley's use of the term 'phlogiston' discussed in both Hesse (1976) and McAllister (1986). They note that Priestley's terms such as 'phlogiston' or 'dephlogisticated air' are often translated into modern terms like oxygen or carbon dioxide with an appeal to the charitable assumption that Priestley was speaking the truth. McAllister rightly protests against this since Priestley could well have been false: "the major weakness of this approach lies … in the assumption of the principle of charity and that hence the greatest possible truth-content should *invariably* be read into past texts. Why, when we know how easy it is to assert a gravely incorrect theory?"[195]

Hesse further argues that if charity is applied in this way, theory change cannot be properly studied. When everything said in the past can be translated into the terms of present-day theories, in a sense everyone was always right, so it becomes seriously problematic to speak about improvement in science. Both authors argue that applying charity in the sense of optimizing truth of past concepts in translation into modern terminology is not a good procedure and leads to serious misdescriptions of past science. It is for example not clear at all that Priestley wanted to refer to oxygen even though the phenomena he tried to explain are very similar to what modern theories purport to explain.[196]

These are all just observations but they only question Quine's use of the principle and not Davidson's: assuming charity on the level of rationality, i.e. assuming that others, like us, strive for overall coherence of their beliefs, upon discovery of a contradiction in a body of beliefs aim to remove this, in general attempt to speak the truth, etc. Both Hesse and McAllister appear to leave the door open to such forms of charitable interpretation. Hesse writes that: "we want to preserve Priestley's rationality, not necessarily the truth value of his theory."[197] And elsewhere she states that we assume that: "the alien system

[194] Hacking (1975).
[195] McAllister (1986) p.325 italics in original.
[196] Hesse rightly argues that this comparability offers a ground for historians to use present-day explanations of these phenomena as "such knowledge increases understanding of Priestley and his system." Hesse (1976) p. 277. See also chapter 7 in connection to this argument.
[197] Hesse (1982) p.708.

intended truth on its best evidence."[198] Therefore she argues there is no need to seek further principles of preservation of scientific rationality.[199] Indeed the principle of charity has already been applied!

McAllister makes the identification of the truth-content of individual terms and sentences dependent on an assessment of rationality: "Denotations and truth-values are seen to be established on the basis of evaluations of rationality rather than on an arbitrary methodological principle."[200] For him the principle of charity therefore has 'suspect legitimacy'.[201] Following the 'principle of no privilege' of Hesse, which says that our own theories are as much subject to change as theories in the past, McAllister uses our concept of rationality, not as a yardstick, but only as a zero point or rational null indicator. Other forms of rationality can be gauged with respect to this null indicator. Now it is argued that if we do this we do not need charity about truth: assessments of truth follow from assessments of rationality. It has been agreed upon above that applying charity on the truth content of sentences does not work. Yet the procedure that is further sketched by McAllister is hard to distinguish from a Davidsonian application of charity.

The other 'imperialist' form of charitable interpretation amounts to Whig history. According to Wachbroit Whig history is the invariable outcome of historical interpretation guided by charity:

> "History of science informed by a charity-based theory of rationality results in a brand of Whig history. Judgements concerning the rationality of past scientists are made from the present (i.e., our) point of view. For example, whether the Medicean astronomers were rational in their refusal to believe Galileo's observations based on the telescope-i.e., whether their refusal is a piece of internal or external history of science – is determined by what we would find rational to believe were we in their epistemic position. The principle of charity, and with that any charity-based theory of rationality, rests on certain facts about us, the most important being that it makes sense to talk of our standards of rationality and of what we would find rational."[202]

Wachbroit here correctly highlights a mistaken application of charity as imposing a presentist norm on all past science and an evaluative attitude towards things diverging from the norm. Yet he is incorrect to assert that all charity-based interpretations lead to Whig history. There is indeed a presentist

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[198] Hesse (1976) p.269.
[199] Hesse (1982) p.708.
[200] McAllister (1986) p.326.
[201] Ibidem 324.
[202] Wachbroit (1987) pp.35-47.

(or theory-laden) aspect involved in charitable interpretation, but it is incorrect to conflate any form of presentism with Whiggism. The goal of applying charity is not to explain the past backwards. Instead the principle is a pre-condition to make any historical interpretation possible: it is needed in order to make sense of differences and offers instructions when comparing between conflicting interpretations, when no other guiding lines can force a choice. Moreover the charitable interpretation procedure defended in this chapter starts with supposing agreements, but does not have to end with them. The initial agreement is only a first approximation. For all these reasons it is not the case that all charitable interpretation leads to Whig history.

With these 'imperialist' examples of charitable interpretation out of the way, we can now consider examples of relativist usages of charity. An interesting case is offered by the 'modern classic' in historiography of science *Leviathan and the Air-pump* (1985), groundbreaking at the time of publication and now an exemplary case of contextualist historiography of science. In the introduction to this work the authors, Shapin and Schaffer, wrote: "We shall be adopting something close to a member's account of Hobbes's anti-experimentalism. That is to say we want to put ourselves into a position where objections to the anti-experimental programme seem plausible, sensible and rational. Following Gellner we shall adopt a charitable interpretation of Hobbes's point of view."[203] They argued that it was not bizarre in the 17th century to oppose experimental investigation of nature, as it would nowadays be. Charity for them is a useful tool to take away the aura of self-evidence of modern science:

> "We want to show that there was nothing self-evident or inevitable about the series of historical judgements in that context which yielded a natural philosophical consensus in favour of the experimental programme. Given other circumstances bearing upon that philosophical community, Hobbes's views might well have found a different reception. They were not widely credited or believed - but they were believable; they were not counted to be correct - but there was nothing inherent in them that prevented a different evaluation."[204]

The debate between Hobbes and Boyle revolved around the issue whether a vacuum could exist or not. At stake was the reliability of the methods of research and proof with which to demonstrate this. The approach to scientific questions needed to be clear, open for control, and yield certainty. Hobbes argued that only rationalism and mathematics provided this openness, since

[203] Shapin and Schaffer (1985) p.13.
[204] Ibidem.

logical deductions and calculations were clear and certain. Boyle on the other hand argued that the experimental programme provided the sought foundation of knowledge because everyone could perform experiments, results could be shared and no philosophical doctrine had to be assumed with which to value the results of the experiments.

Shapin and Schaffer now aim to prove that Hobbes' arguments against Boyle were sensible and rational. The arguments included the following. Experiments were not very well repeatable, Boyle's air-pump for example frequently leaked, hence produced different results and thus was not reliable. The results were also hard to check because only a small group of initiated people was allowed to witness the experiments. Obtained experimental result still required an explanation and hence a theory. The ruling metaphysical doctrine of the time, Descartes' corpuscular philosophy, offered satisfactory explanations of physical phenomena. Hobbes was a Cartesian and so he could reject a vacuum on philosophical grounds alone. Since all space had to be filled with matter, the explanatory results, dying animals and shrinking material, had to be explained by the working of as yet unknown particles. Assuming otherwise would smell of the occult: surely there couldn't places in the universe filled with nothing, i.e. places that fell beyond God's control?[205]

The search for a secure foundation of knowledge was pressing in the 17th century. Europe witnessed the Thirty Year's War and religious conflicts were fought in many countries. England had faced the overthrow of the monarchy and had been ruled for almost a decade by Lord Protector Cromwell. The Boyle-Hobbes debate was held during the Restoration Settlement in which solutions were sought to end all possible political and religious quarrels. The solution to the foundation of knowledge was one of them. According to Shapin and Schaffer the choice was between Hobbes' rationalism and Boyle's experimentalism. King Charles II trusted the Royal Society to decide the issue. Within this group of exclusive members, experimentalism was favoured. Boyle was an important figure in the Royal Society, while Hobbes was not even a member! The authors argue that the conflict was settled following these social factors, and therefore experimentalism won. The closure of the conflict

[205] It is interesting that on current interpretation it may not be proper to speak of vacui after all. This is due to the so-called Casimir effect, formulated in 1948 and empirically confirmed in 1997, which says that electromagnetic forces are present through virtual particles, in otherwise empty spaces. Needles to say Hobbes did not envisage this when he argued against the existence of vacuous space, but it shows how careful historians must be in the use of modern knowledge when interpreting past science.

however had nothing to do with the rationality of Hobbes' viewpoints and arguments.

The respectful treatment of past actors, i.e. interpreting beliefs and deeds of everyone as rational, has been attacked as an over-application of charity based on the use of a far too wide, in fact hardly defined, rationality concept.[206] It may be a good methodological principle as first approximation to treat all contestants in scientific dispute as rational. The problem is that after the first approximation no re-interpretation takes place, as if no aspect of the intellectual behaviour of both Hobbes and Boyle was ever irrational or mistaken. It could be argued that some aspects of Hobbes were irrational, for example because he was not open-minded enough towards the new experimental programme. Or it could be argued that Hobbes' position was indeed rational, but on the whole scored less well than Boyle's position, and was therefore not preferred. Or it could be argued that different levels of assessments should be kept in mind. Hobbes' behaviour to oppose the experimental programme can then be described as rational and his arguments can be found sensible but nonetheless he can be said to have been wrong about a number of things. None of these options are open to Shapin and Schaffer because they stick to their unqualified first approximation of rationality.

To remain indiscriminate in this way is in fact not a form of charitable interpretation but a result of *symmetrical* interpretation. Because both contestants in the Hobbes-Boyle controversy are seen as equally rational, the debate between them could not have been solved by rational means. In SSK-style the authors argue that we must look at the socio-political constellation in which the intellectual debate was fought to explain the choice for the experimental programme.

Another indication that Shapin and Schaffer are using symmetry instead of charity is the fact that they do not use charity to bridge a gap in understanding and opt for the most rational interpretation. On the contrary, there is no underdetermination in this sense: considering the arguments of Hobbes, the authors present strong evidential support to qualify Hobbes' anti-experimentalism as rational and sensible. The only gap in explanation that needs to be filled is how theory choice can come about when no appeal to rational standards can close the debate. The solution is provided by making an appeal to social factors, namely the symmetry principle of the strong programme.

What Shapin and Schaffer insist on is that any verdict of rationality or irrationality must be based on evidence, that is on causal analysis. No prior

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[206] Norris (1997).

assumptions of rationality must guide the process of interpretation: what counts as rational is always shaped in historical contexts and nowhere else. This comes close to what Henderson has been arguing for with his principle of explicability.[207] He turns against a sociology of error and argues that rational and non-rational interpretations both require evidence (i.e. causal analysis): "we clearly see that rationalizing explanation and irrationalizing explanation are, in an important sense on a par. Attributions of certain forms of irrationality can be, and are, every bit as explanatory as attributions of rationality… any preference for rationalizing explanation must arise from theory-dependent preferences."[208] For this reason Henderson rejects charitable interpretation, and even some aspects of humanity-based interpretation. To the problem of conceptual relativism that such naturalism produces Henderson suggests that a minimal cost analysis in terms of a comparison with respect to epistemic virtues such as simplicity and fruitfulness, between existing beliefs and candidates to replace them, is sometimes needed.[209] Further he also realizes that he is trying to say something about constraints on historical interpretation, but in order to do so he needs to historicize these constraints themselves. In order to combat this circularity Henderson appeals to a strategy of bootstrapping.[210]

Returning to the use of charity by Shapin and Schaffer we must notice that their reference to the concept, and especially their unreflective reference to Gellner, is curious. According to Gellner, Shapin and Schaffer would not count as proper contextualists. He was one of the first who warned that too much rationalizing explications of distinct social practices are bound to misdescribe the social situation. He pointed this out in discussing examples from anthropology such as the concept of 'agurram' or the logic of the 'Zande'.

The *agguram* is a spiritual leader of Moroccan tribes who reacts on instruction from above as well as on daily experience. According to Gellner it is not difficult to perceive logical inconsistencies in the workings of the *agguram*. Yet: "Despite its logical incoherence, the concept of *agguram* survives because it plays a significant role in dictating social behaviour. We would misconstrue the situation if we neglected this social role and insisted on logical charity. Because language can have functions others than communication of truths, translation

[207] Henderson (1993).
[208] Henderson (1993) p.254.
[209] Ibidem p.250. This is a reflection of an idea of, among others, the later Kuhn. In chapter 7 we will return to it, as comparative analysis of theoretical virtues is an important part of the interpretation of the rationality concept that is proposed there.
[210] Following Glymour (1980). The idea of bootstrapping will be discussed in chapter 6.

cannot always be charitable."[211] Thus "the over-charitable interpreter, determined to defend the concepts he is investigating from the charge of logical incoherence, is bound to misdescribe the social situation. To make sense of the concept is to make non-sense of society."[212]

Another example stems from the Zande. Zande people believed in witchcraft. Witches can be male or female, as both can possess a witch substance somewhere in their bellies. Men inherit this substance only in paternal line and women only in maternal line. Now, to tell whether someone is a witch or not logic dictates that one only has to check the tree of descent of that person. Yet on a case-by-case basis Zande people violate this logic and invoke a variety of explanations. This may be explicable considering the social circumstances but it would be wrong to describe the behaviour as rational since basic logical principles are violated. According to Winch and also Bloor (1976) the system of reasoning the Zande use is not better or worse than our system as both should be understood in their own context.

For Gellner a lot of practices, especially religious ones, can be understood in their social and psychological functioning, but they should not be interpreted as rational or as true.[213] His general idea for interpreting societies distinct from ours is the following. As long as we find behaviour, with the inclusion of a consideration of the local circumstances, rational, not much further explanation of the behaviour is needed. But as soon as the divergence with what we find rational becomes too great we must interpret others as irrational, provided we can come up with explanations for this behaviour.

Perhaps Shapin and Schaffer's reference to Gellner was a case of Chinese whispers, given the chain of references that was involved: Shapin and Schaffer actually refer to Collins (1981b), it is he who refers to Gellner (1973), and it is Gellner who explores charitable interpretation for the social sciences, via the work of sociologist Winch and anthropologist Evans-Pritchard, based mainly on Quine (1960).

Thagard and Nisbett (1983) support Gellner's views, as they defend the third interpretation of charity principles from the list above: 'Do not judge people to be irrational unless you have an empirically justified account of what they are doing when they violate normative standards.' They argue that charity is a useful maxim but only to a certain extent. People can be interpreted as irrational

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[211] Thagard and Nisbett (1983) p.254.
[212] Gellner (1973) p.39.
[213] Note that these social practices may also not be perfect. As Lévi-Strauss pointed out: "Dire qu'une société fonctionne est un truisme; mais dire que tout, dans une société, fonctionne est absurdité." This also holds for the society we live in at present!

when social reasons for their behaviour can be given. This presumably means that the investigator, when confronted with a different cultural practice, has to be aware that both types of explanation may be applicable.

The extent to which charity is a useful maxim was also explained by Gellner. He argued that we need some form of prior normative standard in order to determine what aspects of the context we are interpreting will count as relevant: "After all, there is nothing in the nature of things or societies to dictate visibly just how much context is relevant to any given utterance, or how that context should be described."[214] Charity is thus a necessary mechanism of selection but 'contextual charity ends at home'. That is, in interpreting others we must go on our expectations of successful reasoning. These expectations must be attuned to the specific context, but this cannot go too far as that would lead to misinterpretations of the very context under explanation. Every historical context, including our own, contains inconsistencies, falsehoods, irrationalities, etc. Hence: "Excessive indulgence in contextual charity blinds us to what is best and what is worst in the life of societies. It blinds us to the possibility that social change may occur through a replacement of an inconsistent doctrine by a better one or through a more consistent application of it."[215]

We can take stock of the discussion on the proper use of the principle of charity so far. Two issues appear to be central: how to account for improvement and what counts as proper contextualism. The trouble with the excessively normative and the excessively descriptive approaches to charity is that they both make it practically impossible to speak of room for improvement in a given context. The highly normative approach does so because everything is translated in present-day terms and hence no significant change can be accounted for. On the other hand, counting everyone 'right' on the other hand leads to theoretical relativism. Thagard and Nisbett put it this way: "A rampant principle of charity pre-empts the possibilities of criticism and improvement. If we cannot assume actions and judgements to be irrational, then we cannot hope to educate and improve choice strategies and inferential procedures. A heavy-handed charity principle would freeze human behaviour in an unprogressive amalgam of late-twentieth-century procedures."[216]

A middle ground must thus be sought between these extremes, but that is no easy matter. It is for example known that 'primitive' tribes who have never seen squares or lines cannot recognize these as such. Are these people less rational

[214] Gellner (1973) p.31.
[215] Gellner (1973) p.39.
[216] Thagard and Nisbett (1983) p. 263.

than we are? One would be inclined to say no, because geometric abstractions serve no purpose at all in their way of living. In what sense would it be an improvement if more abstract forms of reasoning became available in this context? Perhaps none at all. Room for improvement then must be something like a zone of proximal development.[217] An assessment of improvement is therefore highly dependent on the features of context and not on some extra-historical standard.

The other issue, the proper description of context, is closely related to the question how to conceptualize room for improvement. A proper description of contexts should also show errors, absurdities, inconsistencies, illogicalities, etc., in that context. This is what Gellner, Thagard, Nisbett and others have been arguing for. However I am reluctant to accept the choice they offer between either charitable interpretation *combined with* a sociology of error, or else symmetrical explanation of past science. In this way symmetry and charity always exclude each other. This either/or choice is a result of a dichotomy between social explanations and rational explanations of science. When rational interpretations of past contexts fail, one has to explain the functioning of irrational elements, present in the historical context, with reference to social factors. This leads to an unsatisfactory treatment of errors in science, as will be demonstrated in the next chapter.

To meet the challenge of moving beyond the dichotomy, while maintaining an evaluative context, it is essential to see that there are in fact *common* assumptions behind the two opposing ways to study past science (i.e. charity + sociology of error vs. symmetry). One such common assumption is that speaking of progress requires a context-independent norm of rationality. Either such a norm is available, and we can speak of progress, or it is not available, and we can no longer speak of progress, and hence we must resort to causal explanations of past science. More such common assumptions are unearthed in chapter 5, section 3. My contention is that history of science still has not found a way to satisfactorily move beyond these common assumptions and that the debate on the proper approach to the study of past science is stymied by them.

For now, the issue of the description of context is closed by looking at two examples from historiography of science featuring successful applications of charity. The first stems from a study by Jardine of Copernicus' orbs.[218] Jardine uses the principle of charity to support an interpretation of a key passage from Copernicus' book, *De revolutionibus*. The interpretation he has to offer goes

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[217] This concept stems from Vygotsky (1978).
[218] Jardine (1982).

against the interpretation of a number of colleagues such as Menzzer, Wallis, Kuhn, Duncan and Rosen. The point of discussion between them is what the word 'quoniam', roughly translated as 'seeing that' or 'given that', refers to. Jardine states that "the Latin is ambiguous and my reading rests largely on the principle of charity of interpretation."[219] He thus translates the term so that it facilitates the most coherent reading of the text. Prior to this, chapter 4, book 1 of *De revolutionibus* had been read as chaotic and obscure. Jardine's translation makes the 'that' refer to something else and this alters the received interpretation of the whole chapter. A coherent justification of Copernicus' idea of uniform circular or compound circular motion of each of the visible heavenly bodies can now be read into the chapter. This idea crucially underpins the rest of Copernicus' astronomical theory of the mobility of the Earth and the heliocentrism defended later in the book.

Jardine argues that Copernicus supported the idea that celestial orbs were solid orbs, not just the result of mathematical imagination required to save the phenomena, but real physical things like bodies or corpora. These orbs carried the planets in their circular motion. Support for this claim is in line with a couple of natural philosophical doctrines that were, according to Jardine, commonplace at the time, such as a sharp distinction between terrestrial and celestial domains, a distinction between two types of movers, *motores separati* and *motores inseparati*, and the idea that planets are inseparable from the substance of the orbs they belong to. If these natural philosophical ideas are seen as a justification of Copernicus' astronomy, the argument of chapter 4, book 1 becomes much less incoherent.

The direct textual evidence to support this interpretation is, as Jardine himself acknowledges, 'slender'. As one of the crucial textual passages is also ambiguous, there is no other way to favour a translation of the passage than to make use of the charitable assumption that Copernicus strived to be as consistent and coherent as possible. Jardine seeks further justification for his interpretation by relating it to contextual evidence. This evidence comes from Aristotelian doctrines that Copernicus must have learned during his training, the writings of contemporaries, testimonies of successors and other writings of Copernicus himself. When considered in the context of 16th-century views on the status of mathematical astronomy, the slender textual evidence becomes compelling, according to Jardine.

This is a clear case in which charity is used to solve a problem of underdetermination of interpretation (note again the difference with Shapin and

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[219] Jardine (1982) p. 192.

Schaffer's use of charity). The choice for the most coherent interpretation is made after a careful dismissal of existing interpretations in historiography. The choice is also contextualized as far as possible. It is therefore well defendable and leads to revealing insights. It is also a good example of the continued usefulness of charitable interpretation, which exceeds the early stages of (radical) interpretation.[220]

Very much like Jardine, G.E.R. Lloyd has advocated the use of charity combined with thorough contextual description. According to Lloyd the original context in which ideas were put forward needs to be recovered as fully as possible, yet this alone will not suffice because in participant observation the observer is not a participant and his or her observations are never entirely theory-free. Lloyd stresses the heuristic effect of charitable interpretation and the *dialogue* that can follow upon the initial approximations:

"Understanding another's point of view is difficult and can never be complete, for we must always be prepared to revise what we had thought we had understood. Yet it is not impossible to reach at least an approximate understanding, as the very possibility of such revision presupposes. I can never be sure that what you mean by the terms "star" or "sun" or "heart" or "blood" is precisely what I think you mean by them, in general or in particular collocations. But the principle of charity of interpretation allows and dictates that I adopt some notion of your understanding as a working hypothesis, open to revision as the dialogue between us proceeds."[221]

Lloyd further shows how difficult translations in fact are by looking at concepts used by the ancient Greeks:

"The terms we conventionally translate "doctor" (iatros), "mathematician" (mathematikos), "philosopher" (philosophos), "physicist" or "natural philosopher" (phusikos, phusiologos), "musician" (mousikos), "architect" (architekton), "engineer" (mechanikos), all have points of contact with, but all diverge to a greater or lesser degree from, what those conventional renderings may suggest. Moreover, none of those terms pick out a single inquiry or activity about which the Greeks themselves had clear and unanimous views. Each was in Antiquity already the subject of disagreement and dispute,

[220] In his book *The Fortunes of Inquiry* (1986) Jardine argues that we need interpretive principles to deal with threats of incommensurability, indeterminacy and underdetermination. He wants to maintain that theories form 'inquiry series'. This requires some degree of maintenance of reference and the possibility of translating sentences of one theory into a successor theory. He supports the claim that change, and thus difference, can be understood only against a sufficient background of agreement. He supports Davidson's ideas on charitable interpretation over principles of humanity for this reason. See especially p.126.
[221] Lloyd (1992) pp.565-566.

with rival individuals or groups implicitly adopting or explicitly defending divergent conception."[222]

For Grandy the best translation is the one, which best preserves meaning.[223] Lloyd however aims to show that translation that fully preserves meaning is impossible. With 'thick description' and a sophisticated use of charity we can get at no more than approximations that must always remain open for revision.

The idea of the dialogue is central to the interpretation process. First this is so because a neutral language via which the participants' language can be translated into the observer's is not available:

"Neither modern nor even ancient categories will do precisely for all our purposes. I have argued that in general ancient ones are preferable to modern, but also that it would be absurd to think that we can use the ancient ones entirely to replace our own. Any search for an entirely neutral language in which to report and discuss ancient ideas is, in any case, bound ultimately to fail."[224]

Second, the dialogue points into the direction of comparative historiography of science:

"The consideration of the convergences and the divergences between ancient Greek ideals, methods, and practices and later European ones can be used, judiciously, to throw light on the former–as well as on the latter. So too can comparisons with other ancient societies, where those between ancient Greek and classical Chinese inquiries seem to me particularly fruitful. It is not that we should try to judge either by the criteria of the other. It would be foolish to ask why the Greeks had no concept of Qi or the Chinese no exact equivalent to stoicheia. It is rather that the exploration of markedly different traditions of intellectual inquiries in contrasting social and cultural circumstances can serve as a reminder of the parochialism of each–or at least of their culturally specific characteristics. Thus, as I have claimed, it is largely by the comparative method that we can reach clearer ideas both of the explananda and of possible explanations."[225]

In both cases the dialogue enlightens both participants through the comparison of their views. This is an interesting addition to the already comparative nature of charity-based interpretation.

[222] Ibidem p.567.
[223] Grandy (1973) p. 440.
[224] Lloyd (1992) p.576.
[225] Ibidem p.576-577.

5. Towards a weighted principle of charity for historiography of science

In this last section, the main points that have been made with respect to the principle of charity will be summarized. We have established that charitable interpretation rests on a comparative choice procedure in which the choice for the most fitting historical interpretation is guided by a concept of rationality. Therefore how the interpretation procedure turns out depends very much on how this concept is understood. The assumption of a rationality concept is indeed a form of theory-laden interpretation. This however is unavoidable. Even the most liberal usages of the rationality concept are evaluative, as counting everyone as rational is also an evaluative stance.

We need charity to get the interpretation process running. When we are trying to understand foreign conceptual schemes something is needed to establish a connection between our conceptual scheme and the foreign conceptual scheme. Because of the deep holism of meaning, beliefs and cognitive attitudes, no independent means are available to gauge interpretation. So we need guiding principles to break into the web of foreign belief. Agreement comes prior here to disagreement, as differences can be made sense of only on the grounds of similarities. If too much error or disagreement is presupposed the possibility of making connections with the network of the speaker would decrease and thereby the intelligibility of the speaker's behaviour and attitudes would undermine the very possibility of regarding the speaker *as* a speaker.[226]

This is the first reason why charity-first interpretation works better than humanity-first. The second is that with humanity-first we still don't know what to do in cases where clues for interpretation are absent, because with humanity-first we must equally expect rational and irrational behaviour and hence we don't know what to favour. For these reasons a properly understood principle of charity has been endorsed in this chapter.

Insisting on charitable interpretation is generally at odds with symmetrical explanation of past science. It also presents a challenge to perspectivism. As we know an important argument for perspectivism stems from the incommensurability thesis. The discussion of the principle of charity makes clear that complete incommensurability cannot be made sense of: "Clearly no historiographical activity would be possible given a total failure of translation from past texts as envisaged by Quine, the substantive challenge is therefore to

[226] Malpas (1988) p. 20.

secure evaluations of past science in the face of partial failure."[227] Any difference has to be stated in our own terms; this alone assumes agreement.[228] It is the partial disagreement or partial failure of translation on which the charity-guided interpretation process further works. Moving beyond perspectivism means bringing about a *direct* connection between our conceptual schemes and the world. This has produced an interesting connection between Davidsonian charity and posthumanism, and it is something we must include when developing our version of extended naturalism.[229]

As some have pointed out, charity may even be more than a *sine qua non* with regard to understanding others; it is an essential component of understanding itself.[230] As bringing about understanding is nothing else than drawing connections between conceptual schemes, the principle of charity is grounded in such interconnectedness.

Defending a charity-first approach on general grounds is however not enough, as the application of the principle needs to be spelled out in more detail, also with respect to interpretation in history of science. We have settled for a Davidsonian interpretation of the principle. This principle has been rejected on the grounds of its insensitivity to error or disagreement, of being an embodiment of linguistic imperialism and of being simply a species of verificationism.[231] Further it has been said that charity is useful only in the initial stages of interpretation and can be suspended thereafter. According to Malpas much of the criticism of Davidson stems from mistaken readings of his writings and mistaken understanding of what he is trying to accomplish with the principle of charity.[232] Following his analysis I end the chapter by listing six points of observation. These make clear that none of the criticisms mentioned above applies. Taken together the observations define the use of the principle for history of science.

First, charity should be applied on the right level. It should not be applied to translation of sentences with the aim of maximizing truth-values of past utterances. Agreement with others should be sought on the level of

[227] McAllister (1986) p.323.
[228] Davidson (1973) p.6.
[229] In the work of Davidson 'direct mediation' is further developed in the concept of triangulation: person x and person y interact with each other but they also interact with the world. See also Amoretti and Preyer eds. (2011).
[230] Geurts and Van Brakel (1988), Malpas (1988).
[231] Interpreting Davidson as a verificationist amounts to (mistakenly) downplay the difference between him and Quine. Examples are Sterelny (1981) and D'Oro (2004).
[232] Malpas (1988), more elaborate is Malpas (1992).

propositional attitudes or cognitive goods such as rationality, intelligibility, consistency, sensibility, plausibility, meaningfulness, etc., including assumptions that others strive for logical reasoning, communication of truth, action in accordance with belief, etc.[233]

Second, these categories should be tailored to the historical situation. We have to place ourselves in the historical situation, imagine what the problem situation was and which cognitive resources were available to solve the problems. Logical principles that we think are valid but that do not fit the network of beliefs of past actors cannot be expected to hold in that context. Although this allows for variety of belief, it does not lead automatically to an omnibus relativism.

Third, charitable interpretation is not insensitive to attributions of irrationality and error. From the very start of the interpretation process there is room for *explicable* error. Seeking agreement on the level of rationality makes this possible as we can say that past scientists intended to state correct theories, following the best of their capacities, but that nonetheless later theories proved to score better. Further assumptions about possible disagreement are built in from the start of the interpretation process. It is part of *our* network of beliefs to expect differences with past networks of belief. When good reasons can be given why an error in a past network of belief can be expected, this may be used in the interpretation process from the very start. The central demand Davidson places on rationality is consistency or coherence. Including explicable error is a move to maintain overall coherence of our beliefs. It may however be that inconsistencies go unnoticed because they are compartmentalized and do not infect the rest of the network of beliefs. This must not be judged severely. But as soon as we are confronted with inconsistencies in our network of beliefs, we must attempt to resolve them. We assume others do the same.

Fourth, it cannot be stressed enough that charitable interpretation is a process that starts with supposing agreement but does not have to end with it. When no satisfactory interpretation emerges we have to change some of our earlier assumptions, i.e. make adjustments in the connections between our network of beliefs, desires and attitudes and that of the others. This in turn may involve making adjustments in the networks themselves. This implies that the interpreter has to take distance from his or her own set of attitudes because the interpretation process started on the premise of complete agreement of these attitudes. This process terminates when the interpretation of texts can be

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233 One can also think here of Gricean maxims guiding linguistic communication such as the principle of relevance, see Grice (1989).

satisfactorily accounted for. However, when new evidence calls for it, the interpretation cycle may have to start over again.

The process can be compared with interpretation guided by Weber's ideal types.[234] Weber argued that reality is overcrowded with structures. Man can extract something meaningful out of it only by selecting structures that appear important to him. This is what ideal types do. Ideal types are abstractions or perfections of a particular phenomenon under study. The ideal type might be the result of a quick generalization of a few individual historical cases. Ideal types should not be applied at random but must meet demands of causal and meaningful adequacy and must remain within the borders of our imaginative capacities. The generalization functions as a model with which comparable cases can be studied. For example, with the interpretation of an ideal field marshal, who is fully knowledgeable about the weaknesses and strengths of his own and his opponent's army, aware of all possible strategies and their effects in combat, etc., one can start to explain why a particular battle developed as it did. The capacities of the ideal field marshal suggest how the course of events should develop. When deviations from these expectations occur, one can either maintain the ideal type and seek reasons for the deviations, or, when this is not possible, one has to change the initial expectations. Interpretation thus starts with a model expecting certain forms of behaviour but the process may require changes in these expectations and hence adjustments of the model. Yet without the initial formulation of the ideal type no interpretation process could get started because there would be no standard to refer to.[235]

This leads to a fifth consideration. We can view interpretation of the past as entering into a dialogue with it. Of course no past actor can speak back to us directly but comparisons between past and present, next to being beneficial for understanding the past, can teach us something about the present, cf. the argument given by Lloyd above. Self-knowledge is possible only through confrontation with others. Understanding others is a matter of making connections. Hence entering into a dialogue results in a network of connections.

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[234] Weber (1922). Interestingly a German tradition of thinking about charitable interpretation in terms of a 'Prinzip des Wohlwillens' or 'Wohlwillende Interpretation' goes back to Weber. See Forum für Philosophie Bad Homburg (1990) and Bühler (2003). Weber's thinking has deep roots in the hermeneutic tradition as well.

[235] Note that in chapter 7 some distance to Weber is taken up when the concept of rationality is considered in terms of heuristics. Heuristics are goal-oriented procedures but do not require idealizations.

Improved understanding is expressed by expanding and deepening connections between two networks. This is a continuous balancing act and it may involve adjustments in our own system of beliefs. One can read this dynamism in Davidson: the theory of action and the theory of belief of *both* the speakers and the interpreters are interrelated. Setting up a connection between these networks makes them both liable to change. Since interpretative situations change continuously the dialogue is essential: no interpretations can be finite as interpretations are always open to reinterpretations.[236]

Charitable interpretation was motivated to solve the problem of indeterminacy of translation or interpretation respectively. What might not appeal to some is that we are still left with some indeterminacy at the end of the interpretation process. Quine however was right when he pointed out that: "translation is not the recapturing of some determinate entity, a meaning, but only a balancing of various values."[237] Charity is needed to get this balancing going in the first place. It does lead to fixing interpretations but not to a full closure of these. Full closure is simply not part of the nature of interpretation, hence a degree of indeterminacy has to be accepted.

Sixth, in the process of interpreting the past there is no sharp break between the initial application of charity and the application of charity later on. Charitable interpretation continues to be a useful device, as Jardine's study of Copernicus has demonstrated. There continue to be cases where underdetermination prompts a choice, even when the historian already possesses a lot of insight in the matter at hand. If a coherent interpretation can only be fixed by using charity, charitable interpretation continues to be relevant beyond the initial stages of historical interpretation. As long as there is no information to the contrary, charity keeps advising us not to attribute too much error or inconsistency to others.

With these six elucidations it has become clear that the standard criticisms to charitable interpretation do not apply. As Gellner put it, charity is not used to dominate others: it is just an essential part of the interpretation process. Other people do the same! In the previous chapter it was concluded that science has to be studied from within, since no appeal to external standards can be justified. The weighted charity principle formulated in this chapter elaborates on this point. What needs to be accepted is that the process of interpretation is circular

[236] In spite of difference in style, interests and philosophical orientation Malpas notes the similarities of this 'circle of interpretation' with continental philosophers such as Schleiermacher, Gadamer and Heidegger. This is not surprising considering the common origin of these ideas sketched in section 1.
[237] Quine as quoted in Malpas (1988) p.33.

and the resulting interpretations are never definitely fixed. Where conceptual or logical circularity lead to flawed explanations, not every form of epistemic circularity is harmful. Further the openness for re-interpretation is an invitation for improvement. One has to come up with very good reasons to bring changes in interpretation about. The unattractive alternative to all this is conceptual relativism, or evenhandedness. Evenhandedness however amounts to empty handedness, and not to the desired mutual respect, because what it is that deserves respect has become unclear. I therefore conclude that true tolerance and true respect can be found only in discrimination.

Chapter 4 Towards a Proper Conceptualization of the Notion of Error

1. Introduction

It is natural to expect that the notion of error occupies a prominent place in any theory of change in historiography of science. After all, why should we change our theories of the world if these are never found to stand in need of correction? Could there be a history of science at all if no errors occurred? Yet, as will be demonstrated in this chapter, the strange thing is that none of the major approaches to past science offers a satisfactory analysis of the phenomenon of error. The bold thesis that can be inferred is that history of science lacks a good theory of change.

There are two reasons for this. On the one hand the main task in philosophy of science and epistemology has been to secure positive knowledge. Although philosophers have often recognized the importance of the study of error, this has not led to deep investigations into the phenomenon. Approaches to past science informed by philosophy reflect this attitude. On the other hand the process of naturalization of science studies has led to an output of mainly descriptivist and localist historiography. The increase in detailed historical knowledge this has brought has not been used to intensify epistemological debates. Reading current historiography of science, it is, at first glance, as if no one has ever made a mistake. Deeper inspection shows however that an altogether different view on what knowledge is, and how it is formed, leads to a different perception of what errors are. Knowledge is seen, not as (a variant of) justified true belief, but as *authorized* belief (see chapter 2). This means that the notion of error also has to be understood in relation to social processes of authorization.

For analytical purposes, I believe it is useful to classify all approaches to past science into two groups, which reflect the two basic outlooks on the phenomenon of error. Even though profound differences among members of each group exist, it is clarifying to focus on what the approaches have in common. I call the first the 'errors as obstacles' approach.[238] Science in this

[238] This takes its cue from Schickore (2009) p.31, where she identifies what she calls 'appraisive history' as follows: "Errors are obstacles; they have a negative impact on

approach is basically seen as an error-correcting process. Scientific methods, technological improvements and standards of rationality enable us to remove errors from our theories of nature. I call the second the 'errors as failures' approach. This approach denies that context-independent methods for the correction of errors exist. Ultimately the acceptance and rejection of claims to knowledge are grounded on standards that are the result of negotiation. Truths, that is, accepted claims to knowledge, have gained the upper hand in these processes. Errors have failed to gain prominence. One can analyse such failures in terms of failing to meet the demands of a particular situation, like failing to meet specified functionality in the design of artefacts. Hence the second group is identified as 'errors as failures'.

Note that this classification in two groups is not similar to the realism vs. anti-realism division, as realists and anti-realists can be found on both sides.[239] The division in two groups comes about when we consider the issue of demarcation between an internal realm of science and an external context, which often boils down to a strict distinction between rational and social factors. Any approach to past science that maintains some form of this demarcation finds itself classified under the 'error as obstacles' approach. Approaches that move beyond demarcation, i.e. symmetrical approaches to the study of science, find themselves classified under the 'errors as failures' approach.

In the following two sections the two approaches to error will be further clarified. Both approaches have certain merits but also suffer from serious shortcomings. This discussion will yield the requirements that a satisfactory theory of error must meet. In the final section of the chapter we will be looking at the implications for both history and philosophy of science. I argue that we should include the study of the phenomenon of error in a broad philosophy of experiment. I also defend the claim that the notion of uncertainty has to occupy centre stage in a theory of scientific change. I view the wish to decrease uncertainty as the primary driving force in science and it is from that the phenomenon of error in past science has to be accounted for. Just as in the previous two chapters on the principles of symmetry and charity, I end the chapter by indicating how historians of science can put these ideas to work.

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scientific developments. To correct them, the sources of these errors - bad instruments, incorrect theories - need to be removed."

[239] Laudan's approach is for example classified in the 'errors as obstacles' group and following Laudan (1981b) he must be seen as an anti-realist. Bloor (1999) has committed himself to ontological realism, and his approach is classified in the 'errors as failures' group.

2. The 'errors as obstacles' approach and its shortcomings

For a good grasp of the 'error as obstacles' tradition we have to go back to the 17th century and to Francis Bacon, who indicated four sources of error, the so-called Idols of the Cave, Tribe, Theatre and Market place.[240] The Idols represent forms of conditioning that limit our capacities to consider the facts of nature without prejudice. The Cave refers to aspects of a person's upbringing, i.e. the life experience of an individual, which is different for everyone. The Tribe refers to general human cognitive limitations, such as our faculties of perception. The Theatre refers to the whole set of theological and philosophical doctrines people imbibe. Finally the Market place refers to limitations set by language. The fact that each language embodies different classifications of the world is problematic. But even in one language many "ill and unfit words" occur which "wonderfully obstruct the understanding." Only when the four Idols are circumvented is it possible to arrive at a true picture of nature. Bacon thought this to be possible: "the human senses and understanding, weak as they are, are not to be deprived of their authority, but to be supplied with helps."[241] This help of course had to come from experimental research, which for Bacon provided unbiased access to nature's secrets. Thus, Bacon's empiricism was grounded in an attempt to avoid errors.

Descartes agreed with Bacon that errors could in principle be avoided with enough caution.[242] For Descartes mistakes could come about only by improper use of the will. God had endowed us with sound faculties of judgement, yet the uncontained use of them could lead to error. Man necessarily is a limited being and in his reasoning he cannot have access to all possible ideas. In this limitation lies a source of error, as judgements need to be made. People should withhold belief when judgements do not lead to 'clear and distinct ideas', which was Descartes' definition of certain knowledge. Often people do not do this and hence this, together with our limitations, is the major source of error. However, Descartes thought that following prescriptions of sound reasoning could avoid most errors, even taking into account our finite intellectual capacities.

In the 17th century it was common to identify science with true knowledge, which makes errors fall outside the boundaries of science. In the course of time, however, it turned out that to arrive at certain knowledge in a straightforward

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[240] Bacon (1620), aphorisms, book I, XXXIX-LXVIII. I briefly referred to the Idols in chapter 1, section 6.1.
[241] Bacon (1620) LXVII.
[242] Descartes (1641), Fourth Meditation, Part Two.

manner is not an easy matter. This led to the realization that it is better to view science as a process of theory replacement. The demarcation between science and non-science thus came to lie in the method governing the soundness of theory change.

We can interpret the search for the correct scientific method in the 18th and 19th centuries as a search for the right kind of empiricism. Skipping over the many scholars who thought about the scientific method in these centuries, we can still clearly see a reflection of this attitude in the birth years of professional historiography of science.[243] Sarton, for example, recognized, apart from errors as a result of superstition, undeserved privileges, etc., a category of errors as part of the good tradition. Theories, containing errors from a later perspective, could be regarded as genuine improvements on their predecessors. For Sarton, the most important thing in science was the continuous criticism of its results.[244] Sound methodology had to ensure this: "There are no dogmas in science, only methods; the methods themselves are not perfect but indefinitely perfectible."[245] It was through the application of these continuously improving methods of research and critique that the margin of error gradually decreased.

Philosophers of science have equally focused on the systematicity behind theory change. They have set up meta-methodologies explicating what makes science a rational endeavour, demarcating it in this manner from other human activities. This is perhaps clearest in Popper's critical rationalism. For Popper the concept of error was of special significance. He declared that all the essays of *Conjectures and Refutations* were "variations on a single theme- the thesis that we can learn from our mistakes."[246] This was put even more strongly in an afterword to the second edition in which Popper added, "all our knowledge grows only through the correcting of our mistakes."[247] Popper viewed science as a process of error elimination through the application of a falsification procedure. All scientific hypotheses had to be put to critical trials, testing their claims and predictions. Theories that failed to meet the tests had to be rejected and replaced by others. According to Popper this process of falsification was capable of unmasking all potential errors. It is basically a process of trial and error. Popper first was reluctant to draw an analogy between his theory of

243 See Losee (2004) for the ideas of a number of earlier scholars.
244 In this respect, as we saw in chapter 1, Sarton thought historians of science had a very important role to play, complementary to the work of scientists.
245 Sarton (1952) p.41.
246 Popper (1963).
247 I owe this observation to Hon (2009) p.11.

science and the theory of evolution but later he put explicit emphasis on this analogy.[248]

While Popper saw science as a process of error elimination, he did not develop a theory of error in sufficient detail. For him, errors are still only equated with false belief, because only hypotheses can be put to tests of falsification. Popper's framework does not account for the variety of levels on which errors can occur. From Popper we learn nothing about the causes of error, nor of the various effects errors in science can have.[249]

Something similar is at hand with the historian Alexandre Koyré. Koyré thought that historians of science had to pay more attention to the phenomenon of error because this could yield profound insights in the process of theory change. According to Koyré correct knowledge claims stand in need of much less explanation because it is the natural thing to arrive at truth. In case of errors extra reasons must be supplied to explain why they occur.[250] As already mentioned in chapter 2, Koyré discussed in detail the captivating case of the simultaneous occurrence of an error in the work of Galileo and Descartes. Both arrived independently at a same mistaken formula for acceleration during free fall. The fact that the error in the formula of free fall came up twice must, according Koyré, be explained by the then dominant theory of impetus physics, which made it hard to conceive of motion in terms of temporal relations. The main conceptual change Galileo achieved was to give up a basic idea of impetus physics, namely that the motion of an object was the result of an internal cause of that object. Instead he started to see motion and rest as physical states that could be determined by calculation. Bodies once in motion had no need to stop, or even to slow down, as in impetus physics.

Causes of error, according to Koyré, had to be sought in the weaknesses and limitations of the human mind, which he attributed to psychological and even biological conditioning.[251] An essential part of overcoming errors came from adjustments in the general 'thinking cap'. In this way Koyré provided an explanation of the causes of error and also offered a mechanism of how they are

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[248] Popper (1984). A broader discussion on the analogy between science and evolution can be found in chapter 6.

[249] Hon (2009) p.12-13.

[250] Another historian of Koyré's generation, E.J. Dijksterhuis, valued the study of error likewise because it yields insight in the process of scientific thinking. Scientists' blotting papers can be of interest, not in order to find errors in the scribblings of 'great men' but to reconstruct the development of ideas before they became expressed in 'logically faultlessly ordered systems of definitions, axioms and hypotheses', Dijksterhuis (1950) p. 375.

[251] This was possibly inspired by Mach (1976). See section 4 below.

corrected. And yet, very much like Popper, he did not offer much insight in epistemic phenomena of error.[252] The mode of explanation he had to offer may have been appropriate to the Galileo-Descartes case, but clearly cannot function as an explanatory model for the phenomenon of error in all science. Unmasking errors does not always require a fundamental change in thinking about nature, as in the case of Galileo. Further, it remains unclear whether the error in the formula of free fall played a decisive role in the change in general perspective or not. Also, Koyré thinks about errors in terms of hypotheses only and this again does not cover the full range of aspects that have to be associated with the phenomenon of error. Even though Koyré attached great importance to the study of error, his treatment of the phenomenon, like Popper's, lacks the required breadth and depth.[253]

The conception of science as an error-correcting process, guided by some form of meta-methodology, which ensures the rationality of the whole endeavour, has permeated philosophy of science deeply. Even in the models of 'weak' demarcationists, which allow for a substantial role of historical context in the assessment of past science, this basic attitude towards errors is present. Such approaches rely on a so-called sociology of error in order to account for errors in science. At some point a separation between rational and social factors comes about where only social factors can explain persistent belief in erroneous theories in the past. Of these weak demarcationists I will discuss here the ideas of Lakatos, Laudan, Kitcher and Mayo.

In Lakatos' methodology of scientific research programmes there is an ambiguity about the place of errors. In some places he set rationality and error in outright opposition: "One can or should not explain all history of science as rational: even the greatest scientists make false steps and fail in their judgement."[254] This must mean that accounts of error belong to externalist historiography. But in other places he suggests that the problem of deviation from the correct path can be solved internally. His methodology of research programmes allows for temporal recessions in the development of a research programme. Errors can then be part of that internal history but only as an empirical explanation of a recession in the development of a research

[252] This is likewise pointed out in Hon (2009) p.15.
[253] Kuhn's interpretation of the history of science as a sequence of alternating paradigms elevates Koyré's idea of changes in 'thinking cap' to a general mechanism of thinking about change in science. Kuhn's approach to past science is however placed in the 'errors as failures' group and will be discussed below.
[254] Lakatos (1970) p. 118.

programme.[255] Nonetheless, the dominant view is that errors are impediments to the progress of science. The model of rationality has to ensure that science is capable of removing the errors. When carefully selected, this model can be shown to accord with large parts of the actual course of history. However, Lakatos made the highly controversial claim that, when this is not possible, the rational reconstruction should form the main text, whereas what actually happened could be relegated to the footnotes. No historian today would be willing to take responsibility for such historiography.

In Laudan's philosophy of science it is, unlike in Lakatos, not necessary to settle on one model of rationality in order to rationally reconstruct past science. Instead Laudan says: "We must cast our nets of appraisal sufficiently widely that we include all the cognitively relevant factors which were actually present in the historical situation."[256] This has to be established by taking into consideration the set of reasonable choice options, available cognitive resources, etc., present in the given historical context. For Laudan we must assess whether, given these specificities, rational choices were made. For him rational choices are determined by problem-solving effectiveness. It is possible to establish this objectively, even though on the issue which problems count as relevant problems we have to follow historical decisions as well. We can however establish which of the competing alternatives in a concrete case of theory choice had to be seen as the best problem solver.

In this model one can also speak of errors only in terms of problem-solving effectiveness. When people violate the demand to choose the most effective problem solver we must turn to a sociology of knowledge to find out why they did. Even in the highly context-sensitive framework of Laudan there still is an asymmetrical explanation of knowledge claims. Deviations from his model of rationality need to be accounted for by social factors, whereas correct choices can be explained by reference to rational factors. Again, error resolution is the main driving force in science, in the guise of discarding less good problem solvers in favour of better ones, and with the acknowledgement that problems found worthy of pursuit, and even to some extent standards to assess problem-solving effectiveness, are dependent on context.

Philip Kitcher went in a similar direction in an attempt to reconcile formal and sociological approaches to science.[257] According to Kitcher the following points should be uncontroversial about science: 1.science is progressive: this

[255] Schickore (2005) p. 541.
[256] Laudan (1977) p.128.
[257] Kitcher (1998).

manifests itself in increased powers of prediction and intervention, 2.this provides evidence that the entities we speak about actually exist, 3.this evidence is however not conclusive and remains open for revision, 4.scientific disputes are settled by appeal to canons of reason and evidence, 5.these canons progress with time. On the social side it is uncontroversial that 1.science is a human endeavour carried out by cognitively limited beings living in social groups that have a history, 2.scientists approach their research with categories and preconceptions that have been shaped by the history of the group to which they belong, 3.social structures determine how research is transmitted and received and 4.social structures affect the kind of questions that are thought to be most significant.

These last two points are very similar to what Laudan has been arguing for. Yet Kitcher clearly states that, in general, scientific disputes should be settled by appeal to canons of reason and evidence. According to him this is exactly why the whole endeavour is progressive.[258] Approaches based on symmetry undermine this key aspect of science. He suggests that for a proper sociology of knowledge we should return to Merton's sociology of institutions and his idea of the core values, commun(al)ism, universalism, disinterestedness and organized scepticism, which serve to demarcate the institution of science from other institutions in society. Although Kitcher acknowledges a degree of contextual contingency in science, he defends a line of demarcation that in my view is stronger than Laudan's, which is supported by reference to the somewhat old-fashioned approach of Merton.

The final example of the 'errors as obstacles' approach we will deal with is Mayo's programme of error statistics. This is perhaps the most important example, as Mayo's theory is one of the most developed accounts of the phenomenon of error in philosophy of science to date.[259] She asserts that in most theories of science:

> "Little is said about what the different types of error are, what specifically is learned when an error is recognized, how we locate precisely what is at fault, how our ability to detect and correct errors grows, and how this growth is related to the growth of scientific knowledge."[260]

This is a just observation and in line with what has been said above about Popper and Koyré.

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[258] Kitcher (1998) p.34.

[259] The central publication is Mayo (1996).

[260] Mayo (1996) p.xii.

Mayo interprets science as a collection of models and proposes to follow a strategy of actively probing for error on all these levels independently, which include primary hypotheses, data models, experimental models, etc. Her work is meant as guidance to present-day science, but can also be applied to the study of history. Her main case study is in fact historical, namely Perrin's investigation of Brownian movement. Perrin's research was designed to choose between the kinetic-atomic theory of Einstein, which explained Brownian motion by molecular agitation, and the phenomenal theory of thermodynamics. Mayo argues that Perrin convincingly proved that Einstein's theory was right because he adopted a strategy of probing for error on a variety of levels. Perrin applied statistical tests to rule out experimental artefacts, probed for concealed regularities behind the Brownian motion, and also tested for possible errors made in the calculated value of the Avogadro number.

According to Mayo, it is on the basis of such research that an argument from error can be constructed. When a hypothesis is probed for error in all directions, and no errors have been found, it has passed a 'severe' test. From this one can infer that the hypothesis represents reliable knowledge. It can become a very complex task to establish whether the reliability of claims to knowledge has been sufficiently argued for. It seems to me that this is not an argument against Mayo's theory because science may indeed be this complex, and a full-blooded experimental philosophy may have to reflect that. As long as there remains enough analytical grip on the complexity, the complexity itself is excusable.

For Mayo science relies, more than anything else, on methods of research. Probabilities of excluding error can be attached to methods of research. When a hypothesis passes a test with high probability of excluding error this offers strong evidence that the hypothesis is actually right. While we do not obtain absolute certainty, we do get a significant form of error control and this ensures the reliability of claims to knowledge.

Mayo acknowledged that a degree of indeterminacy must be accepted in modern science. In order to make well-founded claims we must therefore rely on some form of probabilistic reasoning. In her error statistical approach she however does not attach probabilities directly to the theories, hypotheses, or the evidence in question, as is for example done in Bayesianism, but to methods of research. Mayo rejected the Bayesian approach because she found it too dependent on a subjective assessment of prior probabilities.

She further defended the claim that we should combine the systematic techniques of statistical testing with a less formal list of canonical strategies to avoid clear exemplars of going wrong in a particular scientific field. The formal and informal ways of testing for error together constitute what she called an error repertoire of a given scientific discipline. We really learn in science by improving on our methods for error control. Methods in science must be seen as ways to exclude errors made in the past. As this tends to become forgotten, methods 'mask' errors in science and this, according to Allchin, is one of the reasons why philosophers, and historians alike, have neglected to properly study the phenomenon of error.[261]

Mayo makes a central point of the idea that 'experiments have a life on their own'. Her error statistical approach belongs to the philosophical current called New Experimentalism of which Ian Hacking, Peter Galison, Allan Franklin, Ronald Giere and Nancy Cartwright, are other important representatives.[262] New Experimentalists argue that there is a part of science, namely experimental practice, that is more or less independent of theory. The domain of experimental knowledge consisting of processed data, knowledge of experimental effects, knowledge of aspects of models, and also knowledge of the presence of absence of errors in these models. Practical knowledge is always related to higher-level theory but is at the same time independent of it because it is not dependent on one particular theory only. New Experimentalism claims to avoid a host of philosophical problems, which a purely theory-oriented outlook on scientific change has to face, such as the Duhem-Quine problem, the problem of incommensurability and the problem of underdetermination. These problems come about through sharp discontinuities on the levels of theoretical discourse. When this happens the New Experimentalist can safeguard continuity in science with the recognition of a body of experimental knowledge.

It is thus no surprise that Mayo remarked:

"The response to Popper's problems, which of course are not just Popper's, has generally been to 'go bigger', to view theory testing in terms of larger units- whole paradigms, research programmes, and a variety of holisms. What I have proposed instead is that the lesson from Popper's problems is to go not bigger but smaller."[263]

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[261] Allchin (2001). The idea of error repertoires as 'black boxes' of errors may offer interesting research possibilities for historians of science since they can try to open the boxes and study how past mistakes have shaped the development of scientific disciplines.
[262] See Chalmers (1999) or Boon (2009) for a detailed discussion of New Experimentalism. There is a clear link between New Experimentalism and the practical turn in historiography of science. Prominent historians of science such as Peter Galison can be associated with both.
[263] Mayo (1996) p.x.

Mayo argued that there is a blindspot in Popper's theory with respect to learning in science. When hypotheses fail to pass tests, they have to be fully rejected. When they have not failed any tests, they still do not count as positive knowledge. In the first case the problem is that we do not learn enough about what precisely is at fault and the destruction of hypotheses may be too drastic. In the second case we can never make the inference to reliability when this *is* justified, for example on the strength of correct predictions of the theory in question. Criticism can be much more constructive and knowledge claims better justified if experimental practice is properly taken into account.

Still, both Mayo and Popper defend the view that the central task of philosophy of science is to account for theory choice via rational procedures, which are context-independent. Just as in Popper we find in Mayo the view that our knowledge only grows through severe criticism of it. There is a clear analogy between Mayo's severe test and Popper's falsification procedure as both deliberately aim to find errors. It is true that methods for testing in Mayo's philosophy are dependent on scientific disciplines. Error repertoires are built up in the course of time and hence the methods that have to play a normative role are taken from scientific practice itself. However as long as the methods are accepted they also have a context-independent normative function.[264] Yet this can simply be read as an articulation of Popper's rather general concept of falsification.

Mayo and Popper thus are alike in terms of philosophy of science because they both view science as a process of error elimination. But they differ in terms of epistemology. They draw different conclusions with respect to the justification and reliability of knowledge claims, which stems from a different treatment of experimental practice. Mayo's general perspective on science is however still within the 'errors as obstacles' approach.[265] She repeatedly stresses that there is only normal science, which must be understood as standard testing. The testing procedures are designed to exclude errors and are context or paradigm independent.

Mayo's approach has the significant advantage over others that it is no longer just theory-oriented. She addresses a wider range of aspects of the phenomenon of error than for example Popper and Koyré did. However, her approach continues to suffer from other shortcomings that affect the 'errors as obstacles'

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[264] The methods can be subjected to empirical tests themselves. With this argument Mayo commits herself to normative naturalism, as explicitly pointed out in Mayo (1996). Normative naturalism at first sight appears as an attractive middle-ground option between formalism and naturalism. For this reason it is discussed at length in chapter 6.
[265] The close alignment of Mayo to Laudan in Mayo (1996) also supports this conclusion.

approach as a whole. All approaches in the 'errors as obstacles' group work with a normative meta-methodology because these norms are designed to exclude, or overcome, errors in science. I believe that the demarcation between the rational and the social that follows from this is too strict in all cases.[266]

Rational testing procedures are not the only way to reveal errors. Confrontations with other viewpoints, for example, can shed new light on things and show what is at fault. It is also hard to maintain that errors are always the result of social factors. Sometimes scientists follow a general approach that works in some cases, but not in others (see the example of the physiologist Pflüger below). This makes it hard to say that this approach was rational in some cases but based on social factors in others. In the previous chapter we have concluded that the overall rationality of past historical actors can be preserved even when the theories they defended turned out to be wrong. An excessively strong insistence on demarcation makes it hard to take this insight on board.

Weaker approaches have difficulty to maintain their choice of norms against counterexamples. We have seen that Laudan insisted on problem-solving effectiveness as the overriding factor in theory choice. There is however indication that this virtue can be outweighed by others. Giere (1988) provides the case of geology, in which the stabilist theory was long preferred over the mobilist theory, even though it was clear from the beginning that the latter could solve many more problems. Yet defenders of the mobilist theory for a long time could not provide a mechanism for the transportation of gigantic landmasses. Giere concludes, contra Laudan, that a problem solver can be rejected because other virtues appear to be more important. This is not to deny that problem-solving ability can be an important virtue that scientists often prefer, but it cannot be the only virtue at stake and should be weighted with respect to other virtues.

Weak demarcationists are reluctant to accept such extensions because they feel that through accepting a broader variety of norms they run the risk that all normativity in their theories of science goes by the board. It is for this reason that Feyerabend called Lakatos a 'fellow anarchist': in Lakatos' approach everything hinges on the pursuit of research programmes and this can be interpreted in such a way that everyone is free to follow the programme of his or her liking. Likewise Hon has suggested that Mayo's approach leads to nothing more than 'etc.' lists of methodologies that moreover hold only in

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[266] I agree with Giere (1985) pp.342-343 that "a fundamental distinction between rational and irrational activities is itself not an effective way to understand science, or any other human activity."

specific disciplines.[267] He criticized Mayo for not providing a more general philosophical analysis of experiments. Thus weak demarcationism has to strictly maintain its norms, as otherwise it runs the risk of the charge of relativism. I believe that an inherently more flexible approach to the concept of rationality can avoid this pitfall.[268]

Another problem with the 'errors as obstacles' approach is that errors are viewed solely in the negative. They are obstacles, and the only thing that can be done with them is removal. In this way we lose sight on the many ways in which errors can be fertile to the development of science, which will be demonstrated in section 4 below. To capture these fertile effects of errors requires a broad view of learning in science, and not just the narrow approach of selecting a particular norm of rationality. It is true that Mayo argued, contra Popper, that probing for error yields positive effects. If known procedures of testing do not find an error then we may conclude that the hypothesis in question represents reliable knowledge. When errors are found we gain precise insight in what is at fault and hence where to adjust things. Still in Mayo's approach only the inferences that can be drawn from error detection count as positive. It is not the case that errors in themselves can have positive effects on the course of science.

A common point of critique against approaches from the 'errors as obstacles' group is that they pay insufficient attention to the frameworks in which errors emerge and are detected. This can be expected from theory-dominated models of science. Popper for example made a point of the contextual blindness of his falsification procedure because of the resemblance to random generation in evolution. Yet, the critique applies also to Mayo's error statistics. She focuses on error detection and the methods by which this detection can be brought about. What we do not learn however, is how detection methods become canonised, nor in what way they are improved. We also do not learn how new hypotheses are generated as a consequence of error detection. There is no account at all of the ways in which contextual factors bear upon experimental practice.

To be fair, Mayo recognizes the problem as she writes:

"What about the work that goes into designing (or specifying) hypotheses to test or infer? Much less has been said about this, and it is a central gap to which I encourage philosophers of experiment to fill."[269]

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[267] Hon (2003a).
[268] I defend such an approach in chapter 7.
[269] Mayo (2014) p.65.

It is however not at all clear whether this gap can be filled within Mayo's framework. Her main case study on Perrin is not really exemplary because Perrin does not detect any errors. We would like to see a case in which errors are detected by the error-probing procedure and what happens afterwards. Moreover, in the Perrin case study Einstein's theory was developed as an alternative *without* first deeply probing for error. So it would seem that theory does come first here, which supports a theory-dominated view on science that Mayo is so anxious to get rid of.

Mayo also resorts to the classic 'trick' in philosophy of science, referring to the 'context of discovery' as an area that does not lend itself for philosophical analysis because it is not systematic.[270] This is an unsatisfactory answer considering the fact that only methods of research ensure reliability of knowledge claims in Mayo's framework and we basically learn in science by constantly improving on these methods. When the testing procedures are not given, but instead must be extracted from historical practice, we would like to know how that goes. More in general I believe that there *are* systematic things to say about the discovery process. This is an area in which much more work can be done.[271]

The final problem with the 'errors as obstacles' approach is that it can deal well only with prospective error. Mayo's error repertoires for example, represent a catalogue of mistakes made in the past. With a set of strategies from the repertoire in hand we can decompose experimental research in a number of canonical questions and probe these for error. But how to probe for errors that are not covered yet by the error repertoires, that are in fact not recognized at all? In what way would such errors ultimately be revealed? If the answer to this question is that this is through confrontations with other perspectives, like theories or paradigms, then either we cannot just go smaller and have to take larger units into account or the theory-dependence tradition is stronger than a New Experimentalist would grant it to be.[272] No catalogue of known errors can exclude all errors from science. There may always be new (sources of) errors that are overlooked. A complete theory of error needs to find a way to account for the occurrence of retrospective errors in past science as well.

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[270] Ibidem.

[271] Groundbreaking, but without much follow-up, is Darden (1991). In Staley (2014) systematic aspects of the discovery process of errors are explored.

[272] In both Nickelsen and Grasshoff (2009) and Weber (2009) the critique that Mayo focuses too narrowly on prospective error can also be found. Further in Chang (1997) the feeling is expressed that Kuhn's ideas of paradigm shifts get flattened too much in Mayo's theory of science.

What all approaches in the 'errors as obstacles' approach lack is a sufficiently detailed account of historical contexts bearing on all aspects of scientific investigation. The stronger the insistence on demarcation is, the bigger this problem becomes. The total range of interactions of our conceptual systems with experimental practice, and the various levels on which the phenomenon of error plays a role, has not been satisfactorily accounted for. While treating errors as obstacles has a number of advantages, securing continuity in science and providing a theory of qualitative improvement, the number of problems is so substantial that I believe it does not make sense to look for ways to repair it and keep the 'errors as obstacles' approach alive. Weak demarcationists have already tried in various ways and in my view the results have continued to be unsatisfactory. Hence we need another approach to the phenomenon of error.

3. The 'errors as failures' approach and its shortcomings

In symmetrical approaches to science the same type of factors account for all acceptance and rejection of knowledge claims. This means that a sociology of error will not suffice: correct claims to knowledge stand in need of social explanation as well. Knowledge is seen, not as (a variant of) justified true belief, but as authorized belief. Error then must be a form of unauthorized belief. In effect errors are what the community decides they are.[273] Although SSK and posthumanism differ in their symmetry-breaking mechanism, for both, decisions about error are the result of negotiation, and hence we can say these are social decisions, no matter what kind of actors are recognized in the negotiation process. The very process of negotiation can be constitutive of what it means to be correct or incorrect in a particular historical context. This often involves debates on standards of measurement and evaluation. Sometimes even whole general approaches to scientific research are at stake as in the Hobbes-Boyle controversy. In two case studies pertaining to the 19th century, one by Schlich on a controversy between Pflüger and Minkowski over the cause of diabetes, and another by Chen on a controversy within photometry, the discussion on error has also been embedded in a broader dispute over what counts as right science.[274]

In the conflict between Pflüger and Minkowski two theories offering a causal explanation of diabetes were in competition. Minkowski (the brother of the famous mathematician) defended the view that the cause of diabetes had to be

[273] Schickore (2005).
[274] Schlich (1993), Chen (2005).

sought in a dysfunction of the pancreas, which according to him contained specific regulators to control blood sugar level. The competing explanation was defended by the German physiologist Pflüger. Pflüger thought diabetes was caused by a general disorder of the nervous system and could not be attributed to the malfunction of one particular organ.

There was much at stake in the debate, especially for Pflüger. He was a leading scientist in the field of physiology, which he considered to be the central discipline of the natural sciences, because it had the human organism as its object of inquiry. He was strongly convinced that a holistic approach was the right way to study the human organism. He had had a lot of success with this approach and contributed to the understanding of the metabolic system, the regulation of body temperature by the nervous system and the relation between electrical stimulation and muscular contraction. Therefore he thought that regulation of blood sugar level was an aspect of the general functioning of the human organism too.

Minkowski on the other hand had no stake in physiology or in a holistic approach to natural phenomena. As a physician it was easier for him to challenge Pflüger's holistic explanation of diabetes. Schlich now argues that the choice for the correct theory was directly connected to a fight for authority over the respective disciplines *and* to a stance on the general approach to science in which Pflüger had invested his whole career. This explains in part the fierceness and eventual radicalization of the controversy.

The controversy was fought mainly in published papers. Next to this both scientists also sought allies. Schlich demonstrates that Pflüger made a few unfortunate choices in this respect, linking up with untrustworthy characters. There was no disagreement about what counted as evidence. Minkowski for example used vivisection. He removed the pancreas in dogs and showed they all got diabetes. Schlich's point however is that this evidence alone could never have concluded the debate in Minkowski's favour. Pflüger accepted most of Minkowski's experimental results but he explained them differently as long as he could. Only when he became isolated, and the room for disagreement became smaller, did he cave in. Therefore Schlich concludes:

> "More generally, and in keeping with sociologists of science, like Latour and Woolgar, whether a view about nature becomes a fact about nature is not determined by nature itself; it is a consequence of the settling of controversies."[275]

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[275] Schlich (1993) p.441.

He supports this conclusion with the fact that Pflüger's overall approach to natural phenomena yielded splendid results in a number of cases and failed to gain adherence in other cases. It can therefore not be said that there was something inherently correct or incorrect about Pflüger's science. From this Schlich concludes:

> "Taking into account the conclusion of the dispute as well as its origin we see that discovery is not the consequence of correct science and a mistake not the consequence of wrong science."[276]

Pflüger's general approach to science cannot be set aside as irrational, as it worked in cases other than finding the cause of diabetes. Hence rationality of the approach cannot have decided the matter in Minkowski's favour and hence an appeal to social factors, network building, etc., must be made to explain how the controversy was settled.[277]

In a similar way Chen accounts for a controversy in photometry about the measurement of the intensity of light in the early 19th century. On the one hand there was a visual approach, dependent on the eye, which rested on a principle of simultaneous comparison, first aided by a shadow photometer, then a mirror or reflective meter, and later a grease spot photometer. This approach could not be standardized very well and relied heavily on the eyes of the person conducting the experiments. The eye could get tired and tired eyes yielded unreliable results. The competing alternative was a physical approach supported by a thermometric photometer which was capable of measuring the actual intensity of light, transparencies of material, and the intensity of different colours in a prismatic spectrum, all in terms of a known parameter, namely temperature.

Chen sketches the process that led to the choice for the visual approach. He demonstrates that conflicting virtues were at stake. It was known that the physical approach was more accurate but it was much slower in use. It was for example a difficult task to distinguish the impact from non-luminous heat from that produced by the light. Manufacturing the thermometric photometers was also expensive and difficult, whereas the grease spot photometer was easily

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[276] Ibidem.

[277] I am not entirely convinced by this explanation as no reasons are sought in the paper why the holistic approach worked in some cases and not in others. Perhaps the kind of phenomena this approach can handle well is different from other phenomena, such as the cause of diabetes. If this is the case then another explanation of Pflüger's error is required, namely in terms of the application of an unfit approach to a scientific problem, presumably caused by a confused understanding of the phenomenon in question.

portable and much simpler in use. Further it delivered results more quickly and hence was more efficient. In that context the virtues of simplicity, efficiency and scope of application weighed against empirical accuracy. As in the case of the Pflüger-Minkowski controversy, the debate featured personal attacks, acts of rhetoric and seeking alliance with other authorities. The lighting industry played a major part in these debates, which in part explains why the more practical virtues won over the demand for accuracy. The practitioners found the margin of error of the visual approach acceptable.

The two case studies are clear examples of symmetrical analyses of error and truth. They offer insight in the interplay of factors that lead to the establishment of acceptance ('truth') and rejection ('error') of scientific theories, as well as preferences for scientific methods. Falsity is understood in terms of authorization, and authority can be acquired only in processes of negotiation. The practical turn in historiography of science that made historians zoom in on all the intricacies that are involved in experimental research can be very well connected to this approach to truth and error. Scientific research, very much like engineering, always sets out to reach certain goals. A scientist is like a craftsman performing skilful work.[278] When the desired goals are not obtained, something has gone wrong. The notion of error should therefore be understood in relation to these specific goals.

The view on science as a means to an end brings us to interpreting errors in terms of failure. Something can fail only with respect to a particular goal, for example the desired functionality of a technological design. From the perspective of failure, errors (and truths) are never absolute but always relative to specific demands. As functional demands shift, the perception of failure may change accordingly. What previously worked can start to fail. Petroski has explored this technological notion of error in a number of works.[279] He defines failure as an unacceptable difference between expectance and performance. As such differences will always be present, it is the engineer's habit to look at everything in the world as if it needs fixing. For Petroski it is desire, and not necessity, that drives the need for improvement.

According to Petroski failure is much more informative than success because it is only through failure that we learn and improve. Technological design is a constant process of accommodation to demands that ultimately start with the

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[278] In Smith and Schmidt (2007) it is for example argued that cognitive processes are always embodied in practicalities. Craftsmanship has to be learned through difficult processes of trial and error.
[279] His most recent books are Petroski (2006) and Petroski (2012).

demands that survival sets us. The constant interaction between successful design, failure, redesign, failure, etc., is what leads to improvement. Success masks potential failure instead of being a demonstration that failure is absent. Success can also be a burden for further development because it weighs innovation down.[280] An example is the firm Kodak, which dominated the photography market for a long time. It has now gone bankrupt because it failed to react to the digital revolution in photography. Yet Kodak itself was the first company to invent the digital camera! But the board decided not to invest in digital photography because it seemed to jeopardize on-going business. They also did not expect the digital camera to catch on, as the percentage of people owning a personal computer at the time was very low.

Prominent scholars in science studies, such as Harry Collins, Trevor Pinch and Peter Galison, have attempted to draw an analogy between technological failure and failure in science.[281] For Collins and Pinch this is a straightforward undertaking because they see no fundamental distinction between science and technology, or in other words between pure and applied science. Their aim is to capture science alive, that is, in the making. Their studies show that invariably many factors were in play when failures have occurred. Failures therefore cannot be blamed on one individual or on the malfunctioning of one artefact only. In public debate the analysis may often be narrowed down to such explanations, but in reality many layers of decision-making and operation interact and this leads to failure for example because unintended results occur or because of irresponsible risk-taking. Collins and Pinch therefore argue that we should try to understand the intricate processes of decision-making in order to avoid major failures occurring in the future.

Galison has likewise pointed out that good failure analysis is based on a multi-causal model. He asks why major technological failures are meticulously investigated whereas major failures in science are not. He discusses a peculiar case in which Fermi received the Nobel Prize for the discovery of nuclear fusion based on research that contained a number of flaws. As it later turned out Fermi never witnessed nuclear fusion but nuclear fission. The error was due to mistaken reading of the experimental data. The question is what caused this. According to Galison many factors were in play, such as relations of authority in Fermi's lab, aspects of group culture, the race to be the first to discover fusion,

[280] Note that progress is not linear in this model. A change in technological design can bring gains but at the same time also involve a loss of possibilities, similar to Kuhn's paradigm shifts. Moreover a new thing often creates problems of its own.
[281] Collins and Pinch (1998), Galison (2005).

the influence of nationalism, but also the novelty of the phenomenon. The interplay between these factors needs to be understood in order to account for the occurrence of the failure. Again it is not easy to distribute blame: only a multi-level analysis can yield understanding of errors made in such major research projects.[282]

The similarities between science and technology, and hence between error and failure, are all too obvious. Both science and technology are man-made enterprises. Both have a clear practical side. Both experimental systems and technological systems have intentional directedness. In both fields use is made of mathematics, models, predictions and simulations. In both there is also a mixture of theoretical, experimental and instrumental 'cultures'. Further, at the forefront of present-day research, such as genetic engineering or nanoscience, science and technology appear to have merged.[283]

Isolating causes to find out what is wrong is similar to isolating phenomena in an experiment and getting at the right causal connections. Consider Mayo's multi-layered model approach, which breaks down scientific research in canonical strategies with which one can probe for error. Such pro-active failure analysis can count as the hallmark of good design too. Finally both technological and scientific knowledge are never fully certain. They are both fallible, and for both it may be said that change starts to come about only when things appear to stand in need of fixing.

Still, in spite of all these similarities, I am reluctant to accept the analogy in full. My reservation is that a goal-directed analysis sometimes applies to scientific research, but sometimes it misses the mark. Often scientific research 'at the frontier' does not work towards clearly specified goals as in technological design. Researchers have to anticipate the unexpected. It is often not possible to specify errors, because it is unknown whether they are present at all. This retrospective dimension of errors in science, which could not be captured very well in the 'errors as obstacles' approach, is even more difficult to account for in the 'errors as failures' approach because this approach requires a full specification of desired goals. If aspects of phenomena can be established only after a period of time, it is not possible to fully specify these aspects and check

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[282] Nuclear fission may be the only scientific result for which two Nobel Prizes have been awarded. As Fermi already got the prize for Physics (1938) it was Seaborg who got the prize for Chemistry (1951) for "discoveries in the chemistry of the transuranium elements". A curious instance of error correction in the history of science!

[283] According to Pickstone (2005) technoscience represents a fifth way of knowing that came up in the course of the 19th century and has now become the dominant way of knowing.

for error at an earlier date. Studying errors from the perspective of failure therefore does not capture the full range of the phenomenon of error.

Kuhn's framework of paradigm alternation does not suffer from this problem. It is clear that from the perspective of a new paradigm, errors in the preceding paradigm become visible, that could not be seen as such in the older paradigm. The interesting notion in this respect is the notion of anomaly. Anomalies are not errors but phenomena that cannot be accounted for within a specific frame of reference. Anomalies do however provide an indication that something is wrong with the paradigm, but often it is not clear what this is. A shift in perspective, from one paradigm to another, is needed to account for the anomaly and locate what was at fault with the preceding paradigm. An interesting aspect of the phenomenon of error is addressed here, namely the situation in which there is growing uncertainty about a theory, but it is not (yet) possible to tell exactly what is wrong. While I do not think that the Kuhnian approach as a whole offers a satisfactory analysis of past science, this particular aspect touches upon a dimension of the phenomenon of error that a good theory of change in science must be able to capture.

It is a bit difficult to place Kuhn in either of the two major approaches to the phenomenon of error. As I pointed out in chapter 2, a flavour of meta-methodology is kept alive in his model of science. Institutionalized critique is restricted to periods of normal science and fundamental critique to phases of revolutionary science. In his later work Kuhn weakened the incommensurability thesis and argued that comparison between theories in different paradigms was possible on a number of virtues such as simplicity, accuracy and consistency. Further, he suggested that a measure of progress could be problem-solving capacity. Yet, virtues for Kuhn do not determine truth and error. And problems, as well as their solutions, always have to be related to the specific contexts in which problems occur. Even in the more moderate versions of Kuhn's model the notion of error remains tied to paradigm-specific standards and this fits the general perspective of 'errors as failures' more closely than 'errors as obstacles'.

The 'errors as failures' approach has a number of good points. The dichotomy between social and rational explanation of past science, which we have found unworkable, is given up. There is no lack of attention to historical context. On the contrary: error and truth are seen as outcomes of processes of negotiation and a variety of factors is recognized to operate on these processes. Knowledge claims that are rejected have failed to gain support. Failure analysis, inspired by technological design, appears to work for the study of past science, but only when prospective error is at stake. Finally, what counts as an error is fully dependent on the frame of reference in which the error occurs. I believe that there is an important lesson to draw from this insight, namely that there is no such thing as a pure error. What an error is, is always dependent on something else, whether this is a standard of judgement, a social process of negotiation or some desired functionality. This is not incompatible with approaches insisting on the operation of a meta-methodology in science, as judgements of error do not stand alone, but have to be related to meta-methodological standards. Still, the idea that what counts as an error always depends on a particular frame of reference can be captured better in all its breadth and depth in the 'errors as failures' approach.

The 'errors as failures' approach however suffers from a number of shortcomings. The first problem is that theory appraisal has fully shifted to historical context. In this way a comparative evaluation of successive theories over longer periods of time cannot be carried out and this makes it difficult to speak of qualitative change in science. Following Kuhn, the connection between successive theories, or successive reference frames, is brought about through anomalies, which indicate that something is wrong. To work with the notion of retrospective error in history of science requires the assumption of some form of continuity. It is not evident how to capture this dimension of science if error is studied only in terms of failure. Even in Kuhn's framework paradigmatic changes are so drastic, that possibilities for comparative evaluation become dismal. This problem can be put in other terms. Within the parameters of a particular context it is possible to improve and learn from failure. Yet in another context, other demands may be set on science and technology. It is not evident that what was learned can be of use in the new context. No good theory of learning in science therefore emerges from this approach.

Secondly, the 'errors as failures' approach requires full specification of all the relevant factors operative in a historical context, because otherwise the function of knowledge and the processes of negotiation cannot be fully understood. This invites four forms of critique. First, it can be difficult to establish from a later perspective what all the relevant factors were, because we have to rely on the sources. Second, it is highly doubtful that all factors of interest are always consciously present to past actors. When symmetrists do not follow science forward only and make use of present-day insights from the social sciences in order to gain a better understanding of the past historical context (improving on the historical actors), it is not clear why this is allowed for the social sciences, and not with respect to later insights into natural phenomena, from which we can reasonably assume that historical actors also grappled with.[284] Third, the exclusion of hindsight may actually impair understanding of a particular context. If we know where past actors went wrong in light of later developments, we gain clues in understanding their behaviour.[285] Fourth, historical contexts are never isolated. What counts as relevant in a context is often also a function of its relation to others contexts, both temporally and geographically. If we cannot expand our platform of research, it is not possible to acquire a good understanding of the workings of individual contexts, yet error analysis in terms of failure heavily relies on this information.

To sum up, the 'errors as failures' approach has a number of shortcomings that all flow from the narrow contextual focus it imposes on the study of error. This makes it hard to account for qualitative change in science. Only part of the phenomenon of error, namely prospective error, is therefore addressed. Retrospective error requires some form of comparison between historical contexts. The challenge is to specify how subsequent frames of reference relate to each other and what acceptable forms of comparison can be. Sophisticated comparison between then and now, or earlier and later, may not necessarily lead to passing unfair judgements on past actors. What it should bring about is an understanding of the room for improvement in a particular historical context and how this was used to make the next step in the development of science. The 'errors as failures' approach needs to be seriously modified to be able to incorporate these dimensions of the phenomenon of error.

4. Towards a proper theory of error

4.1 Philosophy of experiment

In order to capture the full scale of interaction between conceptual systems and experimental research, the study of errors must be made part of a broad philosophy of experiment. For Hon, error is the key to a philosophy of experiment:

284 See also chapter 2 for this criticism against the strong programme.
285 This argument was also given in chapters 1 and 2.

"I seek generalizations of the experimental activity that emerge through a study of the notion of experimental error. I claim that while capturing the nature of experimental activity, the notion of experimental error also reflects, albeit negatively, central conceptual features of experiment."[286]

According to Hon, philosophy is lagging behind history and sociology in studying scientific experimentation. These fields have addressed many more facets of science including technological, cultural, sociological and anthropological dimensions. He asserts, "in the case of error, analysis cannot remain on an abstract, general level; it has to address also the material situation and its current knowledge, which is in a word–history"[287] This is an interesting observation. The turn towards a broad philosophy of experiment neatly fits in with the practical and material turn in the historiography of science. It is also in the spirit of New Experimentalism. What Hon offers is actually a plea to coordinate efforts in science studies in order to come to terms with the phenomenon of error.[288] Within this envisaged cooperation it is a philosopher's task to come up with a proper theory of experiment that covers the general and systematic aspects of experimental practice.

Hon's own proposal for such a theory involves setting up a typology of errors that reflects the cycle of research. The typology consists of the following levels: 1.background theory, 2.assumptions in the actual set-up of the experiment and the working of the devices, 3.observational reports, and 4.theoretical conclusions.[289] The first two make up the preparation stages of research, the second two the test stages.[290] Hon suggested replacing Bacon's Idols with four new sources of error that can be associated with the four stages of research.[291]

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[286] Hon (2003a) p.176.
[287] Hon (2003b) pp.254-255.
[288] In Allchin (2006) the same point is made.
[289] Hon (1989), repeated in Hon (2009). Less discriminative classifications are Roth (2003) and Buchwald and Franklin (2005). Buchwald makes a distinction between errors in practice and errors in understanding. Roth distinguishes between a ground level and two higher levels of error. All this can be subsumed under Hon's approach.
[290] Hon appears to exclude here from the concept of error, mistakes in calculation or logical reasoning. This is in line with two others papers of his in which he makes a terminological distinction between error and mistake (Hon 1995) and Hon (2004). A mistake for Hon is something that could have been avoided whereas an error is something that could have not been avoided. The concept of error then is confined to retrospective error only. This surely brings the point home of the bias towards prospective error in HPS but since the terminology has not caught on I have decided to be a bit more liberal in my use of the terms error and mistake.
[291] Hon (2003a).

He called these the Idols of the Script, pertaining to the background theory, the Stage, pertaining to assumptions concerning the apparatus, the Spectator, pertaining to the position of the researcher, who has to measure and observe and finally the Moral, pertaining to interpretation and theoretical conclusions. We can see that both theoretical and practical levels are present, but also that the researcher is included as a possible source of error.[292]

On all these levels errors can occur and errors occurring at different levels have different epistemic effects. In Hon's typology the conceptual level is present in phases one and four. In phase one, concepts generate expectations, whereas in phase four new concepts emerge. Hon's idea is to construct from the four categories a material argument that covers the transitions from matter to proposition. Like Mayo he argued that we can justify the reliability of hypotheses by carefully checking for errors at all stages of research. He was however dissatisfied with Mayo's error theory, because her theory yields lists of ad hoc strategies that differ by discipline. His aim is to transcend such 'etc.' lists.[293] The question is whether a general view can be extracted from the myriad of strategies, procedures, conceptions, styles, methods, etc., used in actual practice.

With this question Hon asks for meta-methodological precepts. The solution he offers is to focus on all possible sources of error. It is far more useful to get a good grasp of what the sources of errors are, instead of focusing on the detection of errors. Once we learn more about the sources, the problem of detection is much easier to solve. This sounds like a good idea but defining a typology alone does not seem to be enough to make it work. Hon is clearly looking for stronger normativity than Mayo's normative naturalism has to offer. Yet how this demand for more normativity is squared with the required increase in attention to historical context is not made clear in the publications Hon has devoted to the subject. To investigate the sources of error more deeply, I believe we need to make a shift and consider science from the perspective of uncertainty (see the next section).

Another error typology, proposed in Allchin (2001), claims to connect the normative and contextual dimensions of experimental research. Allchin's typology, which he calls 'error analytics', is meant as an expansion of Mayo's error statistics. Like Hon, Allchin was motivated to say more systematic things

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[292] Hon argued that his approach is more discriminative than either Pickering, who distinguished between material procedures, an instrumental model and a phenomenal model or Hacking, who used the categories ideas, things and marks to think about experimental science. These notions can all be allocated to Hon's four categories.
[293] Hon is here arguing also against Franklin (1989).

about experimental research than Mayo's framework allows for. His typology consists of four categories of error: material, observational, conceptual and discursive. Material errors are caused by use of improper materials, following improper procedures (lack of skill) and/or perturbation of the phenomenon by the observer. Observational errors include insufficient control to establish the domain of data, errors caused by incomplete theory of observation or by the observer's perceptual bias.[294] Conceptual mistakes consist of flaws in reasoning, inappropriate application of statistical models, inappropriate specification of models from theory, misspecified assumptions, wrong theoretical generalizations and theory-based cognitive bias.[295] Discourse errors are communication failures, mistaken credibility judgements (forms of trust based on authority), unchecked socio-cultural biases, and even public misunderstanding of science through poor science education or poor science journalism is included on this level.

The differences between Hon's and Allchin's classifications are interesting. Allchin focuses on the epistemic structure of scientific research and not directly on the cycle of the scientific method as Hon had done. Allchin's errors types cover a spectrum that ranges from relatively local to relatively global errors. According to him this reflects the layers of transformation from the original world to scientific theory. We move from the apparatus via the individual observer and the book to the community, but this does not necessarily have to reflect the chronology of scientific investigation.

A second difference with Hon is the inclusion of the category of discourse errors. The errors of this category are of a purely social nature and according to Allchin the most global. Allchin writes that if we assume that the four layers are present in the construction of any scientific claim we can integrate philosophical and sociological analysis of error:

"Another hallmark of the framework of error types is showing how one can interpret fact and error in science according to the same concepts. That is, it takes seriously the Strong Programme's principle of symmetry. But the solution, here, is not to adopt an exclusively

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[294] He provides the example here of an error in research on 'mesosomes', i.e. structures discovered in electron microscopy that were seen as facts but are now seen as artefacts of the experimentation process (Allchin 2000b).

[295] A clear example of this is given by the investigations into the causes of beriberi. The Dutch professor Eijkman long worked on the mistaken assumption that beriberi was caused by the presence of some food ingredient. It took a long time to get rid of this basic assumption and allow the idea that a disease can also be caused by withholding food ingredients as Gerrit Grijns proposed. Only with the modern vitamin theory could all this be explicated in a satisfactory manner.

sociological perspective. Rather, the typology of error embraces how philosophers and sociologists each describe certain errors, along with their complementary facts. Errors have many sources, some experimental, some conceptual and some cultural. By symmetry, each parallel fact (that is free from the given error) relies on the very same experimental, conceptual and cultural factors. Again, philosophical and sociological factors fit a common framework." [296]

This analysis strikes me as being far too superficial. The idea that one has to interpret error and fact according to the same concepts is interesting and I will support it in section 4.2, but it cannot be reconciled with symmetry in the way Allchin suggests here. Symmetrists would always insist that the fourth category is the overriding factor that is responsible for all decisions on fact and error in a given community. Approaching determining factors of science from a less hierarchical perspective requires a reconsideration of the principle of symmetry, as we have carried out with the proposed relationalism in the final section of chapter 2. Like Hon, Allchin does not bite the real bullet of integrating the normative and descriptive dimensions into one proper theory of scientific change. Where Hon's analysis is incomplete, Allchin's is simply incoherent.

4.2 Science from the perspective of uncertainty

4.2.1. From removing obstacles to removing uncertainty

In the previous sections we have discussed a group of approaches that see science as driven primarily by the wish to get rid of errors. The proposal I want to make is to change this, and consider science as an endeavour, not primarily driven by the wish to remove errors, but by the wish to decrease uncertainty. As mentioned in chapter 1, Daston once remarked that 'all epistemology is born in fear'. The fundamental epistemological 'fear' is not to be wrong but to be uncertain. As Claude Bernard said 'it is the vague, the unknown, which moves the world'.[297] What counts as truth and error crystallizes in the process of decreasing uncertainty. Truth and error grow up together, so to speak.[298]

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[296] Allchin (2001) p.57.

[297] Rheinberger (2009) p. 84. Note that error and uncertainty are not always clearly distinguished. Uncertainty can be seen as confusion, which is often conflated with error: Knorr-Cetina (1999) p.276-277.

[298] Note that I use the terms truth and error in an unqualified sense for ease of exposition. As claims of truth and error always rest on time-bound frames of reference I do not assume that the qualifications are permanently fixed.

While it is not a very common idea to recognize uncertainty as the primitive state in scientific development it is nonetheless present in the literature. The idea can be traced back to Mach's *Knowledge and Error* (1905). Mach argued that the search for knowledge is a process of forming associations. Some associations turn out to be correct and others turn out to be wrong, but this happens in one and the same process.[299] In a similar vein Douglas Allchin has proposed to make a fundamental epistemological shift to connect the possession of knowledge not to truth and justification, but to the notion of explicability.[300] Knowledge in his view does contrasts not with error but with uncertainty. Allchin pictures the intended shift as follows:

Positive knowledge or fact, truth	Negative knowledge or artefact, falsity

Conventional distinction

Knowledge of both fact and artefact (resolved)
Uncertainty (unresolved)

Revised distinction

Allchin argues that negative and positive knowledge involve the same procedures of justification. Establishing where the errors are, and what they precisely are, requires the same work as finding support for hypotheses, that is, gaining evidence, testing scenarios, etc. According to Allchin philosophers, sociologists and historians of science should study the strategies that allow researchers to isolate, identify and remedy error.[301] Further, one can investigate how scientists have used the knowledge of error, and how they developed catalogues of past mistakes, cf. Mayo's notion of the error repertoire. This may also involve the characterization of canonical errors, or general error types.

I believe that Allchin's main idea, that science proceeds from certainty to uncertainty, deserves support. It is however tidier to keep the notions knowledge and error separate. Hon for example has argued that understanding

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[299] For example he wrote: "Ob die Reaktion Nutzen oder Schaden bringt, ob insbesondere biologisch fördernde oder irreleitende Vorstellungen sich einfinden, in beiden Fällen liegen dieselben physischen und psychischen Vorgänge zu Grund." Mach (1976) p.109.

[300] Allchin (2000b).

[301] For these strategies Allchin refers to the interesting work presented in Darden (1991) and Bechtel and Richardson (1993).

an error is not knowledge in itself, but proven to be, false claims to knowledge, or an indication that grounds to the knowledge claim could not be provided.[302] It is not apparent why we would need a concept of negative knowledge in order to make the shift to the perspective of uncertainty work. As this seems only to increase confusion, it can better be avoided.

The shift to the perception of science as driven by the wish to decrease uncertainty is like occupying a superposition: a sublime strategy that avoids a troublesome dichotomy. The idea is that from the superposition the dichotomy between demarcationism ('errors as obstacles') and symmetrism ('errors as failures') can be overcome. With the shift to the perspective of uncertainty, the primary thing in science is to reduce uncertainty. The correction of errors can still be part of this, but now as one way, among others, in which uncertainty can be decreased. Removing errors is no longer taken as the primary driving force of scientific development and also not the sole way in which progress can be achieved.

4.2.2 From a quantitative to a qualitative approach to uncertainty

That a degree of uncertainty plays an important role in science has long been recognized and led, at the beginning of the 1980s, to a 'probabilistic turn' in philosophy of science. This turn was cast in the language of indeterminism, chance and bias. Philosophers have attempted to quantify decision-making in the context of uncertainty, through models of probabilistic reasoning. Good examples of this approach are Bayesianism and Mayo's error statistics. Statistical notions of error were also developed, like type-1 error (rejecting a correct null-hypothesis) and type-2 error (failing to reject an incorrect null hypothesis) or the distinction between systematic and random error. Probabilities are attached to theories, evidence, or in Mayo's case to methods, and with these probabilities in hand one can calculate what the optimal decisions are. With absolute certainty no longer within reach, the aim became to get at exact measurements of the degree of uncertainty and thereby retain analytical control over scientific decision-making.[303]

302 Hon et al. (2009) p.1, also Hon (2009) p. 21.

303 See Halpern (2005) and Lindley (2006). Heisenberg's Uncertainty Principle may have played an important role in the attitude of retaining statistical control. The principle states that position and momentum of a particle cannot be known at the same time. Yet the degree of uncertainty (the chance that a particle will be at a particular place after some time elapsed) can be calculated precisely. See also Prigogine (1996) for the impact

In my view, the process of theory change in science cannot be captured in quantitative terms only because the estimation of probabilities will necessarily have to be grounded on qualitative decisions. I propose to study uncertainty primarily in qualitative terms. This involves the recognition that uncertainty is first and foremost an aspect of persons. In the probabilistic approaches estimations of the degree of uncertainty are attached to theories (or in Mayo's special case to methods) and in terms of degree of determination, which is a mind-independent feature of scientific theories. Theories about the world can be said to be underdetermined, indeterminate, or probable, but not uncertain. Only persons can be in a state of uncertainty, which provokes doubt, anxiety, uneasiness, etc. Underdetermination of theory choice by empirical evidence is no more than one of the causes of uncertainty in scientists. Uncertainty can, for example, arise also through an incomplete state of the art of knowledge in a given field, because of an incompatibility between theories or incoherence in one theory, due to overwhelming complexity, due to confrontation with something new, or due to not knowing exactly in what direction one has to look for when it is also uncertain how trustworthy or useful the data are that have been collected.

It is thus very well possible to feel uncertain about something without being able to state exactly the level of indeterminacy of a theory. (Un)certainty therefore should not be equated with (in)determination. The objects of certainty (its ontological status) are the beliefs of scientist. The difference between certainty and uncertainty is one of degree and not of kind. Epistemological uncertainty is attached to the state of a believer, that is, the degree to which the believer doubts his/her belief. Complete certainty can be interpreted as a form of extreme order, which is not often achieved.[304]

However, in science one is never certain or uncertain in the abstract. The degree of uncertainty may not be measurable in exact terms, but it is given by the problem situation, the state of the art of knowledge, available evidence, the number of competing hypotheses, the availability of conceptual and technological resources, the role of dominant assumptions, the role of particular values, etc. In short it is a function of concrete historical circumstances, which historians have to carve out precisely.

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of uncertainty at the level of fundamental particles on the understanding of *all* scientific knowledge.

[304] I benefit here from remarks made in an online article 'The Philosophy of Medicine' accessible via: http://www.123helpme.com/view.asp?id=35681.

An important aspect of this set of historical conditions is the mode of tolerance for errors. This differs from period to period, cf. Daston (2005), who suggested that the main perspective on errors has changed significantly a number of times throughout the 17th, 18th and 19th centuries.[305] Tolerance for error can also differ in more specific ways, and for example be expressed in accepted error margins in specific fields of research. As this generally happens in mathematical terms we can see that quantitative analysis is still useful to study scientific decision-making. An estimation of the degree of underdetermination of a theory can for example provide an indication of the direction in which more evidence must be sought. The suggested shift in thinking about science in terms of uncertainty first should be viewed as an extension of the discourse of the probabilistic turn in philosophy of science, and not in terms of full opposition to it.[306]

The analysis of the causal complexity of concrete historical situations to specify the degree of uncertainty has to grant a central role to the motivations, attitudes, intentions and actions of scientists in the decision-making process. Historiography of science has however decidedly missed a 'psychological turn'. In the formalist tradition there is a general attitude of anti-psychologism. Psychology offers causal explanations and this naturalist approach does not sit well with the formal approach. On the naturalist side science studies scholars have not been keen either to include psychological explanations in their accounts of past science.[307] The reason for this is that the inclusion of cognitive factors seems to turn rationality into a special category again, and this had to be avoided at all costs. For this reason the notion of agency, which plays such a prominent role in posthumanism, is at the same time seriously underanalysed.

In cognitive psychology interesting studies have appeared in, roughly the past decade, on both the topic of error and decision-making under uncertainty.[308] Gigerenzer (2005) is especially relevant to the study of errors in past science.

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[305] Of interest in this respect is also Hon (2004) who argued that knowledge about the conception of error of past actors can be revealing for the understanding of their general approach to science. He offers a comparison between Kepler and Galileo. Kepler recognized both errors which could not be foreseen and mistakes, whereas for Galileo there could only be mistakes, i.e. for Galileo all errors could in principle be foreseen.
[306] Note that the new and old senses of uncertainty in science are also explored in Boumans, Hon and Petersen (2014).
[307] A notable exception is the cognitive approach defended in Giere (1988). In Kuukkanen (2008) interesting support from cognitive science is presented for Kuhn's idea of concept learning through similarity relations.
[308] Books include Sorrentino and Roney (1999), Gilovich, Griffin and Kahneman (2002), Kagan (2002), Gigerenzer (2008) and Zimmerman (2008).

He believes that we must assume that our cognitive capacities have not changed much in the past few hundred years. Therefore, present-day insights in the workings of our reasoning faculties can be of aid to historical understanding of past actors. Gigerenzer argues that an important source of errors stems from the human need to order the world. Ordering and classification leads to generalizations and over-application of rules. We can see this in common mistakes in perception due to context-sensitive reasoning. An intelligent system adds or subtracts information where in a number of cases it should not do this. The ability to order however represents a huge advantage over persons (or other species) who do not have this ability, because it yields the possibility of interpretation. There have been tests with persons suffering from a disorder in the central nervous system. There is a story of such a patient who was able to replay a piano sonata of a professional musician, *including* all the mistakes the musician had made. The patient just copied with phenomenal accuracy every detail he had heard. However, he could do nothing with this in terms of interpretation, as it was impossible for him to deviate in any way from what he heard. The ability to interpret and classify the world represents a huge advantage. When errors of the type of excessive generalization occur this is, according to Gigerenzer, a sign that an intelligent system is at work. The presence of errors is not incompatible with rationality. Historians of science can use this when they consider the fertility of errors in past science, a topic that will be dealt with in the next section. Publications from cognitive psychology on reasoning with uncertainty help to find an interpretation of rationality so that rational factors can play a role in historical explanations of past science, without turning them into a special category.

4.3. Benefits of the uncertainty perspective for the study of past science

Studying science from the perspective of uncertainty is a sublime strategy that harbours a number of benefits for the study of past science. I signal three main benefits and indicate how historians of science can put these to use in future research, thereby granting a proper place to the notion of error in historiography of science.

First, the shift in perspective makes it possible to account for retrospective error. This can be made clear by looking at the notion of 'going amiss', which was introduced in Hon et al. (2009). 'Going amiss' is an *a posteriori* characterization of overall conceptual frameworks or overall historical situations. The notion of 'going amiss' pertains to situations in the past in which people felt something was not right but they were not in a position to tell what it was. Only from a later point of view it became possible to indicate where the errors in the past situation should be located. The 'going amiss' notion allows us to treat every past participant in a scientific debate with the utmost respect because even though his or her contribution to this debate may have been false on later grounds, at the time itself it was respectable enough to defend. Yet the notion does not force us to be neutral on all contributions to past science.

The case study provided by Schickore, on the debates over the structure of nerve tissue in the first half of the 19th century, provides a very good illustration of how this analysis of past science might work.[309] The issue at stake was how the globule hypothesis, that is the idea that the structure of nerve tissue looks like a string of beads, had to be interpreted. Earlier historiography had it that there was only one theory in support of the globule hypothesis, that the theory as well as the hypothesis were mistaken and that it required both technological progress and theoretical improvement to reveal the true structure of the nerve tissue, which consists of cells. The removal of false belief then led to the correct cell theory. In this view the mistaken globule hypothesis had in no way been productive to the development of science.

Schickore complains that in this account we do not learn anything about the reasons for defending the globule hypothesis nor do we learn how the transition from one theory to another came about. She is however equally dissatisfied with the alternative account offered by Pickstone, in which the error disappears completely. According to Pickstone the globule hypothesis was completely understandable, given the patterns of thought at the time, and we can judge between right and wrong only following standards of appraisal operative in the historical context.

We are thus offered the choice of using the concept of error either from a presentist standpoint or from the actor's point of view. In the first case we learn nothing about the historical development of projects of enquiry, in the second case we cannot be appraisive anymore of past belief. Both these options are unattractive. Both interpretations skip over the fact that there was great uncertainty about the fact of the matter. Seen from the present the globule hypothesis is clearly mistaken, in the past it was clearly a rational way of thinking about the structure of nerve tissue.

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[309] Schickore (2009). The 2009 volume contains more very good contributions but Schickore offers the clearest demonstration of how the analytical notion of going amiss works.

Schickore opens up an alternative view by making clear that the uncertainty about the globule hypothesis in the past was great and has to be taken as a starting point of analysis. According to her, a plethora of conflicting accounts over the structure of nerve tissue was available, including a multitude of interpretations of the globule hypothesis itself. There was disagreement on both a theoretical and a methodological level. The debates reached a first settlement, which led to a rejection of a particular interpretation of the globule hypothesis as erroneous by the participants. Yet many other obscurities remained after this was settled. Schickore shows that a variety of partial agreements and disagreements among the participants could be witnessed. She correctly argues that we need a new concept in order to do justice to the participants, and at the same time retain an evaluative stance, which she introduces as follows:

"The conceptual advantage of the notion 'something is going amiss' is that its application does not require us to identify concrete claims as 'correct' and others as 'erroneous'. Using this term we are not committed to making specific appraisive judgements about particular claims to knowledge. Instead, we appraise whole sets of observations in terms of 'coherence/incoherence' or whole sets of practices (as they are described in the scientists' writings) in terms of 'uniformity/diversity' or 'stability/instability'."[310]

In other words the claim is that more general frames of reference can be comparatively evaluated with respect to a number of virtues.[311] What is needed in order to carry out such comparisons is the assumption of agreement between past and present on the level of cognitive attitudes. We may for example assume that when scientists are confronted with uncertainty they attempt to resolve it. For Schickore the application of the notion of 'going amiss' "…reflects our tacit assumption that observations of similar objects under similar circumstances should yield similar results," and "…we attribute to the practitioners the same assumption."[312] Note that seeking agreement between past and present, not on the level of direct claims to knowledge, but on the more general cognitive level is exactly what was defended in the previous chapter on the principle of charity.

Gavroglu has likewise pointed out that 'going amiss' is an anachronistic notion because it is attributed to research programmes *ex post facto*.

"The study of going amiss provides us with the possibility to make comparisons with those cases which got it right. A comparative approach will accentuate the

[310] Schickore (2009) p.39.

[311] As already indicated I support the virtue approach and articulate my interpretation of it in chapter 7.

[312] Schickore (2009) p.40-41.

methodological differences in the relative research programmes, but will also help to reveal underlying assumptions and commitments to different theoretical schemata."[313]

Theoretical or experimental practices are necessarily constrained by frames of reference. Such frames, Gavroglu argues, are conducive to 'going amiss'. We need to discover how and why, and according to Gavroglu the way to do this is through a comparative approach. Only in the context of restrictions and limitations can we come up with meaningful theories. These can be wrong, but we need to keep an open mind to the possibility that the mistaken theory has had productive effects. We need to study the ways in which errors can be fertile.[314] Wolfgang Pauli famously commented on a paper of one of his students: 'It is not even wrong!' and this is indeed a heavier verdict than to be wrong, when reasons for falsity are clear enough.

Assessments on the level of whole frameworks are of a gradual nature and can be established only in comparison. It follows that scientific controversies need to be studied *in the longer run* and we should not ask for closure at too early a stage.[315] When science is seen as a process of gradual decrease of uncertainty we can make sense of the fact that in a historical situation, at the beginning, or during later stages of the process, the errors could not yet be specified clearly, or could not be identified at all. This is possible only with hindsight. The going amiss notion fits this shift to a more diachronic focus on past science very well. The question historians need to answer is how uncertainty was gradually resolved in a given period of time. This approach has mildly anachronistic aspects but in my view in a constructive way.[316]

When placed in a longer-term development we can provide a more fine-grained analysis of scientific controversies. As Schickore's case study shows, controversies are better studied as intricate forms of agreement and disagreement instead of full oppositions. We obtain an alternative evaluative approach by moving one step away from the direct appreciation of past theories to the actors themselves and their cognitive attitudes. This makes it possible to study controversies without the need to work with full oppositions between contestants and without the need to ask for full closure of the debates at too

[313] Gavroglu (2009) p.139.
[314] Types of fertile errors are discussed in short below.
[315] I am aware that these statements cry out for an articulation of standards of comparison: see chapters 6 and 7.
[316] See also chapter 7 for a discussion of anachronisms.

early a stage.[317] This is the second main benefit that studying the past from the perspective of uncertainty brings.

A third advantage of the uncertainty perspective is that it allows for a 'wide' theory of learning that is far less restrictive than those based on transcendent norms of rationality. As uncertainty can be caused in multiple ways there are also multiple ways in which it can be decreased. This can include productive or fertile effects of error and can also take into account the productive aspects of confrontations between different patterns of thought. Moving one step away from the direct evaluation of theories creates the space of manoeuvring that is required in order to achieve this. As this extra room for manoeuvre has been won only through the shift towards the perspective of uncertainty, this shift appears to be indispensable for any sophisticated form of evaluative historiography.

An important way of learning is one that comes about through confrontation. It would be a mistake to view science as a purely self-propelling phenomenon, in the sense that a force from within constantly drives change. Of course existing problems, known errors, pure curiosity or even the urge to test limits of understanding press scientists forward. Yet change can be brought about also through confrontation. It has very often happened that people coming from one scientific discipline have forced breakthroughs in another. An example is the participation of physicists, most notably Crick, in molecular biological research that led to the discovery of the DNA structure. Another example, discussed above, is the physician Minkowski, whose pancreas theory shed light on problems with which physiologists were occupied. Breakthroughs are also often realized by people working in the periphery of a field or young persons who are new to the field.[318] This is not surprising as all three are cases in which the one who achieves the breakthrough does not have vested interests, which established scientists have. He or she does not occupy an important position, does not have to defend social status and authority and has not committed him- or herself to specific approaches or claims to knowledge. The 'outsider' is also not caught in the written and unwritten rules of the profession. Coming from the outside provides the, perhaps necessary, space to differ radically, to take risks and to propose something new

More specifically on topic, it is often possible to locate what is at fault, or to characterize an error in full, only from a new perspective. In the example cited

[317] This too I connect to the virtue approach in chapter 7.

[318] In physics a great number of significant contributions were realized by participants in their early twenties, which is identified by the term 'Knabenphysik'.

above, of Eijkman and the cause of beriberi it was not clear to anyone what precisely was at fault (if anything) until Grijns introduced the new way of thinking in which the absence of substances, such as vitamins, could be seen as the cause of the disease. Only through this new perspective could errors in the existing explanation be made clear. This notion of retrospective error also manifests itself in the many theories in the past that were accepted for a long time, but eventually came to be seen as erroneous. Examples are the phlogiston theory, the ether theory, the particle view of light, humourism in medicine, circle conceptions in planetary astronomy, the theory of spontaneous generation, the caloric theory of heat and Newtonian mechanics. Reasons for change in appreciation have to be sought in the confrontation with a new perspective, which in earlier periods could not, or did not, come about.[319] It is important to provide a place for such changes in opinion, which cannot be explained with reference to existing problems, anomalies or recognized errors alone, but can also come about through the very confrontation with other perspectives.

Lets now turn to a consideration of the notion of the fertile error.[320] These errors can be identified as 'good' errors, whereas errors that really only hamper further development are the 'bad' errors. There are at least six ways in which errors can be said to be good. The first is the opening up of a new vista that exerted attraction at the time, but later turned out to be wrong. The positive effect of this can be that the new vista helps to overthrow a restraining paradigm that hampers scientific development. An example is the corpuscularian world-view set forth by Descartes as an alternative to Aristotelian teleological metaphysics. Although nobody accepts Descartes' view any longer, the shift can be valued positively because it made possible considering the world in quantitative, instead of qualitative terms, which spurred research in many areas of investigation.[321]

A second way in which errors can be fertile is through the idea that we do not know what the error is until we can fully identify it, possibly even through consciously producing the 'erroneous' effect. This may require hard work but it

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[319] Note that appreciation can also shift the other direction, from rejection to later acceptance, presumably for the same reason. McAllister (1986) provides the examples of Huygens's wave theory of light, Thompson's vibration theory of heat, Polanyi's theory of absorption and Bohr's principle of complementarity. Wegener's theory of continental drift and Darwin's theory of evolution can be added to this list.
[320] I first came across the notion of fertile error in an issue of *Social Research* (2005) devoted to the subject. The first to use the term was probably Wimsatt (1987).
[321] See for example Jorink (2008).

can be rewarding, especially in cases in which the phenomena themselves are unclear. The ability to localize errors exactly yields elements of knowledge in itself.[322] The slogan 'To err is science' is applicable here, in the sense that to know precisely how one has erred is science.[323]

Third, errors can be psychologically instructive and tell us something about our cognitive faculties. Think for example about Gigerenzer's study of overgeneralization. Fourth, a positive effect of errors can be the 'sleepwalking effect'. Following this metaphor, scientists wander around, half-conscious or unconscious about their movements. The path they traverse is full of mistaken inferences but nonetheless they make it to the right place. They are right, but for the wrong reasons. Some of Einstein's errors can be interpreted in this way (Ohanian 2008). Another example is Dalton's double misreading of Newton, which brought him to formulate the atomic theory (Rocke 2005). And Oersted misread Kant's theory of forces and this made him postulate the electromagnetic force (Shanahan 1989).

It is counterproductive to work with a strongly normative theory of rationality that excludes these examples from the proper realm of science, because the unsound reasoning would be detected and consequently the theory would have to be rejected. It won't help to treat these examples as demonstrations of the messy practice of the context of discovery because when testing the theories in the context of justification one has to focus on the arguments in support of these theories. The net of appreciation must be cast wide to allow for a process of gradual understanding in which the faulty supportive arguments came to be replaced by more cogent ones.

Fifth, errors are often caused by bias. Yet these limitations, which can also be deliberately brought about, for example through deliberate simplification, can be productive. Wimsatt (1987) has indicated how models that are wrong can nonetheless be fruitful. Wrong models may serve as a starting point for a series of more complex models. They may suggest new tests or the refinement of established models. They may serve as templates that account for large-scale effects or that make smaller effects noticeable. They may serve as limiting cases that are true under certain conditions, which may lead to the recognition of causal factors. They may define extreme cases between which other cases lie. They may provide a simple arena for determining the properties of a system. Finally, wrong models may serve as counterexamples. Wimsatt argues that, in

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322 Examples are Schickore (2009) on the globule hypothesis, Elliot (2004) on hormesis and Allchin (2001) on the flowering hormone.

323 Allchin (2001).

general, false models give rise to anomalous results that stimulate the development of more sophisticated models and theories. Knowledge that something is wrong stimulates research in new directions, which is another positive effect errors can have.

Sixth, there is the mirror effect. If what is wrong is known, the opposite must be correct. We can capture this with the term ‘turning is learning’.[324] In some cases we do not know what a thing really is if we are unable to give an account of its opposite. This however is not always the case, as truth and error do not have to be exact opposites of each other.

The ways in which errors can be said to be fruitful provide an important argument in favour of a more plural theory of learning. Together with the notion of going amiss, the ways controversies and confrontations can be studied from the perspective of uncertainty, and the typologies given by full philosophies of experiment, the fertile error is a concept that historians of science should take seriously. I have indicated a number of promising research directions a sophisticated evaluative historiography of science has to offer. It is in these directions that a proper theory of change, and hence a proper theory of error, must be sought.

5. Conclusion

The two main approaches to the phenomenon of error suffer from serious shortcomings. Both do not capture the full range of the interaction between our conceptual systems and experimental practice. The focus is either too much on theoretical discourse or on the practical side of science. In the ‘errors as obstacles’ approach insufficient attention is paid to the features of historical contexts, while in the ‘errors as failures’ approach the focus of research is too much on the particulars of historical contexts. In the ‘errors as obstacles’ approach we learn about right and wrong only through some norm of rationality and this vision is too narrow. We found that this even holds for the weak demarcation programmes. But in the ‘errors as failures’ approach learning about errors is relevant only to particular frames of reference, which also leads to a narrow view on learning in science.

Both approaches have difficulty to account for the phenomenon of retrospective error. In both the notion of the fertile error cannot be well accounted for either. Both appear to lack a good story when sudden shifts in perception occur and how to understand confrontations between different

[324] Hon (1995) uses the Latin expression *contrariorum eadem est scientia*.

patterns of thought. These similarities are surprising. They point to hidden common assumptions that have been left untouched when naturalists voiced their critiques of the formalist study of science. In the next chapter these common assumptions will be brought to the surface.

From the perspective of uncertainty we can account for retrospective error through new analytical concepts such as 'going amiss'. This requires attention to all relevant aspects in historical context in order to estimate the role of uncertainty in that context. Thus the suggested approach to the study of errors cannot be reproached for being insensitive to historical context. It includes a wider diachronic perspective, which helps to make better sense of particular contexts. What counts as relevant in one context is also a function of its relation to others contexts. This insight cannot be put to use in the 'errors as failures' approach. To study the decrease of uncertainty one should not focus on a set of rational norms alone. It is not always the case that irrationality corresponds with error and rationality with truth. We need a more liberal theory of learning that includes fertile effects that errors can have and can deal with radical shifts in perspective and challenges that come about through confrontations between distinct conceptual schemes. To realize these ideas in full I believe we need to capture evaluation in thoroughly comparative terms.

General outlooks on the phenomenon of error depend on the question whether one views them as avoidable or as unavoidable. In the views of Bacon and Descartes we find the idea that if prescripts are followed, true knowledge will be found. Hence errors are in principle avoidable. Meta-methodological strictures have been designed to exclude errors from science, but these are generally not combined with the assumption that errors are in principle avoidable. On the contrary, science may be considered as a business of generating errors. Yet, the idea is that, once discarded, the errors will not turn up again, provided the right procedures continue to be followed. After all excluding known possible errors from both practice and theory is the hallmark of good science. In this sense errors are avoidable and possibly, when all errors are overcome, they will forever be avoided and scientific development will come to a halt. If a doctor cures all his patients he is out of his job.

I have argued that history of science is not just about discovering errors and making them avoidable in the future. When uncertainty is decreased this may not be forever, as confrontations with new perspectives, new evidence or new technologies may come to undermine established certainties. Even changes in the very standards of evaluation occur from time to time. To reduce uncertainty again requires new research and it seems to me that it is inevitable that errors come about when new research programmes are executed. Moreover I do not view errors and truths as absolute categories. What was discarded as an error in earlier periods can turn out to be less wrong, or perhaps even right, later on, provided good reasons for such shifts in perception can be given. Theories about the world are more or less credible, given alternative views. A sound theory of scientific change must capture such dynamism. This does not mean that the whole history of science is a catalogue of errors, but only that we cannot assume that acquired certainty lasts forever.

Chapter 5 Disarming the Arguments against Evaluative Historiography

In chapter 1, I presented a list of arguments that can be brought to bear against evaluative historiography. We must find replies to these arguments in order to make assessments of past science legitimate and worthy of pursuit. In this chapter I want to take stock and analyse what the discussion of interpretive principles in chapter 2 and 3, and of the notion of error in chapter 4, has produced with respect to the arguments given in chapter 1. I will do this by looking at the two clusters of arguments separately: first, the cluster of arguments that denies that evaluative historiography can be meaningfully carried out, and second, the cluster of arguments that questions whether evaluative historiography is desirable, even if it can be carried out. Chapters 2 to 4 have already either yielded replies to the arguments or indicated what needs to be developed further in other to disarm the arguments in full. In the end this chapter produces a list of desiderata that needs to be worked out in the remainder of the thesis.

An important indication that, with these desiderata, our explorations are heading in the right direction is provided in the final section of this chapter. In that section I identify *common* assumptions behind the two main approaches (formal and natural) to past science. I argue that these assumptions are wrong and need to be overcome. We will see that the way to do this matches the list of desiderata required to disarm the arguments against evaluative historiography.

In chapter 7, I articulate an approach to past science that meets all the requirements set forth in this chapter. The main point of this chapter then is to show that, if the desiderata are met, we know that the arguments against evaluative historiography have been warded off.

1. The cluster of possibility arguments

The cluster of possibility arguments consisted of the following arguments:[325]

[325] Repeated here from chapter 1. See that chapter for a more detailed exposition.

Theory dependence

1. Every evaluative procedure is dependent on a time-bound conceptual framework. This also holds for present-day standards of evaluation. Therefore neutral assessments of past science cannot be given, and therefore assessments should be avoided as much as possible.
2. It is questionable whether historians of science have sufficient insight into the highly specialized nature of modern scientific knowledge. Therefore they cannot properly judge the content of past claims to knowledge. Therefore they should avoid doing so.

Incommensurability

1. The meaning of scientific concepts (or whole conceptual schemes) changes from time to time. Distinct concepts or distinct conceptual schemes, are not translatable via a neutral translation manual or via a neutral language. Hence comparative ground is lacking, which is needed to assess the relative worth of concepts. Therefore we must accept a variety of belief.

Rule following

1. Science does not progress in a well-ordered step-by-step fashion. In spite of many attempts it has not proven possible to satisfactorily capture the dynamics of theory replacement with a single normative meta-methodology. Without such a formal decision procedure, it is not possible to measure progress through change, and hence evaluative historiography cannot be carried out.
2. Rational standards are context dependent. This means that rational factors do not form a special category. Without demarcation, evaluative historiography makes no sense.
3. In the absence of a clear formal procedure, the correct next step cannot be inferred from previous steps. Therefore we cannot judge whether in a given situation the correct theory choice was made. Hence theory change is not cumulative and hence scientific progress is an elusive concept.
4. Standards of appraisal change from time to time. There are no super-standards available against which those changes can be judged as progressive. Hence all standards of appraisal have equal merit.

All these arguments question the possibility of establishing grounds on which evaluative historiography would be possible. As historians lack access to transcendental standards of rationality and objectivity, to current scientific expertise, to an independent translation procedure, and to a base of independent empirical facts, these grounds cannot be given and hence evaluations cannot be meaningfully carried out.

Most of the arguments questioning the ability to carry out evaluative historiography stress that the history of science is a discontinuous affair. What is required to parry them is a statement of a platform of continuity in order to gain grounds for evaluative comparisons between past and present or between historical episodes. The elements of the platform must be chosen in such a way that they do not fly in the face of the arguments above. They must involve the recognition that the history of science cannot be formally ordered and that shifts in meaning of concepts and in standards of appraisal need to be accepted. What follows is an indication of a number of ideas for the constitution of the platform. Chapter 7 contains a further elaboration of these ideas.

First, there is the idea to use a distinction between a general type and particular occurrences of this type, in concrete historical situations.[326] This idea takes its cue from SSK's symmetry principle, which says that the same type of factors determines all outcomes in science. The general type of factors referred to in SSK is social factors. But social factors are not the same everywhere. Detailed investigation from case to case which social factors were relevant and how they played out in the particular situation is required. All these particulars cannot and should not be defined on type level. Using this distinction enables us to speak of factors in the history of science on a general level, and hence as part of the platform, but at the same time allow for variation in the occurrence of these factors in concrete historical circumstances.

In chapter 7 I argue that the type-occurrence distinction can be fruitfully applied to rational factors as well. The definition of rationality that is provided there leads to assumptions about the cognitive functioning of past participants. Upon these assumptions, charity-first interpretations of past science become possible. The discussion of the principle of charity has shown that complete incommensurability simply cannot be made sense of. The challenge is to secure evaluations of past science in light of partial failure of interpretation. On such

[326] The distinction resembles C. S. Peirce's famous distinction between types and tokens where the type is the general thing and the token its concrete, physical, realization. A token then is a more restricted notion than an occurrence of a type. I believe 'occurrence' suits the present purposes better.

partial failures, the charity-first interpretation offers the way out. We should remember that this interpretation is a process that does not have to result in 'rationality last'. Moreover, even if the overall rationality of a past actor can be preserved, it can still be established that he or she was wrong about things by comparison to later points of view. Because we do not assume charity on the semantic content of past utterances there is the space to be evaluative on this content while retaining maximal respect on the level of cognitive attitudes. A very minimal assumption of commensurability in terms of cognitive attitudes can already yield important clues for evaluative historiography, as we have for example seen in the interpretation of the globule hypothesis through the prism of the notion of 'going amiss' in chapter 4.

A second aspect of the platform will be the application of anachronisms. Following Jardine (2000), I defend the claim that anachronisms, if used properly, can enhance understanding of the past, instead of distorting it. In chapter 7 we will see that proper use of anachronisms in historical interpretation involves a circle of interpretation, which shares many features with charitable interpretation discussed in chapter 3. A third element of the platform has to do with the use of modern scientific knowledge. It has been argued in chapter 1, section 8, that modern insights in particular phenomena can yield valuable tools for historical interpretation on the assumption that past actors grappled with phenomena comparable to those we face today. Only in this sense present-day knowledge can be turned to good use in historiography of science.

To problem 2 of theory dependence it can be replied that historians do not need to be acquainted with all details of modern knowledge in order to use it for historical explanation. In the example of the history of geology given in chapter 4, if one knows that a mechanism has been found that can account for the transportation of large land masses, perhaps only a bit more detail about the workings of this mechanism is all that is required to deepen our understanding why the stabilist theory maintained the upper hand for such a long time over the mobilist theory. Moreover there is no such thing as absolute certainty. Present-day understanding of phenomena may have improved over past understanding but it may very well be that present-day understanding will be altered in the future. If the argument asks for complete certainty it is ill directed.

We are not aiming, as Sarton was, to turn science and history of science into complementary undertakings of the same project. This would require making all historical knowledge relevant to present-day science and all scientific knowledge to historical research. A degree of exchange between the two is certainly useful but it can be had without striving for overall unity. Proper use of modern knowledge may require (more) cooperation between historians and scientists. Scientists can benefit from this cooperation too as historical scholarship can reveal forgotten lines of research or discover things by restaging past experiments.[327]

The rule-following argument is based on the view that assessments of progress depend in full on rational factors. In chapter 4 I argued that this does not always have to be the case. There are many ways in which errors can be fertile, such as opening up a new vista, arriving at a good conclusion, even though the path to it contains errors (being right for the wrong reasons) and sometimes (deliberately imposed) limitations can be productive. Progress can also come about through confrontation with something different or just by exploring new directions, not necessarily driven by a pressing problem of research. I believe these possibilities for improvement represent diverse ways of learning, which cannot be reduced to standardized procedures of rationality. This is not to say that assessments of progress are no longer dependent on rational factors. In the majority of cases they still will be. Yet, the intended way to interpret rational factors: in terms of the pursuit of virtues and by making the type-occurrence distinction, does not reduce rationality to a single norm.[328]

Finally, the possibility arguments require a specification of a diachronic approach, which does not lead to Whiggism. I find this approach in conceiving of the history of science as collection of research programmes. With hindsight we can see the beginning and endpoints of research programmes. This makes it possible to treat all the episodes falling within these points as belonging to each other, namely as phases of the same research programme. This may make one think of Lakatos' methodology of scientific research programmes. However, in chapter 7 I hope to show that the perspective of past science as a collection of scientific research programmes does not entail a sharp distinction between internal and external realms of science. Nor does it involve positing extra historical norms of rationality. Both are the case with Lakatos and these are exactly the reasons why sociologists and historians of science have rightfully found Lakatos' methodology unacceptable.[329]

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[327] See chapter 7 for further discussion on restaging experiments and the complementary functions of history of science to present-day science.
[328] For details see chapter 7.
[329] Bloor (1976) for example is explicitly reacting to Lakatos.

2. The cluster of desirability arguments

Theory dependence

3. It is questionable what is gained by 'purifying' claims to knowledge by lifting them out of context. This does not help in deepening our understanding of the meaning of past knowledge claims. A better project is to aim to understand past claims to knowledge in their respective contexts: evaluations stand in the way of such a project.
4. The project of gaining understanding of the past is not helped by mixing it with our own bias. Even though this is unavoidable to some extent, one can aim to be as neutral as possible. An evaluative attitude would lead to an undesirable selective gaze on the past. The bias of the historian against persons, countries, periods etc., may lead to overlook aspects of the past that were relevant, possibly even *for* things highly valued in the present.

Presentism

1. To use the present as a touchstone for the past leads to constant rewriting of the past. As the present is in constant change, the selection of what is found relevant in the past changes too. This is strange because the past no longer changes.
2. Presentist historiography is teleological. It is circular because it explains developments towards outcomes while using these very outcomes as explanatory tools. Further, finalistic historiography lends the historical process a form of necessity that it most probably did not have. It also leads to ancestor hunting, search for origins, pioneers, anticipations, prototypes, etc., of things that for the historical actors were not ancestors or origins of later developments at all. This leads to serious misinterpretations of the real motivations, ideas, etc., of past actors. Using hindsight in this way is thus harmful and should be avoided
3. Evaluations of past science based on present-day expert knowledge are scientific assessments and not historical assessments. If evaluative practice becomes dependent on current scientific insights, then it is hard to see what the point of *historical* investigation and evaluation is.
4. Use of anachronistic terminology in the explication of past science leads to distorted pictures of historical reality. The past should be studied as a self-contained entity and it is better to confine oneself to actors' categories only.

Underdetermination

1. Theory choice is underdetermined by empirical evidence, as well as by standards of rationality and other cognitive values. Thus it must be that theory choice is determined by something else and this is purely human factors, whether social or personal. Therefore theory choice should not be evaluated in the traditional sense since the traditional epistemic categories are not primary.
2. Accepting standards of rationality or validity or any other cognitive value as an explanation for theory choice is circular. Theories are said to be rational, valid, successful, etc., *ex post facto*, after they have come out as the winner of a controversy.

This cluster of arguments does not question the grounds for evaluative historiography. The arguments say that even if this were possible, the use of evaluative categories and standards would obscure historical understanding and/or lead to circular explanations of past science. An evaluative approach should thus be avoided because of its negative consequences for historiography. A number of things can be said in reply to these arguments. Note that it should be kept in mind that some of the things we have said in reply to the possibility arguments apply here as well: what is desirable can coincide with what is necessary.

All the arguments of this cluster, in one way or another, demand that the historian of science take up a position of neutrality towards his or her research subject: the past should speak for itself. An evaluative attitude is not a neutral attitude and hence should be avoided. It should however be recognized that a fully neutral approach to past science cannot be obtained. *Implicitly*, in terms of motivations for research, selecting sources, valuing these, etc., one has to be judgemental and these implicit judgements are inevitably influenced by the historian's own time. To be *explicitly* evaluative is indeed to move one step further. The choice however is now no longer between being neutral and being non-neutral. While the arguments against presentism are in general quite plausible, I agree with Jardine that it is utterly simplistic to restrict historical explanation to actors' categories as a remedy to presentist sins.[330] The discussion should be about which sort of commitment is the most beneficial to historical understanding. The platform which will be set up in chapter 7 must be read as an expression of such a, moderately presentist, commitment.

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[330] Jardine (2000).

As we have seen in chapter 2, the insistence on neutrality causes a number of inhibitions. The localism of SSK, for example, leaves all kinds of historical questions unaddressed. These involve questions about the direct contact between localities, as well as comparative questions bringing more distinct localities into relation. The assumptions about social factors SSK does allow for, unfortunately lead to overly schematized studies of scientific controversies. Posthumanism does not fare much better. Either posthumanist accounts of past science fall back on SSK-style of explanation, or the lack of selective criteria makes all actions of all historical agents relevant. To describe these is not only impossible, it is also undesirable because it would lead to a repetition of the past, without the gain of understanding what has happened. Moreover, the crucial notion of agency has remained surprisingly under-analysed, and hence does also not bring many gains in terms of historical understanding. To overcome the restrictions set on historical interpretation by SSK and posthumanism, more input in terms of analytical concepts is necessary.

With respect to the charge of circularity, the reply has to be that not every form of circularity is harmful. In chapter 3 we have seen that some degree of circularity in historical interpretation is simply unavoidable. More specifically we have articulated a charity-based interpretation in the form of a dialogue.[331] The dialogue can lead, not only to interpretation of others, but also to improved self-understanding, for example with respect to the concepts we use.[332] When properly carried out such a dynamic interpretation process does not lead to freezing the past into present-day categories. Interpretation starts from a specific framework, yet because of the dialogue, one does remain tied to the premises with which the interpretation has started.[333] This takes the main sting out of the desirability arguments. What has to be accepted is that interpretation can never come to full closure: it is an ongoing dialogue.

The diachronic view on the past in terms of a collection of research programmes can also be used to take the sting out of the argument from underdetermination. Symmetrical approaches to past science have to insist on closure at every joint in the road because otherwise a degree of

[331] See the discussion on Lloyd in chapter 3, section 4.
[332] This is one of the strong points of the comparative approach to history. Ragin (1987), for example, subscribes to the view that the dialogue is a central aspect of any comparative approach in the humanities. More reflections on comparative historiography of science follow in chapters 6 and 7.
[333] Interpretation of others always simultaneously requires an interpretation of the concepts by which this interpretation of others is carried out. This is known as the double hermeneutic problem, cf. Giddens (1984).

underdetermination lingers on and this cannot be reconciled with the idea that social factors always force a choice for one of the competitors. If we allow for a longer-term perspective it is not required to expect full closure of controversies at each instance of theory choice. As will be further explicated in chapter 7 we can make a distinction between weak and strong choices in past science. Only the strong choices lead to closure. When strong choices are considered as the outcomes of long processes of deliberation involving many weak choices, possibly recalling earlier steps, etc., we are no longer forced to accept the conclusion that the strong choices are invariably determined by social factors.

The cluster of desirability arguments requires a positive reply, because an alternative perspective on science has to be made attractive. The point of engaging in evaluative historiography is not to play a blame game or to pick bones, but to gain a deeper understanding of the dynamic process that science is. Evaluative categories can be beneficial to historical understanding because they provide clues that cannot be obtained otherwise.[334]

An important idea is to start thinking differently about determining factors in past science, namely not in hierarchical but in relationalist terms. The new relationalism, suggested in chapter 2, introduces an approach to the study of symmetry breaking in terms of changes in relations. It includes all possible determining factors without assuming a strict order between them. This makes the approach stay beyond demarcation between internal and external factors but at the same time the approach crucially allows for cognitive factors once again to play a role in the explication of past science, without making them categorically dominant over other factors.[335]

In any case it is the historian's job to find out which factors were relevant in a given situation and how these factors combined to determine theory choice. The naturalist aspect of the approach to which we must commit ourselves is given by the focus on concrete problem situations in the past. The efficacy of determining factors is dependent on these problem situations. The historian needs to carve out the relevant factors, including a good grasp of the (cognitive) resources available to the historical actors. This naturalist aspect of the extended naturalist approach will be further explored in chapter 6.

[334] See also chapter 1, section 8.
[335] The need for this was reinforced in chapter 4.

3. Common assumptions of the two major projects in the science studies

Throughout this thesis I have identified two main approaches to the history of science. One that aims for a formal analysis and the other that aims for a natural, or causal, analysis of past science. In chapter 4 this has also been captured as an opposition between based on demarcation vs. approaches based on a symmetry principle. Other terms which are frequently encountered but which are less exact are objectivism vs. relativism or Whiggism vs. contextualism.

Laudan has captured the distinction between the two major views on science in terms of positivism vs. post-positivism.[336] This terminology is not wholly satisfactory either. The formalist tradition in the study of past science originates in logical positivism, but the term positivism does not apply to all scholars in the formalist tradition: a clear example is Popper. Nevertheless Laudan's paper on the subject is highly relevant because he is, as far as I know, the first philosopher to indicate a number of *common* assumptions behind the two major projects in science studies. This comes unexpectedly, as the two projects radically oppose each other. How can it be that such strong adversaries share common assumptions?

Laudan indicates that, although post-positivism has been highly critical of positivism, a number of pretty strong positivist doctrines have continued to live on in post-positivism. It is because of this that he asserts, "the roots of post-positivist relativism are found deeply embedded in positivist soil."[337] These roots are often hidden from view in the articulation of post-positivist approaches to the study of science. It is a significant step forward, especially for the discussion of evaluative historiography, to lay bare these common assumptions because they enforce deadlocked oppositions, such as the one between judging and understanding. It is false to suppose that these two concepts are mutually exclusive, yet the notion is very persistent. To move beyond false dichotomies we must move beyond the *shared* assumptions about science on which they rest.

I will first give the shared assumptions Laudan identified and then add a number of other common assumptions that can be extracted from our own analysis. This gives a list of eight assumptions about science that we need to reject. Like the inversion of a photographic negative, this provides us at the same time with a list of things about science that should be accepted.

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336 I am referring to Laudan (1996) pp. 3-28. Others have picked up his terminology. Zammito (2004) for example has post-positivism run from Quine to Latour.
337 Laudan (1996) p.24.

Laudan mentioned five, frequently used objections against positivism, which contain as first premises flawed assumptions about science. These are: [338]

1. The thesis of translatability as a pre-condition for comparative assessments, which has triggered the doctrine of incommensurability.
2. The thesis of a subjectivist meta-epistemology, which has triggered the idea that scientific disputes based on methodological differences cannot be rationally resolved.
3. The thesis that algorithmic rules of theory choice are a precondition for scientific rationality, which has triggered the claim that there are no such rules and hence scientific rationality is non-existent.
4. The positivist insistence on radical underdetermination of theories triggers the indeterminacy of translation of Quine or the relativist thesis that evidence is irrelevant to theory evaluation.
5. The notion that cumulative theory change is a precondition for scientific progress has triggered Kuhn and Feyerabend to claim that there is no progress since theory change is evidently not cumulative.

Positivist assumptions about science are taken as premises in these five objections. These premises are taken to have evident consequences. Thus, when the premises are rejected these consequences have also to be rejected. Take for example the fifth objection. There it is supposed that progress depends on cumulative theory change. The positivist thinks he can establish a cumulative sequence of theories and therefore he can account for scientific progress. The post-positivist denies that such a line of progression can be given and therefore a conception of scientific progress cannot be justified. Either you have both or you have neither. But this shows that the *connection* between the notions cumulative change and progress goes by unquestioned and is hence supported by both. Similarly scientific rationality is made dependent on 'algorithmic rules of theory choice', commensurability is made dependent on translatability, etc. Post-positivism does not challenge any of these connections and in this way basic flaws in the positivist image of science are retained in post-positivist approaches.[339]

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[338] Formulations taken from Laudan (1996).

[339] Laudan indicates that one reason for the persistence of positivist assumptions is the linguistic turn in philosophy which continues to be influential in post-positivism, with which he may have hit another nail on its head. The notion of translatability occupies a central place in thinking about the history of science. Bloor applied the social mainly to linguistic behaviour and he connected SSK to Wittgenstein's 'meaning is use' doctrine.

Because they retain the same connections between concepts, but value the premises differently, the two projects oppose each other by 180 degrees. The common assumptions make it impossible to occupy a middle ground. This explains the occurrence of what I have earlier called a demarcation reflex among contextualist historians. Presented with the stimulus of terms such as rationality and progress, they instantly react dismissively. For them these concepts cannot be included in historical explanation because if they allowed this, the positivist assumptions about science would have to be embraced as well.[340] The current project has to demonstrate that, on different premises, there is room for notions of rationality and progress and hence these can be allowed to play a role in historical explanation without the need to embrace the positivist project.

To Laudan's list a number of other, but equally mistaken, shared assumptions can be added. Both projects are committed to a language of determination of theories. Fuller, for example, has identified the traditional approach to past science with a strategy of overdetermination and the alternative approach with underdetermination.[341] Historical accounts of the former are backward looking and the course of history in it is overdetermined by non-human factors. In the latter the accounts are under-determined by non-human factors and written forward, without knowledge of future effects of choices made. A typical question about the scientific revolution from the over-determined perspective is, why did it happen in Western Europe in the 16th and 17th centuries and not somewhere else? The underdetermined question is radically different: why did the scientific revolution happen *at all*?

Indeed all approaches suppose that one set of factors is decisive in determining theory choice. On the one hand natural or rational factors are given as dominant. On the other hand social or personal (agency) factors are given as dominant. It is true that in posthumanism a dichotomy between social and natural factors is no longer recognized. Yet in posthumanism a dominant set of factors, pertaining to the agency of humans and non-humans, is still operative. Further, rational factors are barred from playing a decisive role in

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Latour too speaks of inscriptions and focuses on processes of translation, transmission, etc., in the circulation of knowledge. Another example is Galison, who suggested studying the contact between the various subcultures in physics via concepts taken from anthropological linguistics such as 'pidgins' and 'creoles'. These examples show how deep the linguistic turn still influences our thinking about science.

340 A symptom of such rejections is to make a caricature of present-day scientific knowledge as the unshakable truth and turn all believers in the progressiveness of science into naïve realists. See for examples Bloor (1976) pp. 8-10 and Latour (1987) p.234 and p.243.

341 Fuller (2008).

determining theory choice. So even in posthumanism a hierarchy of factors is clearly present.

With these common assumptions added, we now have eight assumptions of past science that need to be rejected. This leads to the following statements about science that *should* be accepted:

1. There is no viable notion of a theory-neutral observation language (yet comparative grounds can be established).
2. Not all scientists subscribe to the same methodological standards (yet disputes are rationally resolved).
3. There are no mechanical algorithms for theory evaluation (yet rationality plays a role in science).
4. Theory choice can sometimes be underdetermined (yet evidence is relevant to theory choice).
5. Theory change is not cumulative (yet science is progressive).
6. In studying theory choice the determination of theories is not of primary importance.
7. There is no principled hierarchy among the determining factors in science.
8. No strict demarcation between internal and external factors is possible (yet there is room for evaluative historiography).

The first five follow from Laudan's analysis, the last three are added by me. Laudan would not agree with them because he continues to operate with a notion of demarcation. Notwithstanding the historical sensitivity of his model of science, he resorts to a sociology of error. I believe that we need to embrace 6, 7 and 8 as well, in order to arrive at an acceptable form of evaluative historiography.

It is not difficult to see how the positivist assumptions relate to the arguments against evaluative historiography. With the exception of presentism all the other arguments are present. Thus incommensurability relates to 1, theory dependence to 2, rule following to 3 and 5, and underdetermination to 4. It follows that what we have said in reply to the arguments against evaluative historiography must apply to 1-5 above too. Indeed 1-5 ask for comparative ground, which we will claim with the platform in chapter 7. They also ask us to rethink the notions of rationality and progress: this too will be undertaken in chapter 7. Finally the new relationalism and the perspective of uncertainty offer fresh perspectives on science, with which 6, 7 and 8 too can be adequately handled.

Our aim is to find replies to the arguments against evaluative historiography. In this chapter the directions in which these replies must be sought have been indicated. This has yielded a list of desiderata that needs to be developed in the last two chapters. The nice result of this section is that we know that if the desiderata can be met, we can simultaneously get rid of the persistent positivist assumptions. These assumptions are the remnants of the formal tradition that have continued to influence debates on approaches to the study of (past) science. Moving beyond them therefore necessarily involves moving in the direction of naturalism. Yet a fully naturalist approach to science cannot be embraced. The realization that with the replies to the arguments against evaluative historiography deep flaws in thinking about science are addressed, is a strong indication that we are on the right path, but also raises the bar. Overcoming positivist assumptions requires the development of an alternative model of science that can neither sit at home with the positivist approaches nor with the post-positivist ones. In the next chapter, two likely candidates for a naturalist approach *with* an evaluative dimension, which I will call extended naturalism, will be investigated, namely normative naturalism and evolutionism. Neither is wholly satisfactory for our purposes. This means that we have to settle on an alternative form of extended naturalism, which will be articulated in the final chapter. In the present chapter ingredients of that alternative have already been indicated. We know now that if these can be adequately articulated, the arguments against assessments of past science will have been disarmed.

Chapter 6 Extended Naturalism

1. The search for a golden mean

Formal approaches to science run into problems when the norms they put forward do not reflect actual decision-making that has to be classified as rational. The lesson we have to draw from this is that the postulation of norms of good science, that is scientific rationality, has to be grounded in actual history. Giere has correctly qualified the programme of providing an extra-scientific foundation as the secular version of the medieval project of finding proofs of the existence of God.[342] If we want to move beyond positivism, and its persistent assumptions about science, naturalism needs to be adopted first.

Naturalist study of science is not a 20th-century invention.[343] Yet most discussions of naturalism start with Quine. Quine famously rejected two central tenets of positivism: reductionism and the analytic-synthetic distinction.[344] This forced him to reject the entire positivist approach to the study of (scientific) knowledge and replace it with a naturalized epistemology.[345] Central in this naturalized epistemology is the idea that propositions hang together in a web of interconnections, the so-called web of belief. New propositions gain support in view of their logical relations to other propositions held to be true. Changes in the web occur either by some external cause or when contradictions in the existing web are recognized. The epistemologist must inquire by which causal processes new beliefs enter the existing web of belief. For Quine this is entirely a matter of psychology (broadly conceived) and not of logic.[346]

The ambitions of philosophy are immensely reduced in Quine's framework. There is no 'philosophy first' anymore: philosophy is at best a discipline that is continuous with science, in the sense that it must ask questions that only

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[342] Giere (1988) p.xvii.
[343] Kitcher (1992) speaks of a return of naturalism, which he places after the advent of positivism.
[344] Quine (1980). See chapter 3.
[345] See especially Quine (1969).
[346] Note that the distinction between formal and causal does not necessarily entail an opposition between reasons and causes. Only when the term reason is restricted to some formal norm of rationality does the opposition to causes come about. For example, when we try to explain choices made in the past it is well possible to do so in terms of reasons. As Kuhn already argued in *Structure*, we must explain choices made in the past with recourse to reasons when a choice algorithm is not available to us.

empirical research can answer. Logical empiricists had also argued that the task of philosophy had to be restricted to the analysis of statements delivered by science. With Quine the decrease in ambition is taken a step further. In logical empiricism there is a sharp distinction between the tasks of philosophy and the tasks of science. Science delivers theories about the world, which are formulated in sets of propositions. The analysis of the well-formedness and well-foundedness of these sets is the job of philosophers, based on the assumption that logic has an *a priori* legitimacy. Quine no longer envisaged such a division of labour. For him no logic is entirely content-free, and hence the use of logic has to be accounted for by empirical means and cannot be used as an independent tool of analysis.

Naturalist study of past science amounts to fleshing out causal factors responsible for the choices made in the past. Such factors can be: natural, social, cultural, psychological, and historical. Naturalist approaches differ from one another with respect to the valuation of these factors. As we saw in chapter 2, in SSK social factors are taken to be dominant over other factors, while in posthumanism 'personal' factors (agency) are dominant. Quine grants an important role to nature in terms of sensory stimulation and to psychology in terms of the processing of information. But he also thought attention to historical factors was important:

> "Each man is given a scientific heritage plus a continuing barrage of sensory stimulation; and the considerations which guide him in warping his scientific heritage to fit his continuing sensory promptings are, where rational, pragmatic."[347]

Deepening the empirical approach to the study of past science leads to the question of normativity. The problem with fully naturalist approaches is that the 'is' becomes the 'ought'. A forceful critique of the Quinean programme was for example formulated in Kim (1988). Kim argued that Quine cannot deliver the goods epistemology should deliver, because he completely neglects that epistemology is a normative inquiry. Epistemology should establish what we ought to believe, considering the evidence, and not just focus on where our beliefs come from. Further he argued that Quine's approach is circular. In order to explain scientific knowledge one has to make use of scientific knowledge as an explanatory resource.[348]

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[347] Quine (1980).
[348] For this point see also Andler (2010). Zammito (2004) pp.50-51 for these reasons thinks that naturalism must be rescued from its founder Quine.

As is often not well recognized Quine addressed the issue of normativity himself and he formulated a 'stock response' to it. Quine argued that he had been misunderstood and that the normative in his approach is naturalized, but not dropped. He envisaged the normative to work as follows:

"For me, normative epistemology is a branch of engineering. It is the technology of truth-seeking ... it is a matter of efficacy for an ulterior end, truth. The normative here, as elsewhere in engineering, becomes descriptive when the terminal parameter is expressed."[349]

What this 'branch of engineering' or 'technology of truth-seeking' is has however remained hazy. Houkes (2002) follows a literal interpretation of the terms 'engineering' and 'technological' at the expense of a metaphorical reading, which I don't think is very illuminating. The analogy is supposed to work when one can simply express the terminal parameter as 'truth'. It is however mysterious how this connects to the instrumentalist view of reason as taking means to ends. As we saw in chapter 4, with the 'errors as failures' approach, the shift to instrumental reason entails an altogether different view of knowledge and truth. If the analogy were supposed to work this way, then this would invite all the shortcomings of the 'errors as failures' approach given in chapter 4. This was surely not intended by Quine, but it remains unclear how the analogy between epistemology and engineering can work with a traditional concept of truth.

Still, I think we have to take naturalism as a starting point.[350] This means that we must follow closely how the past actually developed and accept a variety of epistemic goals that have been pursued, a variety of methods of research that have been adopted and even shifts in evaluative standards that have occurred from time to time. Yet, we cannot accept that this leads to giving up on all assessments of past science.[351] I would like to call a naturalist approach to past science, which intends to be normative as well, 'extended naturalism'.

The challenge for extended naturalism is to formulate a normative standard 'from within'. Most importantly, this involves understanding the concept of

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[349] Quine (1990) pp.664-665.

[350] Our main interest lies in naturalism with respect to history of science. However, Kitcher (1992) p.80 points out that naturalism can possibly benefit analytic philosophy because it can help to avoid posing ever more intricate conditions and constraints on knowledge and the justification of knowledge.

[351] Kitcher (1992) connects giving up the normative perspective explicitly to Bloor, Latour and Feyerabend. For a thorough exposition of the disappearance of evaluative historiography I refer to chapters 1 and 2.

rationality in naturalist terms.[352] According to Giere, as long as HPS has not succeeded in solving this problem, it cannot function properly:

"The general problem in HPS is to show that philosophical conclusions about what is rational, i.e. norms, may be supported by historical facts and just how this comes about. Until this is done, the historical approach to philosophy of science, is without a conceptually coherent programme."[353]

Twenty years later Kitcher argued that one of the most important requirements, in order to create the possibility of sustaining the reliability of the historical process with an account of cognitive value, is a reform of the basic language in which cognition is discussed. He added that he could see no way to adequately meet this requirement.[354] I believe that we still have to meet this challenge today, which is essentially the same as Giere's.

Putting history first does not mean that philosophy is out completely.[355] It means that we should avoid seeking ultimate proofs of evaluative principles, but not that we should give up on systematic analysis of past science, nor on all evaluation of the content of scientific theories. What must be accepted is that standards formulated 'from within' can never be finite. Any naturalist project in the science studies has to accept a degree of circularity and openness for revision.[356] Schindler draws the following conclusion from this:

"After all the norm and fact divide is not just a problem for philosophy of science but a general problem for any naturalist approach towards norms. Perhaps this challenge … may simply not have a solution."[357]

But perhaps there is a solution. At least two well-developed options are present in the literature, namely normative naturalism and evolutionary epistemology. These approaches to the study of science are both naturalist *and* contain a normative dimension that is set up from within. It has to be evaluated whether these options are suitable for the kind of evaluative historiography we seek, taking into consideration the requirements this historiography has to meet, which were given in chapter 5.

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[352] Giere (1985) p.332.
[353] Giere (1973) p.290.
[354] Kitcher (1992) p.118.
[355] This point was made in chapter 1 as well.
[356] See chapter 3, section 5 and chapter 5, section 1.
[357] Schindler (2013).

2. Normative naturalism

2.1 Overview of the approach

In normative naturalism, it is observed that well-tested methods of research, proofs and evaluation have established themselves for good reasons in the specific scientific disciplines. They are the best-known means to reach corresponding ends. The means originate in particular historical contexts, but there is a mode of application of them that transcends these particular contexts, because similar ends can be promoted in different contexts. It is therefore admissible to use the established standards and methods for normative purposes. The normative aspect of the approach can be captured with the following rule: if one's goal is X and Y is the best known means to achieve end X, one ought to do Y. This is an instantiation of instrumental normativity: optimal means-ends relationships are not given, but have to be established through experimental practice. In normative naturalism no norms are imposed *a priori*, as they originate in particular historical contexts. But they also do not hold forever *a posteriori* because better candidates to serve particular ends may arise. Norms must therefore always be open for empirical testing, just as scientific theories ought to be.

The main proponent of normative naturalism is Larry Laudan. For Laudan science is first and foremost a problem-solving activity.[358] Problems are substantive questions about objects that constitute the domain of any given science. These can be both empirical and conceptual problems such as getting accurate empirical data, inconsistencies in a theory, inconsistencies between theories and setting aims and methods straight. The right aims of science are not given, and preferences about which aims to pursue may change from one historical context to another. As generally more than one problem has to be solved at the same time, the weighting of problems in terms of importance has to be studied as well. Likewise, what counts as an appropriate problem solver is also dependent on historical preferences. In all these respects we have to go along with the actor's decisions for a long time. Therefore Laudan denies that context-independent meta-norms of rationality can be given:

"Within this account of meta-methodology, we need not concern ourselves with questions about the rationality or irrationality of particular episodes or actors in the history of science. Nor need we invoke shared intuitions about concrete cases in order to

[358] The main text is Laudan (1977).

decide, on this approach, whether one methodology is better than another. We simply inquire about which methods have promoted, or failed to promote, which sort of cognitive ends in the past." [359]

Yet evaluation of choices made in the past is possible in Laudan's model because the rational thing to do, in any situation, is to accept the best problem-solving procedure, or the best problem solver. The notion of problem solver is not restricted to methods. Theories can also be problem solvers. Laudan's idea is that the acceptability of theories must always be relativized to competition. Comparative evaluation is rational when it involves comparing theories on their problem-solving effectiveness. Note that Laudan also recognized other standards of comparative evaluation such as consistency, empirical adequacy and predictive accuracy. But in his model, problem-solving capacity always outweighs the other theoretical virtues.

For Laudan we do not have to know what the perfect problem-solving options are (these may not even exist) in order to assess whether one method or theory is a better problem solver than another. To assess the worth of particular solutions one has to place them in the historical line of development of a field of study and consider the long-term fecundity of the solutions. Only such long-term analysis of research programmes yields stable assessments. Often solutions to problems are only partial. An example is Galileo's solution to the problem of free fall. That solution was an improvement over Aristotle's theory, but not correct from our point of view, as the problem of the centre of the Earth still needed to be solved.

Thus we should assess solutions to problems, not on their own terms, but diachronically in relation to predecessors and subsequent solutions, and synchronically in relation to competing alternatives. This even holds for 'wrong' traditions in which dominant theories such as the phlogiston theory, the caloric theory of heat, or the electromagnetic ether theory eventually stagnated, but within the paradigms that they set, a, to some extent, progressive development in theorizing could be witnessed.

The assumption of normative naturalism is that we are getting better at promoting ends in science by selecting the most appropriate means to these ends. The history of science effectively yields a collection of best practices. It is however essential to see that these best practices remain open for improvement. Kitcher has also pointed this out:

[359] Laudan (1996) p.137.

"Thus, just as we excuse ourselves and our predecessors for failure to be omniscient, concepts of rationality and justification *used in assessing the performances of others* should also take into account our methodological foibles."[360]

If these foibles are discovered the idea is that we will improve. Science in this sense is, according to Kitcher, a meliorative project. In this context Mayo's alignment with normative naturalism also becomes clear. Her 'error repertoires' are canonized sets of strategies in various disciplines. These repertoires are extended when the need for this is discovered in experimental research. Changes in error repertoires are the manifestation of continuous improvement in science.

Sometimes Vienna Circle philosopher Otto Neurath is hailed as the founding father of normative naturalism because of his metaphor of science as a boat on the open sea.[361] The boat is launched to the water without being completely finished. We have to find out where to improve the vessel by reacting appropriately to problems, such as any leaking. There is no access to a dry-dock from which the boat can be checked and repaired. Reparation, hence improvement, can therefore take place only afloat.

In closing this section it is useful to compare Laudan's account of problem-solving effectiveness with Kuhn's ideas. Already in *Structure* Kuhn argued that new paradigms gain acceptance only when they offer a solution to problems, the so-called anomalies, surmounting the existing paradigm. Yet paradigm shifts are not 100% beneficial because some problem-solving capacity of the former paradigm gets lost in the transition, if only because from the perspective of the new paradigm, issues previously thought important simply lose their relevance. The failure to retain all puzzle-solving capacity through paradigm change is known as 'Kuhn loss'.

In Laudan's model Kuhn loss does not come about. On his reticulation model of scientific progress, science consists of three categories: theories, methods and cognitive aims. All three change in the course of time, but not all at once. When changes occur in one of the three categories the other two will, for the time being, not change. Laudan did not accept Kuhn's argument that to accomplish breakthroughs in science, radical shifts are required. Instead he argued that solutions to problems appear as meaningful only when large parts of the existing research programme remain fixed.[362] Without the radical shift, new problem

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[360] Kitcher (1992) p.67, italics in original.
[361] Losee (2004).
[362] That the possibility of understanding differences hinges on assumptions of continuity was a central theme running through chapter 3.

solvers have to be assessed with reference to the rest of the research programme, which has remained stable. This prevents the occurrence of Kuhn loss.

I agree with Laudan that Kuhn's model is too radical and that a layered model of change offers far more possibilities to account for the flexible and dynamic processes that science consists of. Conceiving of science as a multi-layered structure fits the demand, formulated in chapter 5, of developing an appropriate diachronic zoom on the past. Interesting articulations of science as multi-layered structure can be found in New Experimentalism, some evolutionary approaches to past science (see below), Amsterdamska's idea systems (Amsterdamska 1987), Galison's brick model (Galison 1988) and the study of disciplines as hybrids (Galison 1997, Karstens 2012).[363]

2.2 An assessment of normative naturalism as a basis for evaluative historiography of science

Normative naturalism contains a number of attractive points for evaluative historiography of science. The first is the focus on problem situations. It is with respect to concrete problem situations that the merit of the contesting options must be assessed. Normative naturalism thus invites us to study concrete argumentation practices. This accords with Dretske's relevant alternatives approach in epistemology.[364] A situation of theory choice can be carved out by establishing the relevant alternatives. Especially historical scholarship can provide insight in both the problem situation and the relevant factors that played a role in that particular situation. Concrete problem situations provide the context in which an assessment of progress must be traced out. In chapter 4 it was argued that such assessments must commence by establishing the degree of uncertainty. Uncertainty is not something intangible, but can be made quite concrete in terms of relevant choice options. Normative naturalism can be easily adapted to this idea.

A second attractive point of normative naturalism is the assumption that argumentation practices are governed by sets of theoretical virtues, such as in Laudan's case, problem-solving capacity. Preferences for virtues stem from

[363] See chapter 7 for a more detailed discussion.

[364] As pointed out in Schickore (2003). Arguments against the relevant alternatives approach are only embarrassing for the project of providing rock-solid foundations of knowledge. As we are not aiming to establish such foundations, I agree with her that thinking in terms of relevant alternatives may actually be very useful.

history itself, but if such preferences exhibit relatively stable patterns, then these patterns can be used normatively. I believe the virtue approach is basically the right approach to scientific rationality, even though my interpretation of this approach differs from that of the normative naturalists (see chapter 7). In any case, it is an interesting challenge to find out which virtues were preferred in which circumstances, how and why such preferences change (if they do) and to what extent we can generalize virtue preferences into patterns that can be used for evaluative purposes. These challenges provide the historian of science with a possibly very fruitful research agenda.[365]

Closely related to these two attractive points is a third advantage normative naturalism offers for evaluative historiography of science. This is the stress that is put on evaluation of contesting options in the longer run. This clearly involves breaking away from localist approaches to past science, specifically for evaluative purposes. As we saw in the previous chapter such a diachronic perspective is a *sine qua non* requirement in order to make evaluative historiography work. In chapter 7 I argue that a diachronic perspective can in fact be made part of the virtue approach to rationality, if the history of science is considered as a collection of gradually unfolding research programmes, which are organized around specific problems of inquiry.

To the standard charge of circularity, which all naturalist approaches to the study of science must face, i.e. that we 'check' the system with standards that are part of the system; normative naturalism has an interesting 'bootstrap' response. The notion of bootstrapping was developed by Glymour to account for the problem that evidence for hypothesis H can be gained only on the basis of auxiliary hypothesis H' where H and H' are both part of the same theory. Glymour argued that this is excused as long as quantities calculated in H are not calculated in H'. When the value of variables can, to a certain extent, be autonomously established, it is warranted that H' supports H, even though both hypotheses are part of the same theory.[366]

The idea of bootstrapping has also been applied to the study of rational norms. A thorough exposition of this idea can be found in Briskman (1977).[367] For Briskman epistemological standards are introduced in goal-directed environments and aim-oriented activities. Like scientific theories, normative standards compete with each other with respect to a given problem situation.

[365] See also chapter 7.
[366] I am referring to Glymour (1980) and the revision in Glymour (1983). Further discussion can be found in Culler (1995) and Henderson and Horgan (2011). The idea of bootstrapping we owe to Agassi (1974).
[367] Axtell (1992) is a more recent treatment of the issue but it is supportive of Briskman.

The epistemic problem that has to be answered is: how to justify preference for a normative standard against which theories are judged. Briskman argues that the situation that gives rise to this problem is itself the measure of the solution. Because we evaluate and compare relative to the problem situation, he asserts that: "through pursuing aims and articulating competing theories we can actually learn to pursue aims and compare competing theories *more rationally*."[368]

Cognitive aims (including specific problems), possible solutions to the selected central problems, and the standards of evaluation are all part of the same research programme. Yet the elements of this programme are relatively independent. Therefore they can be used in gauging each other. It is possibly that an adjustment in the programme on one of the elements leads to a re-evaluation of the other elements. Briskman identified such adjustments with the term 'bootstrap epistemology' because we learn how to improve our aims while being in the pursuit of these aims. In other words, we pull ourselves up by our own laces. The circularity involved is acceptable because the elements are not inextricably mixed with one another. Laudan's reticulation model can be seen as a variant of bootstrap epistemology. In that model research programmes are seen as collections of methods, theories and aims. A change in the programme occurs, not across the board, but only for one of the elements at the same time. The change can therefore be evaluated with respect to the other elements, which is an instance of bootstrap testing.

Bootstrapping can also help to answer a problem of induction, which faces normative naturalism.[369] The normative naturalist must be able to tell us when revision of means-ends relationships is warranted and when not. This is simply a function of the degree of satisfaction in which aims are pursued. Solutions to problems can for example introduce new problems. An update in terms of

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[368] Briskman (1977) p.526.

[369] A problem of meta-induction has also been formulated against normative naturalism. The normative rule of normative naturalism is: if procedure x worked in situation y and there is no argument in a new situation z, which is comparable to y, not to use x, then x should be preferred. Consistency demands that this meta-methodological principle should be open for revision too. If this is the case we are no longer in possession of a normative guideline and if it is not the case then we have a non-amenable principle and this is against the very content of the principle. I believe this is only seemingly a paradox because the first option can be taken. We can admit that the meta-methodological principle is open to revision, but it has to be revised only if it can be replaced by a better rule. If this rule does not present itself nothing has to be altered and this is consistent with the content of the rule.

means is warranted when such additional problems can be solved without losing the already acquired problem-solving capacity.

An example is Darwin's theory of evolution, which offered an answer to the problem of how to account for the variety of life forms. The time required for the evolutionary process to occur, however, was much longer than one could account for. Lord Kelvin estimated the time that the Earth had needed to cool off after it was formed at somewhere between 20 and 40 million years. If this problem of the age of the Earth could not be solved, Darwin's theory faced serious contrary evidence. Only with the discovery of another source of heat (Kelvin had recognized the Sun as a the only source of heat on Earth), namely from radioactivity under the Earth's surface, could the Earth be estimated as much older, as the process of cooling down must have taken much longer. [370] We can conclude from this that in some cases insurmountable problems may hold a good theory back, while in other cases new problems, posed by a good theory, actually spurs scientific research. Normative naturalism can account for both these cases because it is flexible with respect to shifts in the pursuit of aims and with respect to means-ends relationships.

Notwithstanding all these positive aspects of normative naturalism, the approach still falls short for our purposes. The normative thrust in normative naturalism is either too weak or too strong. It is too weak when generalizations over means-ends relationships cannot be made in any significant degree. We then get long lists of strategies, pertaining to specific fields of inquiry only.[371] The normative question, for any extended naturalism, is what we ought to do, *given* particular interests, limitations, cognitive resources, amount of evidence, scientific heritage, material circumstances, etc. This question cannot be answered when lists of particular strategies do not lead up to a more systematic analysis. While it is true that a degree of methodological pluralism must be accepted, this pluralism cannot be taken too far, otherwise normative naturalism would lose its point.

Normative naturalists can avoid this problem by posing stronger normative demands. Laudan for example has put forward the fairly strong claim that problem-solving effectiveness is the dominant virtue in any situation of theory choice. This is however a problematic claim. As we saw in chapter 4, other

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[370] Rutherford first presented his calculations in a public lecture in 1904 where Kelvin was famously present. Rumour has it that Kelvin slept through most of the lecture, only to open his eyes when Rutherford came to the topic of the age of the Earth, and then briefly nodded when he learned that his theory was overthrown (Eve 1939).

[371] This is the criticism Hon applied to Mayo's error statistics. Recall that Mayo aligned her approach with normative naturalism. See chapter 4, section 2.

virtues, such as empirical adequacy, are sometimes preferred over problem-solving effectiveness in the history of science, and this has to be qualified as rational. We are drawn to the conclusion that although problem-solving effectiveness often outweighs other factors in theory choice, this need not be so on principle.

A more vexing problem with normative naturalism is that, although the approach puts naturalism first, it still results in an analysis of past science based on demarcation between rational and social factors. In Laudan's framework the rational thing to do is always to accept the most effective problem solver. Past choices that do not promote problem-solving effectiveness, in cases where this was possible, must be deemed irrational. Hence such choices must be explained with reference to social factors. Laudan has argued in one of his essays, titled 'The demise of the demarcation problem', that his model no longer suffers from the illusion of demarcation.[372] He indeed no longer specifies definite criteria for rationality, truth, the well-foundedness of belief or what counts as science and what not. But in a subtle way demarcation is still present as he continues to account for one category of past choices via rational factors and another category of choices via social factors.

In chapter 4 it was argued that even weak forms of demarcation cannot be accepted because they fall short in accounting for errors and progress. It is questionable whether the optimal strategies, recognized in normative naturalism, represent the only way in which errors can be detected (and avoided) and progress can be accounted for. Confrontations with other viewpoints, for example with perspectives from other disciplines, can also reveal shortcomings, or open up new ways of carrying out research. This calls for a broad comparative analysis but comparative analysis in normative naturalism is restricted to direct comparison of competing theories or competing methods, with the help of a generalized pattern of means-ends relationships. A broader comparative attitude appears to be required in which the revision of methods and theories is not always dependent on a comparison with clearly defined virtues as evaluative standards.

Moreover, it is not always the case that following the prescriptions of rationality leads to scientific progress. We have clearly seen this in chapter 4, in the example of Pflüger, whose overall approach to scientific problems sometimes worked well but at other times, following the same line, failed to produce satisfactory results. Laudan has given up on truth as a measure of progress. Instead, he has however maintained a strict connection between the

[372] Laudan (1996) pp.210-222.

notions of progress and rationality. I agree with Boon (1983) that this connection sacrifices too much in terms of flexibility in accounting for scientific progress. It blocks the articulation of a more pluralistic theory of learning in science, as envisaged in chapter 4.

Normative naturalism comes close to meeting the demands for an extended naturalist approach to the study of past science. It provides an account of scientific rationality in naturalist terms, as sets of strategies and virtues taken from historical practice. It also incorporates an attractive diachronic perspective for the evaluation of theory choice in the past. However normative naturalism loses its normative appeal when generalizations of means-ends relationships cannot be given. On the other hand, when normative naturalism has to rely on a demarcation criterion, this has the unsatisfactory consequence that the view of learning in science and the notion of progress become too narrow. A middle ground between these alternatives has to be found to retain the good points that normative naturalism has to offer for the study of past science. It is not apparent how this middle ground can be obtained within the programme of normative naturalism.

3. Evolutionary epistemology

3.1 Descriptive and normative interpretations of the analogy between science and evolution

Quite a number of scholars have drawn a parallel between science and evolutionary theory. These approaches are put together in one group here under the label 'evolutionary epistemology'. The idea is to study science by using concepts of evolutionary theory such as variation, mechanism of selection, environment of selection, trial and error, survival and adaptation.[373] Approaches differ from one another in what they consider to be the primary units of selection. Primary units of selection can be theories, methods and cognitive

[373] In Bradie (1986) a distinction is made between two programmes in evolutionary epistemology. The first is the evolutionary epistemology of mechanisms (EEM) and the second is the evolutionary epistemology of theories (EET). In EEM our cognitive mechanisms are studied as operating through biological substrates such as brains, sensory systems and motor systems, which are the products of evolution. If evolutionary theory is correct some form of EEM must necessarily be correct as well. In this chapter we are mostly interested in EET, that is, in a metaphorical application of concepts from evolutionary theory to the study of theory choice. Note that the two programmes need not presuppose one another.

aims. But scientific disciplines can also be seen as the primary units of selection. This variety of interpretation resembles debates in evolutionary theory itself on the issue of what should be taken as the primary unit of selection: species, individual organisms or genes?

Evolutionary approaches to past science also differ in the interpretation of the mechanisms of selection, which operate on the selected units. Selection mechanisms are the concrete causal mechanisms of differentiation, which can explain change in past science. The more these are tied to specific historical contexts, the more naturalist the approach becomes, while with an increase in generality of selective mechanisms, evolutionary approaches to science become more normative.

The default attitude towards evolutionary theory is that it is non-normative. The process of evolution can be described, using a number of analytical concepts, but no qualitative statements are made about the life forms that are studied in evolutionary biology. Life forms merely exist. Their survival depends on how well they adapt to the environment they live in. Life forms succeeding in one environment may utterly fail in another, hence no life form can be said to be intrinsically better than another. Quite a number of approaches to past science, relying on the analogy between science and evolution, reflect this non-evaluative attitude.

For a number of reasons Kuhn (1962) described the analogy between evolution and science as ‘nearly perfect’. Like evolution, science, according to Kuhn, does not strive for predetermined goals. Science does not work towards anything but evolves in reaction to problems, which require solutions, just as species in nature do.[374] Moreover Kuhn could interpret phases of revolutionary science, in which paradigms are in competition, as selection by conflict. As we have already discussed earlier, Kuhn could in this way also work with a notion of progress based on problem-solving capacity, even with the acknowledgement of ‘Kuhn loss’. Still, in 1962 Kuhn added that he was not able to specify in detail the consequences of the alternative view of scientific advance that the analogy with evolutionary theory produces.

In later work Kuhn further developed the analogy.[375] He proposed to interpret the formation of disciplines and subdisciplines as evolutionary branching. Each (sub)discipline must be seen as a species that occupies its own niche. All species have developed strategies of solving particular sets of problems pertaining to the

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[374] This notion of progress without teleology is also what attracted Mayr (1990) in the analogy between science and evolution.
[375] See Kuukkanen (2012).

niche. Kuhn therefore concluded that the problem-solving capacity of science as a whole increases when a rich diversity of disciplines is present. It is literally specialization that is the cause of progress. However it will be difficult to establish what this progress exactly consists of. Contact between distinct fields of study is, according to Kuhn, impossible because the spoken languages in these fields are too different from each other, very much like a bird cannot communicate with a flower. This incommensurability is now seen as a positive force, as it is almost a precondition to be able to handle a wide variety of problems. Still, in terms of the analogy with evolutionary theory, the efficacy of selective mechanisms is more or less restricted to particular disciplines, or paradigms within those disciplines. Kuhn's later views on the analogy therefore do not result in a general evaluative approach to past science.

This also holds for other approaches in which the analogy between science and evolution is used, mainly for descriptive purposes. Toulmin (1972) considered science as a pool of intellectual variants, theories, concepts, methods, and aims that are in constant competition. New variants do not emerge freely in Toulmin's framework but have to appear as suitable candidates in light of specific problem situations. The environment is thus not only selective in judging the competition, but also in the selection of eligible candidates. According to Toulmin sensitivity to concrete environmental contexts is lost in formal approaches to science:

> "An exclusive preoccupation with logical systematicity has been destructive of both historical understanding and rational criticism. Men demonstrate their rationality, not by ordering their concepts and beliefs in tidy formal structures, but by their preparedness to respond to novel situations with open minds—acknowledging the shortcomings of their former procedures and moving beyond them."[376]

The main units of survival for Toulmin are not the intellectual variants but disciplines as a whole, which are formed by institutions, central concepts and shared procedures. Continuity, or identity, of the discipline is ensured by central aims that do not change and by the evolution of concepts, which can be captured in a tree of descent. Only when a discipline perishes will there be discontinuity in science, but until then, the discipline provides a continuous framework within which conceptual change can be studied. The strength of disciplines is determined by the intellectual variants, just as the strength of a species is determined by its members, but disciplines as a whole are in competition for survival in Toulmin's framework. The measure of success of

[376] Toulmin (1972) pp.vii-viii.

species in nature is population growth and Toulmin has applied this to science by looking at the size of disciplinary institutions. Bradie concludes that, in Toulmin's approach to the study of science, normativity is significantly reduced:

"Toulmin argues for a 'local' or 'ecological' concept of contextual rationality. The recognition of the populational nature of concept change leads to the conclusion that there are no universal criteria for rationality or 'global' selection criteria."[377]

A very similar approach to science was developed by Hull.[378] Hull introduced the concepts 'replicators' and 'interactors' to the study of science. Replicators are intellectual variants such as concepts, theories, beliefs, methodological principles and standards of appraisal. These are transmitted via the interactors, which can be individual scientists or research groups. New variants become part of an existing knowledge structure via the interactors. This is why, according to Hull, science is so full of controversy, as through the interactors a competitive selection procedure takes place. As with Toulmin, the main unit of selection is ultimately found on the level of groups of scientists, hence on the level of disciplines. What matters for the identity of the group is, according to Hull, not the immutable content of theories, but the tree of descent that shows the development in intellectual variants. This idea is also similar to what Toulmin had been arguing for. Likewise Hull's model of science exhibits the same relativist tendencies as Toulmin's.

A final example of an approach to science, based on the analogy between science and evolutionary theory, which leads to non-evaluative study of past science, is posthumanism.[379] Pickering (2008), for example, makes explicit reference to 'the evolutionary character' of science. He favours a dynamic model, in which factors of all kinds interact with each other. Through processes of hybridization this leads to everything there is, from the recognition of objects in the world, to the acceptance of scientific theories and the establishment of institutional structures. Yet these 'ontologies' are never finished and always

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[377] Bradie (1986) p.427. Still rational factors have determining force, which is why Bloor (1974) accused Toulmin of 'rearguard rationalism', because, in his view, Toulmin was still endowing rational factors with a special status. This critique, to my mind, says more about Bloor's approach to the study of science in which social factors always have to be dominant than about Toulmin's insistence on the special character of scientific rationality.

[378] See Hull (1982) and Hull (1988). Bradie (1986) p.408 identifies Hull's project as 'Toulminesque'.

[379] See chapter 2 for an elaborate discussion of this approach.

remain open to change, and change comes about through continuous competition.

Latour's actor-network theory is also very much inspired by evolutionary theory. He speaks of networks of alliances that are in competition for domination with other networks and continuously have to face trials of strength. As in the work of Pickering, the notion of hybridization is important for Latour. In processes of mediation between human and non-human actors the selection of intellectual variants takes place. While this interaction between agents is of course central, knowledge claims can be sustained only in networks, so it is on the level of networks that competition and selection ultimately takes place.

A number of interesting parallels can be drawn between Latour's theory of science and what evolutionists, such as Dennett, have had to say about cultural evolution.[380] The notion of 'meme', the cultural equivalent of a gene, resembles Latour's idea of non-human agency. Dennett has expressed the idea that both natural and cultural evolution are continuous processes of bricolage and hybridization.[381] Meaning is determined through networks of relations, and these are constituted through manipulation (Dennett) or mediation (Latour). Finally following Dennett (2010), he is, very much like the posthumanists, not willing to take the social for granted as an independent analytical category.

The number of similarities between these thinkers is curious because Dennett has also expressed strongly normative views about science and these cannot be brought into accordance with Latour's actor-network theory. The brief consideration of the way Kuhn, Toulmin, Hull, Pickering and Latour have articulated the analogy between evolution and science has shown that these are not very helpful for evaluative historiography. In these approaches, either the selective mechanisms become highly context-specific, or the eventually selected intellectual products cannot be evaluated any further. As the comparison of posthumanism with the ideas of Dennett already showed, others have combined the analogy between science and evolution with a more pronounced evaluative attitude. From these approaches we must extract a suitable candidate for extended naturalism and consequently assess whether this approach can satisfy the demands for a sophisticated evaluative historiography.

Selective mechanisms operate on defined units of selection. But what are the selection criteria? After all the fittest survive for some reason. If it is possible to improve on the means of survival then some measure of progress can be established. We can simply ignore the question whether this form of progress

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[380] This is also pointed out in Greif (2005).
[381] Dennett (1999).

leads to the truth about the world and just inquire which 'goods' need to be promoted in order to solve problems of survival. Evolutionists have seen science as a problem-solving activity that embroiders on earlier problem-solving activities that the human species, and their evolutionary forebears, needed to engage in order to survive. Popper for example once said that "science, or progress in science, may be regarded as means used by the human species to adopt itself to the environment."[382] In this view, there is no principled qualitative difference between lower- and higher-order species, something Popper captured with the slogan: "there is, as it were, only one step from the ancestral amoeba to Einstein."[383]

Both Dennett and Giere have argued that the properties that are good for survival, are also the virtues we value so much in scientific theories.[384] Theoretical virtues such as simplicity, empirical adequacy, predictive power and problem-solving effectiveness are also the virtues we need to promote in order to survive. In epistemology grounds for knowledge are sought and found in definitions of truth, rational decision procedures, or as in social epistemology, in social structures. These answers are almost invariably questioned for further grounds as they lean on *a priori* assumptions, which require a specification of grounds in their own right. Often this boils down to a further historicization of the definition of knowledge. Dennett (1999) argues that evolution itself simply provides the bottom turtle here.[385] It stops the reduction from higher aims to lower aims, and directly meets the demand for historical anchoring, because the evolutionary process spans the whole of history. The elegance of this idea is that evolution itself is not goal-directed. Yet in the process of evolution goals, intentions and purposes emerge. It is on these things that further selection takes place.[386]

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[382] Popper (1975).
[383] Popper (1972) p.347.
[384] Giere (1985) and Dennett (2010).
[385] See also Dennett (2010) in which he argues that natural selection is the process behind everything.
[386] Briskman (1977) p.532 has in a similar vein argued that we start out with practical aims and only later move to more theoretical ones. These higher aims are bootstrapped on the more practical ones. See also above. Like Briskman both Dennett and Giere have argued that we improve on our aims by improving in terms of 'tools for thinking' (Dennett) or 'cognitive resources' (Giere). The availability of resources simultaneously delineates specifics of the historical context and sets out the room for improvement in that context.

Evolutionists can in this way provide a fully naturalist answer to question where the virtues and aims in science come from.[387] At the same time one can use the evolutionary analogy for evaluative purposes and compare scientific products with respect to these virtues and aims. Considerations in terms of the promotion of such particular virtues and aims are however missing from the well-known normative evolutionistic approaches to the study of science stemming from Campbell and Popper.

Campbell took scientific theories as the blindly generated variants upon which selection takes place.[388] He also stressed that an independent characterization of the relevant environment of selection was needed to make the analogy between science and evolution work. Yet he did not specify clearly how the interaction between environment and selection should be understood. Nor did he specify how exactly selection of variants takes place. He merely suggested that a critical attitude of scientific communities ensured the selection of theories.

Popper's interpretation of the analogy was similar to Campbell but articulated in more detail. Like Campbell, Popper took theories to be the primary units of selection, and also like Campbell, he made a point of the blindness of variation. He thought that this would most closely resemble Darwinian evolution, as he did not want to allow for 'Lamarckian instruction'.[389] For Popper it followed that the context of discovery, in which new variants emerge, could be ignored. In the context of justification he proposed his falsification theory. Hypotheses make empirical predictions and these predictions can be put to the test. If the hypothesis fails, it has to be rejected. By analogy, this is then how natural selection in science operates.[390]

Theories that have survived critical tests are not permanently established, as the possibility remains that they will be falsified in the future. But there still is a notion of convergence to the truth, as rejected theories are certainly false. Hence, if we imagine the set of all possible scientific theories occupying some space, this space decreases whenever a false theory is rejected. The exclusion of what is false brings us closer to the truth.[391]

Popper's view of science is however too formalistic for our purposes. The context-independent mechanism of falsification does not lead to a deep understanding of the notion of error, as we saw in chapter 4. Rejection of

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[387] Note that Kitcher (1992) p.76 argues that it is a task of naturalism to articulate the notion of epistemic virtue, because it is against this that improvements are to be judged.
[388] Campbell (1960).
[389] Popper (1976).
[390] Popper (1972), Popper (1984).
[391] Popper (1984) p.239.

falsified theories is also too destructive. For example, it does not allow for the evolutionary notion of adaptation to new challenges. Holistic arguments have questioned Popper's assumption that hypotheses can be tested independently from other hypotheses, which led to the theory-tradition in philosophy of science sketched in chapter 1. Such criticisms to Popper's philosophy are basically correct, well known and need not be repeated here.

With respect to evolutionary theory it is perhaps curious that Popper decided to insist on the blindness of variation and selection. Darwin himself allowed for both random and more conscious forms of variation and selection, for example through the work of a breeder. Given the idea that higher-order intentions and motivations can be grounded on other forms of pursuit in evolutionary history, these intentions and motivations can be used as selection mechanisms in their own right. Interestingly, Machamer has argued that this level of selectivity has to be incorporated in historiography of science: "intentionality and the mental, or their teleological analogues, as an essential part of selectivity, are an ineliminable part of history of science, however done."[392] But this appears to require taking insights from cognitive psychology on board, and this is something that historians of science have been reluctant to do.[393]

In any case, it is clear that strongly context-independent methods of selection take us too far from naturalist study of past science. It is however not easy to arrive at an interpretation of the analogy between science and evolution that allows for a multitude of selective mechanisms, while retaining sufficient normative force. Boon (1983) is a less well-known Dutch publication, which in my view contains an interesting attempt to meet this task.[394]

Boon argued that we must make a distinction between local and global levels in science. The forces, i.e., selection mechanisms, operating on the local level are different from those on the global level. On the local level there must be room to try out many new variants in order to solve concrete, and often practical, research problems. As with any normative advice, the actual course of things can differ. On the local level scientific research has to be allowed to wander freely, even if this means wandering along, with hindsight, less fruitful pathways. Not allowing this is bad for scientific development.[395] The more

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[392] Machamer (1994) p.149.
[393] Which is considered in this thesis as counter-productive. See chapter 4 on the approach to the notion of error from the perspective of uncertainty. And see chapter 3 on the charity-based interpretation procedure. See also chapter 7.
[394] There are similarities to the natural selection model given in Richards (1981).
[395] It seems to me that this is still a relevant message in the neo-liberal climate we currently live in, in which control over local levels of research has significantly increased,

global the level, the more conservative selection mechanisms should become. Only at the global level can new knowledge claims become fully accepted, and this cannot be granted to every passing whim. As selection takes place in specific scientific disciplines Boon advices to follow visible manifestations of selection, which from the local to the global run from: selection by individuals, local group meetings, internal reports, journal articles, peer reviewed articles and finally textbooks.

In order to properly understand the scientific process, all moments of selection need to be taken into account and interrelated. According to Boon, other interpretations of the evolutionary analogy suffer from the shortcoming that they highlight only parts of the selection process. The laboratory study of Latour and Woolgar (1978) for example, is interpreted by Boon as at best a partial account of the workings of science, because only a local part of science is addressed. In his view, the book cannot serve as a blueprint for all science because it has to be supplemented with an account of how science works at more global levels. It is only through the connection between all these levels, *and* because of the conservatism at the most general level, that a field of study obtains coherence, stability and unity.

I find two things attractive in Boon's application of the analogy. The first is the idea that there is not just one decision moment in science: as there are many selective mechanisms in play, which are interrelated in complex ways. Still, accepting this degree of complexity does not lead us to give up on evaluation, because Boon tells us in what way the selective mechanisms ought to be applied. Secondly, he suggests that we should capture the interplay between the local and global levels in science in terms of a gradual development of ideas and theories within research programmes. According to Boon research programmes go through distinct phases of development, which demand different styles of research and the pursuit of different kinds of virtues.[396] Systematic knowledge about what the best strategies are, given typical phases of research, can serve as a basis for evaluative historiography.[397]

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through intensive selection procedures in relation to the acquisition of funding, and higher demands set on the future research output.

[396] He has used Douglas' group-raster theory in order to distinguish between four phases within distinct characteristics. I will return to this theory in the next chapter.

[397] This diachronic perspective on past science is further developed in chapter 7.

3.2 An assessment of evolutionary epistemology for historiography of science

The evolutionary analogy turns out to be very flexible. Even mutually exclusive views of science can be found under its umbrella. In the relativistic approaches the unit of selection lies at the phylogenetic level, in disciplines, paradigms or networks. Selective mechanisms do work on intellectual variants but it is the larger units of selection that really matter. In the approaches with a stronger evaluative dimension the main unit of selection is the theory.

As this dissertation mainly focuses on the evaluation of theory choice and theory change, we need to consider the analogy between science and evolution in the latter way. Larger units, such as scientific disciplines, can be seen as the result of selective processes as well, but this need not be taken as primary in order to study theory choice in terms of natural selection.[398] Disciplines, paradigms or networks can function as the selective environment in which the selection of intellectual variants takes place.

The most promising interpretation of the analogy between evolutionary theory and scientific development works with a layered structure in which a multitude of selective mechanisms operate. This interpretation offers a number of benefits for the study of past science. It allows us to account in an elegant way for both the conservative and progressive forces in science and how these interact. The approach is compatible with the virtue approach to rationality, which I consider to be basically correct. This was also the case with normative naturalism, but in addition evolutionism provides a deeper naturalist answer to the question where the desired virtues in science come from. Furthermore one does not have to pose a line of demarcation line between social and rational factors from the evolutionary perspective. Social and rational factors are part of the same evolutionary process. One of the advantages of this is that scientific development can be accounted for in all its dynamic aspects.

Alas, as with normative naturalism, a number of problems remain. The most problematic is the uni-directionality of the evolutionary approach. As Kuhn said: “scientific development is, like biological evolution, uni-directional and irreversible.”[399] It follows that once theories are rejected, they will never return. It is thus difficult to account for theories that were first rejected and only (much) later accepted in the history of science. Examples are Huygens’ wave theory of light, Benjamin Thompson’s vibration theory of heat, Semmelweis’

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[398] I defend a biological hybridization perspective on scientific disciplines in Karstens (2012).

[399] Kuhn (1970) p.264.

theory of puerperal fever, Polanyi's theory of absorption, Bohr's complementarity principle, Wegener's mobilist theory of earth continents, and even Darwin's theory of evolution.[400]

Rheinberger has broadened this critique and argued that the evolutionary approach can also not account for the merging of paths in the history of science. Evolution can follow only one path of development and cannot incorporate 'the paths not taken'. In his view the process of science is better conceived of as "a meshwork of shorter or longer paths, at times diverging from each other and ending nowhere, but others again merging into each other."[401]

Uni-directionality thus poses a problem of historical adequacy. But it is also problematic in relation to a theory of learning in science. In evolutionary approaches we can learn only through selection mechanisms. But selection takes place in historical contexts, which happen only once. It is hard to account for the notion of retrospective error, because this requires the attribution of errors with hindsight. It is not clear how this should be done in an evolutionary framework, because of its uni-directionality. While it is possible to account for improvements within one lineage of descent, it is not straightforwardly possible to comparatively assess problem-solving effectiveness across evolutionary branches. When similar aims are present in more than one discipline one would like to be able to compare the means used to achieve the aims. Such a possibility for a more general, or more typical, analysis, does not present itself in the evolutionary framework. A broader comparative approach is needed to accomplish this.[402]

According to Boon (1983) such considerations show that the analogy between science and evolution has its limits. While he supports an evolutionary view of science as the best way to account for the dynamics of its development, as well as its progressive nature, he also stresses that the desirability of organisms in principle cannot be assessed. Biological organisms just exist: whether this is good or bad depends on criteria outside evolutionary theory. A question for more evaluative ground is perhaps out of place in biology, but for the study of (past) science it is not. Evaluative historiography of science requires a broader comparative ground than evolutionary epistemology has to offer.

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[400] Examples repeated from chapter 4.
[401] Rheinberger (2009) p.89.
[402] Pyenson (2002) p.6 argues that the comparative method can illuminate divergent paths of evolution. See also section 4.2 below.

4. Towards a suitable form of extended naturalism

4.1 Normative naturalism versus evolutionary epistemology

In the previous two sections we have analysed two candidates for extended naturalism. Both contained a number of attractive points, but as a whole normative naturalism and evolutionary epistemology fell short for our purposes. The two frameworks should not be treated as complete opposites as they have a lot of things in common. In both there is a focus on concrete research problems and on concrete practices of argumentation. Both approach the notion of rationality in terms of virtues, and in both assessments with respect to these virtues can be undertaken from a diachronic perspective. All of these are attractive points.

The advantage of normative naturalism over evolutionary epistemology is that cross-disciplinary and cross-temporal comparison of means-ends relations is very well possible. Normative naturalism therefore does not fall prey to the problem of uni-directionality. The advantage of evolutionary epistemology over normative naturalism is that it does not need to demarcate between rational and social factors at some point in the analysis of theory choice.[403] The evolutionary approach can therefore more appropriately capture the dynamics of science.

4.2 Extending the comparative horizon

If we want to combine the best of both worlds, we must find a way to connect the non-demarcationist approach of evolutionary epistemology to a broader comparative platform. Whether comparativism deserves a status as a distinct form of extended naturalism remains to be seen. It is naturalist because in comparing aspects of one historical context to another, one does not transcend these contexts. Yet, because of the comparison, one does achieve more than with a focus on one particular historical context only. The guiding idea is that *contrasts* make things clear.[404] As Machamer put it: “It is the contrast with alternatives, both preceding and simultaneous, that ground any claims to

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[403] Remember that weaker variants of normative naturalism have difficulty providing more than just domain-specific lists of means-ends relationships.
[404] Cf. Feyerabend’s dictum: “Prejudices are found by contrast, not by analysis.” Often the counterfactual is needed to explain the factual. A comparison with what did not happen may shed light on the conditions for what did.

necessity and pull the historical narrative away from the merely correlational or from chance."[405]

However, comparative elements are clearly present in both normative naturalism and evolutionary epistemology. In evolutionary epistemology the fitness of intellectual variants is determined in competition with others. It falls short as an approach to the history of science because we *also* want to be able to compare historical situations and the choices made in those situations. The general comparative procedure that is required for this looks a lot like normative naturalism.[406] Comparative historiography is a three-stage process consisting first of a selection of parameters, which are the units and criteria of comparison. Secondly, there is the act of comparison itself. The third stage consists of an evaluation of the results, which must include a feedback on the assumptions made in the first step. Evaluations cannot 'just' flow from the comparisons, because we need to know in advance on what criteria to compare. Hence some prior guidance is required. In order to count as a naturalist approach, the parameters in step 1 should however be derived, in normative naturalist fashion, from the past itself. They should thus be open to empirical testing via the feedback provided in step 3. Considerations in this respect are similar to the issue of updating means-ends relationships in normative naturalism.

Comparable ground must however be more broadly conceived than is imagined in both normative naturalism and evolutionary epistemology. The comparative approach has a number of benefits in store for historiography as a whole. The determining factors in history can be very well analysed through the comparative method because it allows us to identify the general and the particular. Comparativism has been defended as the ideal way to deal with causal complexity in the social sciences (Ragin 1987). As Machamer put it:

> "an historian does not know how to reasonably discriminate an element of society, e.g. science, and how it works unless one understands how it is similar to and different from earlier phases of something that might reasonably be called that same element, as well as how it interacts with the co-temporal elements of society, which allow it to be seen as different."[407]

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[405] Machamer (1994).
[406] It is, for example, not a coincidence that Shrader-Frechette (2006) p.817 ranks Laudan as the most comparativist philosopher of the 'historical school'.
[407] Machamer (1994) p.159.

The comparative method can be beneficial to historical understanding through the mirror effect: historical episodes (or past and present) can both become clearer when compared to each other. This can for example lead to conceptual clarification. Comparison is also a way to test historical hypotheses, and can function as an equivalent of empirical testing of hypotheses in the natural sciences. As such it can have an objectifying force on the level of theory formation in historiography.

Not all these attractions of comparative historiography are directly relevant for assessments of past science. Still, in the absence of a transcendent measure of progress, we must rely on comparative evaluation. Note that this was already a central theme running through chapter 3: charitable interpretation of past science is deeply comparative in nature. The virtue approach to rationality, which will be defended in detail in the next chapter, has to be understood in terms of comparison as well. The platform that is given in chapter 7 must be interpreted as providing the adequate amount of comparative ground that is needed to properly carry out evaluative historiography.

4.3 The failure of the old project of comparative historiography of science

Before we turn to the more detailed articulation of the desired extended naturalism, I want to take away the possible fear that a plea for a more extensive comparative platform in historiography of science will take us back to positivist approaches to past science. After all, isn't comparative historiography a once tried project that has utterly failed? The old comparativist project in history of science, undertaken by Edgar Zilsel and Joseph Needham, has indeed received a bad press. It must be seen as a prisoner of neo-positivist tendencies (Pyenson 2002) and as a consequence it has suffered from the accusation of European triumphalism and exceptionalism (Teich and Young 1973). Furthermore comparative questions have mostly been of a very general kind, leaving other interesting possibilities for comparison untouched. For example, most comparative historiography of science involves a comparison on the level of countries. Further the main question Needham posed, 'Why did the Scientific Revolution not happen in China?', could not be answered satisfactorily.[408] The

[408] See Teich and Young (1973). Diederick Raven was so kind to send me an unpublished paper of his in which he claims that Needham subscribed to a 'tributary river' model of science. All parts of the World in all of history could contribute to the main River, including the more practical or technological achievements. This did not fit the search for a major break in development between the West and the East in the 17th century at

untimely death of Zilsel in 1944 was also unfortunate because it did not allow him to develop the comparative approach in sufficient detail.

Other approaches to past science, which have sprung up from the beginning of the 1960s onwards, have almost completely elbowed out the comparative approach. According to Pyenson (2002) the comparative approach is denied even the dignity of marginality. While it is true that comparative studies of past science have continued to appear over the years, this has decidedly been in 'underground status'.[409] In recent years a number of books based on an explicit comparative methodology have appeared (Lloyd 2009, Cohen 2010, Bod 2013) but it is too early to speak of a revival of comparative historiography of science.[410] In general it seems that the approach is still strongly held back because of the association with eurocentrism and neo-positivism.

This association, however, can and must be seriously questioned. The comparative method does not have to be restricted to strongly value-laden macro-historical questions, such as the Needham question. It can also be used for conceptual clarification and fleshing out causal relations in a far more neutral way. The comparative method has been worked out theoretically in much more detail over the years for the social sciences (Mandelbaum 1980, Skocpol and Somers 1980, and especially Ragin 1987) but also for historiography (Tilly 1984, Lorenz 1999). Historiography of science can possibly benefit from these works.[411]

In the present endeavour the focus is on assessments of past science and we need to consider the role comparative evaluation should play in them. This role is in my view substantial and indispensable. As the comparative evaluation procedure can, to a large extent, be grounded on naturalist terms, charges of Eurocentrism and neo-positivism do not apply to it.[412] More serious criticism

all. Needham's approach to past science thus suffered from major conceptual difficulties, which is one of the reasons why he could not answer his main question. See also Raven, Crohn and Cohen (2000).

[409] Pyenson (2002) p.4. Pyenson's paper offers a fairly exhaustive overview of comparative historiography of science that had been produced up to 2002.

[410] An earlier programmatic paper is Jacob (1999). Her aim is to move beyond social constructivism in turning towards comparativism.

[411] See also Pyenson (2002) for an overview of theoretical explications of the comparative method.

[412] Agassi (2008) has argued against such naturalist interpretations of comparative evaluation and stated that the comparative approach in general cannot cope with the issue of approximation to the truth and this, according to him, makes it philosophically unsatisfactory. As I believe that truth approximation is not a useful way to account for progress in science this criticism against the comparative approach is moot.

has been voiced in Roberts (2009). She dismissed the comparative approach because, according to her, it forces the past too much in pre-determined categories. She argued that the comparative approach assumes similarities between historical contexts that may simply not be there, as every historical context is different.

Another problem with comparativism is that it loses sight of the dynamics of science. Roberts' own posthumanist strategy of following the actors does not suffer from these shortcomings. The general posthumanist critique on comparativism is that it carves up the past too much, prior to empirical investigation, and thereby loses sight of the concrete dynamics of history.

In reply I admit that the comparative approach should indeed be judged by its ability to produce novel insights and not just confirm what has already been assumed prior to research. Also, a feedback mechanism on the assumed units of comparison and evaluative criteria should be incorporated in the approach. Further I think that the strategy of following the actors does not have to be counterposed to comparative historiography, but can be complementary to it. Transfer history and comparative history may constitute one and the same project.[413] Moreover, it was already concluded above that the extended naturalism we are after has to be found in a combination between evolutionary epistemology and an extended comparative approach. It is the task of the next chapter to articulate this extended naturalism in more detail.

[413] This is argued for in Lorenz (1999).

Chapter 7 A Platform for Evaluative Historiography

1. The constituents of the platform

In the final chapter of this thesis, I will defend a version of extended naturalism, which offers a stronger basis for comparative evaluation than the approaches discussed in the previous chapter. In order to achieve this result we need to set up a platform that can serve as comparative ground.[414] Kuhn once correctly observed that: "The Archimedean platform outside history, outside of space and time, is gone beyond recall. In its absence comparative evaluation is all there is."[415] But it is unclear whether he sufficiently appreciated that a more modest platform *is* required to make any procedure of comparative evaluation work. In a recent paper, Hasok Chang clearly realized this when he advocated a turn to a more judgemental historiography of science:

> "The question is: which part and which version of the present do we choose to take as our platform? And as usual, freedom comes coupled with responsibility. We historians need to face up to the implications and consequences of the judgements we do and must make. I am much happier to accept that burden of responsibility, than to hide behind a murky notion of neutrality."[416]

Chang's paper however, does not even begin to specify what the constituents of such a platform have to be. Therefore his exciting question, 'what to choose as our platform?', is largely left unanswered.

In the terms of chapter 2, a platform can be interpreted as a set of resources. Approaches to past science can be ranked on a scale running from larger to smaller platforms. Which type of historical questions can be addressed, and hence which type of historiography is produced,

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[414] Consider also Bevir's idea on how to deal with conflicting historical interpretations: "If we disagree about the relative merits of different views, we should draw back from the point of disagreement until we can agree upon a platform from which to compare them." Bevir (1999) p.153.
[415] Kuhn (1991) p.14.
[416] Chang (2009) p.254.

depends on the selected resources. What has been made clear in chapter 2 is that, with the extension of the principle of symmetry, elements previously recognized as resources for explanation have become objects of explanation. Hence, in the past few decades, the platform of resources in historiography of science has gradually decreased.

This holds also for posthumanist approaches to past science. It is true that through the use of the network concept, and by focusing on the interaction between agents, posthumanism is no longer tied to specific historical localities or self-contained social structures. Thus posthumanism can avoid the problems of localism that social constructivist approaches to past science, such as SSK, face. However, it is important to realize that this extension of scope, from the local to the translocal, does not help us much in setting up a platform for extended naturalism. Because of the strict focus on the interactions between agents, posthumanist accounts of past science never reach a level from which a comparison between local contexts, distinct in space and/or time, is possible. Furthermore, the posthumanist approach makes use of only a limited set of analytical concepts, deliberately excluding normative concepts from this set. The whole point of extending naturalism is to offer accounts of past science that include a normative dimension in a way that is both acceptable and productive.

There is of course a relation between geographical and temporal scope of historical research and the possibilities of evaluativeness. While I do not think this relation is one of necessary dependence, it must be clear that an evaluative historiography that is based on some comparative procedure requires a translocalist scope, for the simple reason that only this scope allows for comparison between distinct historical localities. Therefore, section 4 below is devoted to an articulation of a diachronic 'zoom' on the past that seems to me the most fruitful for evaluative purposes. It is especially in this context that the shift to the perspective of uncertainty, discussed in chapter 4, will turn out to be useful.

From the discussion so far it can be inferred that I do not believe that an irenic view of the notion of platform can work. On this view we just select the appropriate platform to answer particular types of questions. The idea is that all these types of historiography bring their own merits and insights into the past. However, we cannot simply choose the world we want to live in. The existing approaches to past science differ from one another because they rest on incompatible views on what counts as knowledge.[417] Suppose two accounts of the same historical episode are written from two of these incompatible perspectives. It is hard to see how we could add up the results of these two accounts. If we still granted all approaches to past science equal legitimacy this would result in overall incoherence of the historiography of science.

Thus, throughout this thesis the general argument has been framed in polemic terms. What we seek is a golden mean, which retains the positive aspects of *prima facie* incompatible approaches to science, but overcomes the negative aspects. This requires cracking a couple of hard nuts. I am certainly not arguing that all historians of science will have to start doing the same kind of work. Historiography of science is a very rich field, and a great variety of questions can and should be posed and worked out, both in detail and in more general terms. Just to mention a few: we want to know how the public reacted to particular developments in science and technology, but also how the public in general reacts to new challenges. We want to know why the same theory was rejected in one particular historical context and accepted in another. We want to know how new ideas and discoveries came about, but also think about what the favourable circumstances for scientific discoveries are in more general terms. We study the past to gain insight into how possession of knowledge relates to social control, etc. Not all of these questions directly involve the issue of evaluation of past contributions to science. However, ultimately all these different historiographies must recognize the same determining factors and, when it comes to assessments of past contributions to science, must hold on to, or be made compatible with, the same evaluative procedures.

In the previous chapter, two candidates were considered to function as a golden mean, namely normative naturalism and evolutionary epistemology. In this chapter we build on the positive aspects of *both*

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417 If we can give all possible determining factors in the history of science it is possible to classify all approaches to past science on the basis of their respective valuation of these factors. I did so in Karstens (2011b) and half-jokingly added that the total number of possible approaches to past science is 120. More seriously, I defended that we can lump together the existing approaches into four groups, and that these groups are mutually exclusive.

approaches. These involve first, a focus on concrete historical situations and the way research problems, competing solutions and the arguments supporting them, manifested themselves in these situations. Second, they both follow an approach to the notion of rationality in terms of virtues, including the possibility of making assessments with respect to these virtues from a diachronic perspective. This will all be taken on board in this chapter. However, the problem we had with evolutionary epistemology, its uni-directionality, needs to be avoided. The uni-directionality makes cross-temporal and cross-disciplinary comparison hard, at least much harder than in normative naturalism. Yet, in order to avoid the problem of producing long lists of highly specific optimal means-ends relationships, normative naturalism has to resort to positing a demarcation between rational and social factors somewhere in the explication of past science. This too we have found uncomfortable. The advantage of evolutionary epistemology over normative naturalism is that it does not lead to posing a demarcation between rational and social factors in the explication of past theory choice.

It was argued in chapter 6 that, in order to combine the strengths and avoid the weaknesses of these two forms of extended naturalism, we need to extend our comparative basis. This is done in this chapter setting up a platform. This platform can be interpreted as representing the comparative ground to facilitate evaluations of past science. It consists of the following constituents: a set of elements to define the concept of rationality (worked out in sections 2 and 3 below), an interpretation of the past in terms of alternating research programmes, which facilitates diachronic historiography (section 4), and a specification of conditions under which the use of anachronisms and present-day knowledge is not only acceptable, but also supportive to a programme of evaluative historiography of science (section 5). The elements of the platform can be open to empirical test but need to be well chosen (as I believe they are) because: "… the mechanism of self-correction may be fairly inefficient, it is important to start with a good first approximation."[418]

To borrow Chang's formulation, the platform represents a 'version of the present' as in the specification of all the constituents there is an element of presentism involved. This is obvious in the use of

[418] Giere (1988) p.16.

anachronisms and present-day knowledge. But also for the perception of the history of science as a collection of research programmes we need to make use of the benefit of hindsight to determine where programmes have started and ended. For example, historical actors involved in the first steps of a research programme could not foresee where and when this programme would end. I defend the thesis that historians working in the present can identify the beginning and endpoints of past research programmes and that it is fruitful to use this knowledge for historical interpretation because it allows us to consider the stages of development of the programme as a connected whole, instead of as isolated events. Finally, a thin element of presentism is also contained in the virtue approach to rationality, namely on the type level (for explication see the next two sections).

Taken together all these elements and shifts in perspective are strong enough to disarm the arguments against evaluative historiography *and* finally leave a number of persistent positivist assumptions about science behind us (cf. the analysis given in chapter 5). The fascinating result for historiography of science is that it leads to new ways to study the past in which there still is much to explore. This holds for example for the phenomenon of error: much more insight in the ways to err as well as the ways to overcome errors can still be gained. As we have seen, one could go as far as to argue that the phenomenon has not so far been properly studied (cf. the analysis in chapter 4). Further, the virtue approach has, as far as I am aware, never been at the centre of attention among historians of science.[419] This is unfortunate, because it is especially through historical study that we can learn more about patterns of inference related to preferences for virtues, and from this about the possible normative force of these patterns. Again there is still much to explore for historians of science in this direction.

2. Approaches to the study of science and the pursuit of virtues

The main problem of both the older, but also the more recent and more nuanced, evaluative approaches to past science has been the dominant hierarchical 'either-or' way of thinking. Invariably these approaches involve a statement of demarcation between science and non-science, mostly based on a definition of scientific rationality. The

[419] In philosophy of science this is different of course: see next section.

non-evaluative reaction to this way of thinking has been to exclude rationality as one of the primary determining factors of theory choice. In these approaches, what counts as rational behaviour is dependent on other factors. I believe that this has been an overreaction and hence that we can (and should) include rational factors as primary determiners of theory choice in our total set of determining factors.

What we need for this is a shift in perspective towards a non-absolutist 'more or less' mind-set. I turn to an interpretation of the concept of rationality in terms of the pursuit of virtues in order to make this shift in perspective work. Virtues are not predicative on theories but *attributive* because no fixed values can be attached to them. Something is, for example, simple only in relation to more complex things and complex in relation to simpler things. Thus the virtue approach allows for comparative assessments, even in the absence of absolute standards with respect to the individual virtues. Virtues can be defined as objective qualities that are considered to be good. Virtues thus carry normative force. But because preferences for virtues can differ, understanding the pursuit of virtues also has to rely on description, and this requires historical scholarship.

A number of philosophers of science have suggested considering notions of progress and rationality in terms of virtues. In order to see how my approach differs from these proposals, let us first briefly take a look at a few important articulations of the virtue approach. Thomas Kuhn argued that we should foremost look at the promotion of theoretical virtues in periods when *changes* between paradigms occur. According to Kuhn, an upstart competitor can become accepted only when it promises to score better on five virtues: accuracy, consistency, simplicity, fruitfulness (in the sense of suggesting new experiments) and scope. Historical actors must rely on these virtues in theory/paradigm choice because other criteria cannot be applied yet.[420] Virtues can thus, according to Kuhn, be used for interparadigmatic comparison.[421]

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[420] Kuhn (1977). The notion of paradigm is at times notoriously vague, hence a paradigmatic shift can in some cases be equivalent to embracing a different theory.
[421] In Kuukkanen (2009), Kuhn's later insistence on the five virtues is interpreted as the rationalist aspect of his philosophy. To place Kuhn in the tradition of rationalism is however controversial.

As we have seen in earlier chapters, Laudan proposed to account for theoretical progress through a comparative weighting of virtues. While he recognized some other virtues other than problem-solving capacity, such as consistency, falsifiability, empirical adequacy and predictive success, problem-solving capacity ultimately carries the decisive normative force in his framework.[422] In Lipton (2004) theoretical virtues are again used differently, namely in the form of abduction: a possible explanation must be considered right when it best explains the evidence. Lipton argued that what the best explanation is can be comparatively established using scores on virtues such as scope, simplicity, precision, explanatory force and unification.

These three authors make use of virtues for particular reasons. I believe however that evaluative historiography based on a virtues approach does not have to confine itself to any of these particular usages. It is not apparent why virtues have to be restricted to the best explanation of the evidence, as in Lipton's approach. Virtues can for example also play an important role when evidence is not yet sufficiently available, as Kuhn has argued. Preference for certain virtues might also come first and in part *determine* what counts as evidence. Likewise, the virtue approach in my view does not have to be restricted to the study of paradigm changes only. One of the advantages of the virtue approach is that it facilitates interparadigmatic comparison in a much wider sense. Finally, as argued in the previous chapter, Laudan's hierarchy of virtues is also difficult to accept, as it can be rational to prefer virtues other than problem-solving capacity.

The authors also work with different sets of virtues, although there is considerable overlap between them. A more exhaustive list of virtues would contain empirical adequacy, accuracy/precision, consistency, simplicity, fruitfulness/heuristic value, scope, falsifiability/verifiability, predictive success, problem-solving capacity, coherence and explanatory force (including integration, unification and specification of causal mechanisms). I think that for the purposes of evaluation we should not be restrictive, but work with an extensive list of virtues. All these virtues must be considered as possible selection criteria in situations of theory choice. Historians can in this way make use of virtues in order to study how theory choice was determined in the past, especially when choices needed to be made between several competing

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[422] Laudan (1977), Laudan (1996).

alternatives. But next to this, the same approach can be used also to compare past theories to later theories, including current ones. With this second application it is possible to make assessments of past science in the long(er) run. The difference between the two levels of comparison is only in interpretation and valuation of virtues, on what below I will call the 'occurrence' level. The comparative procedure in both cases is essentially the same.

From the 'more or less' perspective, assessments of past science are never absolute: they can be made only relative to alternatives. From this it follows that the notion of truth does not play a role in our evaluative procedure. The approach defended here therefore circumvents the main issue of debate with respect to virtues in philosophy of science, which has centred on the question whether we can draw a distinction between virtues that are truth-indicative, which are then called epistemic virtues, and virtues that are further indicators of the credibility of theories, the so-called theoretical virtues. Van Fraassen (1980) for example has argued that theoretical virtues are merely pragmatic criteria: they characterize the convenient use of a theory. Simplicity is an example of a theoretical virtue, as simple theories are easier to handle than complex theories. In Van Fraassen's view epistemic virtues such as consistency and empirical adequacy offer firm grounds for theory choice whereas theoretical virtues offer only further indications for theory choice.

A number of arguments have been put forward that downplay the importance of the distinction between epistemic and theoretical virtues. First, a characterization of the concept of empirical adequacy seems to involve reference to theoretical virtues. If this is the case, then theoretical virtues also promote epistemic virtues (Psillos 1999). Second, the problem of underdetermination will be practically impossible to solve without theoretical virtues. In situations of theory choice in which competitors score equally well on epistemic virtues, tiebreakers are needed to force decisions. If theoretical virtues are more often than not indispensable to force decisions, they are more than just pragmatic criteria they carry epistemic force.[423] In short, the gist of these arguments is that the distinction does not hold because theoretical virtues also produce epistemic effects, i.e. they are also truth-conducive.

[423] Schindler (working paper).

These arguments can however be met by counterarguments. First, there is no clear demonstration that possession of theoretical virtues systematically promotes epistemic virtues.[424] Second, theoretical virtues are vague. Because they lack a sharp definition, the application of theoretical virtues depends on contextual as well as personal preferences. Third, there is always more than one theoretical virtue at stake. Theoretical virtues that are not compatible pull theory choice in different directions. This is much less apparent with epistemic virtues. If observable consequences of a theory fly in the face of the consistency of a theory, one has to adjust the theory, an operation different from making a choice between two competing alternatives promoting different virtues. If this holds, it provides a strong indication that the epistemic and theoretical virtues are indeed different in kind.

My take on this debate is the following. I grant that theoretical virtues are vague and that their application depends on contextual preferences. But this holds also for epistemic virtues. Observable phenomena can always find accommodation in more than one theory. Next to this parties may also disagree about what the relevant observable phenomena are, or how to weight the relative importance of different observable phenomena. All this introduces a degree of vagueness to the notion of empirical adequacy. An appreciation of this virtue depends on how it hangs in the balance with other preferences (most likely given by the other virtues). I am aware that this makes the problem of virtues pulling theory choice in different directions even greater. I discuss this pulling problem in the next section.

I think we should downplay the distinction between epistemic and theoretical virtues, but not for the reason that theoretical virtues are *also* truth conducive.[425] In my view, truth cannot function as an explanatory category, as the truth about a scientific theory must be inferred from others things, which include *all* the virtues under discussion. But instead of seeing truth as a derived result, and hence virtues as truth indicators, I believe it is better to replace the notion of truth altogether with the notion of certainty. Truth is an absolute verdict: something can be only true or untrue; there are no options in

[424] For example in Gijsbers (2011) pp. 40-45 Lipton's approach is criticized for failing to provide a link from explanatory virtues to epistemic virtues, or from loveliest to likeliest explanation. Gijsbers isolates each virtue and then criticizes them for *individually* falling short as epistemic categories.
[425] Kitcher (1993) for example calls theoretical virtues 'truth-indicators'.

between. No matter how good a theory is, there is always the option that it can be replaced by a better alternative. It we need to leave this possibility open at all times, it is not meaningful to assess theories as true or untrue.

Things are different with certainty, as we can be more or less certain about something. In my view we can never be completely certain whether we have reached the simplest, the most empirically adequate, etc., explanation. But we can always provide a good argument that one theory is simpler, more empirically adequate, etc., than a competitor. Theoretical virtues should therefore be interpreted as certainty indicators, instead of truth indicators. Satisfaction of virtues leads to a decrease in feelings of uncertainty.[426]

On this approach it cannot be the case that only epistemic virtues define what is rational.[427] The promotion of all virtues can in principle be rational.[428] As epistemic virtue is connected to the notion of truth, I will henceforth refer to the whole array of virtues as theoretical virtues. It is very well possible to define conceptions of scientific progress in term of virtues without invoking any notion of truth. Boon (1983), for example, provides a definition of scientific progress as to coordinate and predict an increasing number of facts in an increasingly precise way. The virtues predictive success, empirical adequacy, coherence and/or consistency and precision/accuracy are present in this definition, but not truth. It is not difficult to extend Boon's definition, adding other virtues. We could, for example, also define progress as 'coordinating and predicting an increasing number of facts in an increasingly more precise and simple way solving an increasing number of problems'. By this logic, evaluation becomes a matter of

[426] Note that this builds on what has been said in chapter 4, section 4.2. The shift to the uncertainty perspective fits with Bevir (1999) who calls for epistemology to take an anthropological turn. For Bevir this means we must focus on intellectual virtues (he distinguishes between cognitive values and theoretical virtues) and define scientific objectivity as a practice based on these virtues.

[427] Schindler (working paper).

[428] My articulation of the virtue approach to rationality leads to an agnostic position with respect to the realism-instrumentalism debate. As both camps have to rely on a comparative assessment of virtues when studying theory choice, a standpoint on the issue whether the succession of scientific theories converge upon a unique structure of the world or not, does not affect the approach towards the study of theory choice in past science.

comparatively weighting scientific theories on the whole array of virtues.

Still, in this thesis we continue to focus on the more traditional theoretical properties. In symmetrical approaches to past science we find assessment in terms of virtues as well, but these are of a different kind. In social constructivist approaches to science one can assess whether theories have the virtue of being functional, or of meeting the social interests that play a role in specific historical situations. With the practical turn in science studies and the development of the posthumanist approaches, practically oriented virtues such as robustness, reliability, reproduction and stability came to be the main evaluative categories.

While I do not deny that it is important to take these virtues into account, they focus on technological control and the role of social structures sustaining claims to knowledge, and not on the theoretical claims themselves. The shift towards a different set of virtues is a consequence of the development of an altogether different view of what knowledge actually is. From this it followed that the acceptance and rejection of theories came to be explained in purely quantitative terms.[429] Moreover, as we saw earlier, symmetrical approaches to past science run into difficulties when it comes to intercontextual comparison.

The focus on the more traditional virtues has the aim of making qualitative assessments possible again in historiography of science. What symmetrists have found unacceptable is that rationality acquires a 'sacred' character in evaluative approaches to past science. In the next section I hope to show that this is not the case for the approach defended here.[430]

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[429] With regard to networks, a relatively new field of study called social network analysis has provided a number of concepts with which networks can be analysed in quantitative terms. See Wasserman and Faust (1994).

[430] For the sake of completeness I mention that social values and even aesthetic preferences (Feyerabend) have been suggested as a *replacement* of rational factors as determinants of theory choice. In a number of publications from the 1990s it is however argued that social values (Longino 1990, Haraway 1991) and aesthetic criteria (McAllister 1996a) can function as important determiners of theory choice, not in opposition, but *next to* rational factors. I expect that such approaches are compatible with the virtue approach to rationality defended in this chapter but it requires further investigation to establish whether this is indeed so, which lies beyond the scope of the present work.

3.The virtue approach applied to the study of past science

The virtue approach will be applied to the study of past science in four steps. The first step is to create a set of theoretical virtues on the ‘type’ level. On this level virtues are loosely defined, staying close to the meaning we intuitively apply to them. Secondly, we must allow for differing interpretations of virtues and different preference orderings of them on the ‘occurrence’ level, that is, in actual historical contexts. The distinction between type and occurrence resembles C. S. Peirce’s famous distinction between types and tokens, where the type is the general thing and the token its concrete, physical, realization. A token then is a more restricted notion than an occurrence of a type. Therefore ‘occurrence’ suits the present purposes better.[431]

The third step is to relate specific virtue preferences to typical problems of theory choice. The most important question to answer is how we should treat cases in which different preferences for virtues have actually pulled theory choice in different directions. Finally, this brings us to a consideration of the normative force of the virtue approach, which has to contain an extension to the descriptive base of historical case studies. The approach I defend puts minimal demands on the rationality of decision-making, thus allowing for a significant degree of variation. The burden of proof, with such a liberal definition of the concept of rationality, is to demonstrate that this framework can still fulfil the desired evaluative purposes.

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[431] In Paul (2012) the distinction between type and occurrence is wittily indicated with the terms ‘thin’ and ‘thick’. ‘Thick’ is derived from Geertz extensive method of contextualization, which is called ‘thick description’. A ‘thin’ articulation of the virtues in question then corresponds to our type level. The type-occurrence distinction also lies at the heart of SSK. There the same type of factors, namely social factors, always determines outcomes in science. But on occurrence level many things fall under this heading and it depends on the particulars of the historical situation which social factors were relevant. In Hoyningen-Huene (2013) science is distinguished from non-science through scores on nine levels of systematicity. His approach is similar to the one defended here in that he also makes a distinction between an intuitive understanding of the nine dimensions of systematicity on the type level, and an occurrence in which exact interpretations of systematicity and ways in which they were effective come about.

In the previous section it was defended that *in principle* all theoretical virtues could be rationally preferred. It depends on the specific circumstances of theory choice which virtues have played and/or should have played the decisive role. But before we can turn to the study of these differences we need to create a set of virtues on the type level. The virtues mentioned above included empirical adequacy, accuracy, precision, consistency, simplicity, fruitfulness/heuristic value, scope, falsifiability, verifiability, predictive success, problem-solving capacity, coherence and explanatory force (including integration, unification and specification of causal mechanisms). These are 13 virtues, or 16 if the last one is subdivided. I do not claim that this list is fully exhaustive but I believe it covers most of the ground and certainly contains the most important theoretical virtues.

The members of this set can be seen as constant elements of the history of science. Hence this list of elements can be made part of our platform. The idea now is that the number of members of the set is large, but not intractable. This offers a remedy against the problem of the so-called 'etc.' lists, which weaker variants of normative naturalism face. When the established optimal relationships between means and ends cannot be generalized from one context to another, all we get is an ever-expanding list of means-ends relations. This has rightly been criticized for not producing the desired clarifying analysis of past science.[432] In other words, the naturalist side of such approaches has become too strong.

The type-occurrence distinction is proposed here as a solution to this problem. The demand is that all theory choice, in order to count as rational, has to be performed with reference to one or more of the virtues of our list at the type level. This list thus provides boundaries within which scientific development has to take place in order to count as rational. But these boundaries are loose, for three reasons. First, on the type level the virtues are not moulded into strict definitions. For example simplicity requires no more than the intuitive 'easy to understand' or 'composed of few parts'. Empirical adequacy can be defined as 'capturing observable phenomena' or even 'saving the phenomena'. This allows for variation to occur in interpretation of the virtues on the occurrence level. Second, it is not required that all virtues be taken into account in all instances of theory choice. In some

[432] See chapter 4, section 2 and chapter 6.

cases choice may hinge on only one or two virtues, while in others a more complex relative weighting procedure has to force a choice. Third, we do not set up an *a priori* hierarchy among the virtues on the type level. In the previous chapter we saw that doing so quickly leads to problems. Laudan's overall preference for problem-solving capacity, for example, has to reject instances of theory choice in which other virtues outweighed problem-solving capacity. Sticking to the stabilist theory of the Earth's land masses provided an example of such a choice, and this had to be judged as a rational choice.

Only when empirical research strongly supports the inference that there are typical situations of theory choice in which typical preference orderings among virtues must hold, are we allowed to use this normatively in interpreting past science. Of course such information would strengthen the evaluative approach of the kind suggested here. But to my knowledge research in history and philosophy of science simply has not produced enough conclusive evidence to justify the adoption of such typical patterns of inference yet.

When these three points are taken on board, the virtue approach is significantly softened. This avoids the charge that rationality is turned into something sacred again. It allows for a thoroughly pluralistic approach to the evaluation of the rational character of choices made in the past. Still, the boundaries set to theory choice are recognizable and identifiable. Thus the pluralism that is advocated here is not unrestricted but is a pluralism within parameters.[433]

Next we need to arrive at an interpretation of theoretical virtues in historical contexts, that is, on the occurrence level. Let me illustrate

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[433] Please note that scientific pluralism has been defended in recent years for a number of reasons (see for example Chang 2004, Chang 2009 and Chang 2012 or Nowotny, Scott and Gibbons 2001). These include the idea that a variety of strategies is needed because different stages of a research programme require different strategies of research. Further, maintaining different lines of research is required in order to ensure breakthroughs in science. It has often happened in the history of science that the influx of ideas and methods from other fields of study turns out to be fruitful. Finally, it has been argued that, in the absence of one evaluative standard, we can arrive at robust claims to knowledge only through a discussion between opposing points of view. Critique in this sense makes us stronger instead of weaker. While I feel sympathy towards these arguments, they do not directly contribute to articulating an acceptable form of assessments of past science and are therefore left out of the discussion.

the distinction through a non-science example, namely the practice of gift giving. Gift giving is a custom that occurs in almost every society. Yet what counts as appropriate behaviour can markedly differ from culture to culture. Quite different customs can, for example, be attached to the reception of gifts. In some countries it is embarrassing to unwrap a present in front of the giver. It is thought that the first reaction to the present would reveal unwelcome signs of appreciation or disappointment and that attention would focus too much on the giver instead of the receiver of the present. The regular custom is to unwrap presents later in private and thank the giver according to habitual procedures. In other countries however, not unwrapping a present in front of the giver is embarrassing, because this is perceived as indifference from the receiver towards the giver.

It is not easy to determine what the best practice with respect to receiving presents is. The one that should be preferred should lead to the least amount of problems, confusions and/or embarrassing situations. But this also depends on further concrete circumstances such as the occasion (wedding, birthday, etc.) and the relation between giver and receiver. Also, gifts may serve a variety of purposes and may involve complex codes of reciprocity. Yet, in spite of all these differences, I believe gift-giving practices can be compared to each other because on the *type* level they are invariably seen as socially welcome. 'Thick' description at the occurrence level takes place within the boundaries drawn by a 'thin' specification of gift giving at the type level.

In the same manner we can also see varieties in application of the theoretical virtues. Simplicity for example can be preferred for a number of reasons. A simple explanation is often seen as more profound than a more complex explanation because the simple explanation requires fewer assumptions. When, for example, a reduction is possible from one level of explanation to another level, this is mostly perceived as a clear sign of progress because a superfluous level of explanation is eliminated. Another reason to prefer simple explanations to complex ones is that scientific laws often hold only in idealized circumstances. Examples are the law of free fall, or the ideal gas law $PV = nRT$. These laws abstract away from reality and hence involve simplification. In this case the virtue of simplicity supports virtues such as explanatory force and predictive accuracy but turns against others such as empirical adequacy.[434]

Simplicity can also be preferred in situations of considerable uncertainty about the phenomena one is researching. As Darden (1991) has indicated, it can be a good strategy to eliminate problematic components of a theoretical model and continue to work on a simplified theory when scientific research faces difficulties.[435] According to Francis Crick it is often unclear which part of a set of collected data counts as relevant. Therefore he argued that it is often better to work with a good concept or theoretical assumption first, and only later complicate matters in terms of empirical adequacy.[436]

Finally, simple theories can be preferred also because they are easier to verify or falsify than complex theories. This has the obvious advantage of creating more focused research and less disagreement about the tenability of a theory in light of problematic evidence. We can however not demand that all theories exhibit a fixed degree of simplicity in service of testability. Sometimes the complexity of a theory is simple irreducible.

The point of all these examples is to show that preference for a particular virtue is determined by the demands of the situation in which scientific research finds itself. Favourable combinations between virtues can also differ accordingly.[437] Differences in preferences for particular virtues can also hold in general, and be connected to distinct

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[434] The historical record shows interesting cases of scientists struggling with these conflicting virtues. Joseph Priestley (1733-1804), for example, was convinced that progress in science could only be achieved through generalization over facts, and hence through simplification. Yet at the same time he also thought that generalization was always bound to fail because there would always be facts left unexplained *because of* the generalization. Brock (2008) demonstrates how Priestley struggled to combine these incompatible notions of necessity and deficiency. For more examples of conflicting virtues see the discussion of step three below.

[435] In chapter 4 I discussed Wimsatt (1987) on the possible fruitfulness of wrong models. According to Wimsatt scientists sometimes deliberately work with simplified models, of which they even know that these must contain errors, because the simplified model can help them overcome particular problems of research.

[436] Crick once said that "evidence can be unreliable, therefore you should use as little of it as you can." Quoted in Boon (1983) p.204.

[437] The same point can be made for others virtues than simplicity. For a study on the multiple ways in which, for example, coherence can manifest itself see Bovens and Hartmann (2003).

historical eras. According to Daston and Galison, in their groundbreaking study on the notion of objectivity, virtues are more than tiebreakers in theory choice. They can also function in a much broader sense as general regulative principles in science. General aims in science can thus be expressed through the pursuit of virtues.[438]

Daston and Galison perceive profound changes in perception of the notion of objectivity in the last three centuries. They connect these changes to shifts in virtue preferences. Their first notion of objectivity is called 'truth to nature'. Objectivity in this sense allows an important role for the investigator to generalize over individual observations. From a set of specimens of, for example, a plant species, an insightful natural philosopher is allowed to infer an ideal type (cf. Goethe's *Urtyp*). Such generalizations can be connected to the pursuit of the virtue of simplicity. With the rise of technical means in the 19th century, most importantly through photography, it became possible to mechanically produce images of nature. This, according to Daston and Galison, led to a shift in thinking about objectivity. 'Mechanical objectivity' came to replace 'truth to nature' as the dominant model of objectivity. The main goal of scientific endeavour became the mechanical reproduction of the 'facts'. This is more closely aligned to the pursuit of empirical adequacy. 'Structural objectivity' represents another model of objectivity, which gained ground in the course of the 20th century. The role of individual scientists has increased again, namely as experts who are highly skilled in pattern recognition. This sense of objectivity can be connected to the promotion of the virtue of explanatory force.[439]

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438 Daston and Galison (2007) pp.39-53 give an extensive discussion of what they call epistemic virtues, which should not be confused with the more formal use of the term in the distinction epistemic vs. theoretical virtues. One of the strengths of their study is that analysis is connected to the study of scientific practices in which the regulative virtues make themselves manifest.

439 For Daston and Galison preferences for virtues also count as moral preferences because they express what scientists should aspire for, and hence they define what a scientific self is. This 'lead' was taken up in order to study the community of historians of science in the 19th and beginning of the 20th century in Paul (2011) and Tollebeek (2011). In the recent *Journal of the Philosophy of History* 6 (2012) a lot of attention is paid to personal virtues as well, especially to the issue how to conceptualize the relation between personal and impersonal virtues. I take the decrease of uncertainty as the primary aim of science. As uncertainty is an aspect of persons it is natural to assume that the pursuit of personal (or moral) virtues has a strong relation to the pursuit of

Daston and Galison make the interesting suggestion that we rely on (sets of) preferred virtues to avoid errors. This means that when shifts in dominant models of objectivity occur; the general epistemological 'fear' of doing something wrong, associated with the pursuit of a dominant virtue, also has to change.[440] This angle supports the idea that a focus on the pursuit of virtues can serve evaluative historiography. Daston and Galison approach notions of objectivity and error in ideal typical fashion and have grand generalization schemes to offer. It is quite well possible that these do not hold up in their entirety, when confronted with more detailed historical research. Still their attempt to gain insight in the preference of virtues on a general level deservers praise, even if empirical historical study forces adjustments into more fine-grained models. Such interaction between generalization and empirical research is the only way in which we can arrive at more stable patterns of typical virtue preferences. Others historians, such as Paul and Tollebeek, have recently shown interest in exploring this direction of research, which I believe is important, and hence should be pursued by more historians of science in the future.

Lets now turn to the third step in the application of virtues to the study of past science. The difficulty in studying the past through the prism of the pursuit of virtues is that preferences for virtues can come into conflict with one another. Daston and Galison advise us in such situations to study "how much hangs in the balance if one is obliged to choose among them."[441] But this is easier said than done, not just for practicing scientists but also for historians aiming to assess past theory choice.

In chapter 4, we have already confronted an example in the history of geology, in which theoretical virtues pulled in opposite directions. Preference for the virtue of problem solving pulled into the direction of the mobilist theory of the Earth, as this theory could solve many more problems than the stabilist theory. But the stabilist theory was long preferred (until the end of the 1960s) over the mobilist theory

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theoretical virtues. However, the personal level also involves a somewhat distinct group of virtues such as patience, moderation, humility, etc. It is interesting to investigate the relations between the two kinds of virtues through historical research but this falls beyond the scope of the present work.

[440] Daston and Galison (2007) p.372. See also Daston (2005), discussed earlier in chapter 4.

[441] Daston and Galison (2007) p.376.

because the mobilist theory could not be supported with a mechanism to explain how the gigantic land masses could be moved. The virtue of explanatory power (not being able to provide a causal mechanism) thus long outweighed the virtue of problem-solving capacity.

Other examples of theory choice, pulling in different directions, are given by the opposition between predictive accuracy and explanatory power. Predictive accuracy has also often been related to 'saving the phenomena'; an example is the ability to predict the position of the heavenly bodies. This can be done by way of calculation, without attempting to find causal mechanisms for the movements of the objects, which has been common practice in mathematical astronomy for a long period of time.[442] With Darwin's theory of evolution we find exactly opposite preferences. Darwin's theory scores very highly on explanatory power because it provides an explanation for extinction and survival of all species. This, however, is achieved almost totally at the expense of predictive accuracy, since the theory cannot predict what the next steps in the evolution of species will be. From these examples Losee concludes that we have to acknowledge that both the virtues of predictive accuracy and explanatory power are important aims in science, but that it depends on particular evaluative situations in favour of which virtue the balance tips.[443] In my view we stand only at the beginning of a gaining a deeper understanding of how and why this happens. Daston and Galison have suggested that epistemic virtues implicitly modify one another by the very possibility of choice among them. Such modifications are one example of the processes underlying theory choice that need to be much better understood.

Preference for theoretical explanation of the same natural phenomenon can also differ due to the pursuit of conflicting aims. Losee gives the example of the behaviour of gas near its critical point. If predictive accuracy is ranked high, the theory of virial expansion, to calculate pressure values at high temperatures, must be preferred. But when explanatory power is found more important, Van der Waals theory indicating how the pressure-volume-temperature equation (PV = nRT) can be derived from kinetic theory must be preferred. In this

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442 A more recent example of this opposition stems from quantum mechanics. According to Kaiser (2011) the dominant attitude in the realm of quantum physics has long been to avoid difficult fundamental questions and just 'shut up and calculate'.

443 Losee (2004) p.108.

example, the different aims do not lead to conflicting theories as the Van der Waals equation can be put in virial form. But it can be imagined that the pursuit of different aims does lead to conflicting theories with respect to the same natural phenomena.

A final example of conflicting aims is been between simplicity and empirical adequacy. It is easy to see how these two can come into conflict. Empirical adequacy urges us to take all data into account. However, connecting all data points by means of one mathematical function mostly leads to an arcane curve. Scientists generally look for what they perceive as the best fit to all the data points. Hence they prefer simplification to exactness.[444] This however leads to the well-known curve-fitting problem: if we start abstracting away from the data what are the grounds to prefer one simplifying curve to another? How to justify such a choice if we can no longer ground this choice on the observed facts? Cartwright has pointed out that when predictive accuracy is at stake even bigger deviations are abstracted away. Scientists sometimes even prefer to work with theoretical abstractions, which they *know* are false. An example is treating molecules as if they were elastic point masses, which they are not. This is thought to be excusable because it brings a gain in predictive accuracy. Cartwright draws from such examples the, for us by now unsurprising, conclusion that successful prediction may be more important in science than convergence upon truth.[445]

The examples above show that virtues can pull theory choice in different directions. How are we to judge theory choice when this happens, given that we do not work with an *a priori* hierarchy of virtue preferences? I think that as long as we do not possess clear guidelines

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[444] Note that this problem plays a role on the level of historiography of science too. Symmetrical approaches to past science can be seen as promoting the virtue of empirical adequacy, but when we get very close to the actual past, analysis of the past becomes obscure. A platform involves abstracting away from historical particulars, with hopefully a gain in analytical clarity as a result.
[445] See Losee (2004) p.105. Note that the virtue of simplicity, when opposed to empirical adequacy, has the same effect. It would be a mistake however to think that simplicity is always opposed to accuracy. Forster and Sober (1994) for example investigate under what conditions simpler theories provide *more accurate* predictions. Similarly in the humanities, the notion of precision can be attached to accurate use of language. The ability to express oneself in precise terms also involves notions such as clarity and can be associated with simplicity as well.

which virtues should be preferred in typical situations, we simply have to follow the choices historical actors have made. If we want to avoid fitting history in a straitjacket we must allow for such freedom of choice.

In a debate with Laudan, Bloor (1981) draws a relativist conclusion from such considerations. For Bloor, changing interpretation of virtues such as simplicity (over time what is seen as simple or complex may change), shows that simplicity in itself is not primary but derivative of others factors, which, as we know, in Bloor's view are social factors. This also holds, according to Bloor, for changes in the preferential order of sets of virtues. While we must agree with Bloor that variation in interpretation and application of the virtue of simplicity occurs, it cannot be accepted that this variation is infinite and that it pre-empts all forms of comparability. Specifications of virtues on the type level should be strong enough to avoid this. I am also inclined to agree with Daston and Galison who write that: "far from relativizing these virtues, history exhibits their rationale, if not their transcendental rationality."[446] The occurrence of variation calls for a more thorough analysis of the reasons for preferences of virtues. If this leads to the inference of typical patterns of preferences, then we can establish a stronger comparative basis for evaluations of choices made in the past. Bloor, however, does not explore this possibility.

A number of ideas have been put forward to deepen the analysis of virtue preferences depending on particular scenarios of research. Kuhn, for example, proposed a distinction between the demands set by empirical practice and the demands set by theoretical hypothesizing. He argued that while consistency and simplicity are important on the theoretical level, this is often not directly the case on the experimental level. When scientists probe new directions of research, accuracy and fruitfulness are, according to Kuhn, "the most immediately applicable, perhaps followed by scope. Consistency and simplicity are far more problematic."[447] The idea of the distinction between theoretical and practical dimensions of past science can be connected to the philosophy of experiment, which was discussed in chapter 4. If we recognize a diversity of layers in experimental research (such as background assumptions, experimental set-up, retrieval and

[446] Daston and Galison (2007) p.376.
[447] Kuhn (1977) p.330

interpretation of results, and theoretical inferences), we can possibly attach preferences for virtues to these layers. The layers can be interpreted as representing the stages of research. When such a temporal element is included we can possibly connect preferences for virtues attached to the respective layers to stages of research programmes executed in past science. See section 4 for further exploration in this direction.

Another way to approach the issue is through insights from cognitive psychology.[448] This is an angle that has not often been used in historiography of science and can possibly do with more attention.[449] Interestingly, a discussion has taken place among cognitive psychologists on the interpretation of rational behaviour, which very much resembles the main discussion of this thesis. Psychologists have long conceived of rationality in terms of optimal reasoning. This met with the so-called ‘heuristics and biases’ approach, favoured by Tversky and Kahneman.[450] Tversky and Kahneman argued that humans do not make decisions according to the guidelines of optimal reasoning because they are generally not in possession of all the required facts, or because their judgments are biased through prejudices, presuppositions, etc. From investigating optimal decision-making, psychologists went to biased decision-making, with nothing in between.

Gigerenzer (2008) has however argued that in the ‘heuristics and biases’ approach the idea of optimal reasoning is *not* questioned but kept alive, because rational behaviour is explained in terms of *deviation* from the optimal norm. I see a clear parallel here with positivist assumptions about science living on in post-positivist approaches, as discussed in chapter 5. According to Gigerenzer the normativity of the

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[448] According to Henderson (2012), when certain epistemic values are taken to be central to a human pursuit we are not far off from considering the cognitive processes by which this pursuit is carried out. He is especially interested in the continuing refinement of the cognitive processes enhancing the reliability of the scientific endeavour.

[449] A notable exception is the cognitive approach defended in Giere (1988). In Kuukkanen (2008) interesting support from cognitive science is presented for Kuhn’s idea of concept learning through similarity relations. Eigner (2010) investigates the role of models in scientific understanding via a cognitive virtue approach.

[450] See Tversky and Kahneman (1974); a recent compendium is Gilovich, Griffin and Kahneman (2002).

earlier approach should *itself* be questioned and not be circumvented: "inappropriate norms tend to suggest wrong questions, and the answers to these generate more confusion than insight into the nature of human judgement."[451] While the approach of Tversky and Kahneman takes human limitations into account, Gigerenzer argues that it does not focus on how decision-making takes place in interaction with the context and according to the demands of the problem at hand.

For Gigerenzer it is these things that determine what optimal reasoning *in that context* is. Norms of reasoning are thus never content-blind but must always be situated. This approach to rationality can be called bounded rationality (a term coined by Herbert Simon in the 1950s) because the assessment of rationality is bounded to particular ends in specific contexts of pursuit. Gigerenzer himself prefers the term 'ecological' to characterize his approach.[452] In his approach strategies of reasoning and decision-making have to be valued as means in relation to ends. The fruitfulness of selected rational procedures can be judged in competition with other methods, and with hindsight, on their longer-term effectiveness.

Gigerenzer also interprets rationality in terms of heuristics, but in a positive way. In the absence of absolute standards of reasoning, humans have to take decisions matching means to ends. For example, when a ball is thrown, and someone is trying to catch it, exact calculation of the trajectory of the ball is possible. When the initial conditions, such as the force with which the ball was thrown, and circumstantial influences on its course, such as wind, are known, the exact place where the ball will land can be calculated. But this is highly impractical: it is not well possible to collect all the required data and the calculation takes far too long. Moreover, unexpected external effects on the ball while in the air cannot be ruled out. It is far more effective then to use a rule of thumb, adjusting the position of the body according to the movement of the ball. Gigerenzer argues that for the catching task this way of going about things is not suboptimal but actually the *best* available procedure we have. An attractive aspect of the positive view of heuristics is that it leads to a dynamic theory of

[451] Gigerenzer (2008) p.12.
[452] Gigerenzer (2005), Gigerenzer (2008).

rational decision-making as the means to meet ends can be improved upon, whenever possible.

While exact calculation and formal ways of reasoning are not of much use for daily practical tasks, they can have a place in Gigerenzer's model, but he expects them only in particular areas of science such as theoretical physics. However, scientists too select rational strategies to meet ends. This is the cornerstone of normative naturalism and we have adopted key ideas from normative naturalism in our articulation of the virtue approach. Cognitive psychology *à la* Gigerenzer can be helpful in identifying how humans manage to meet means to cognitive ends. [453] In any case decision-making in science based on heuristics has gained interest among philosophers of science of late. [454]

Gigerenzer's approach presents itself as useful, especially in light of the extended naturalism we seek to articulate. But perhaps his approach is still too closely related to the forms of extended naturalism discussed in the previous chapter. Therefore I want to briefly mention another interesting approach to rational decision-making stemming from cognitive psychology that goes a step further in terms of generalizations of rational strategies. This is Stenning and Van Lambalgen's multiple logics model.[455] We have seen that for Gigerenzer logical reasoning is relevant only in highly specialized environments to execute specific tasks. According to Stenning and Van Lambalgen, however, logic is used much more frequently in human cognitive functioning. They do not perceive logic as synonymous to one perfect way of reasoning but instead prefer to interpret logical reasoning through a collection of multiple logical models. These multiple logics form a set of typical reasoning strategies, which are like

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[453] This is also what interests Henderson (2012) in Gigerenzer's ecological approach to rationality.

[454] Thus Schickore (2003) p.265 asserts that "philosophers of science have begun to acknowledge that scientific rationality has to do with giving and asking reasons, making value-laden decisions and so on." Clear demonstrations of this are Nickles (2006), Nickles (2009), Seselja and Strasser (2013) and Seselja, Kosolosky and Strasser (2012). These works question the boundaries between discovery and justification on the grounds that, from a heuristic perspective, appraisal plays a role in both contexts.

[455] See Stenning and Van Lambalgen (2008). The authors take direct issue with Gigerenzer. I thank one of my students, Aafke de Vos, for bringing Stenning and Van Lambalgen's work to my attention. See her conference paper on logic and human reasoning presented at CLPS13 in Ghent.

cognitive resources that can be selected depending on the task at hand. As in the approach of Gigerenzer, reasoning strategies are understood as means to solve particular ends but Stenning and Van Lambalgen's approach adds an extra level of generalization. This makes the set of strategies less closely tied to these particulars.[456]

In the multiple logics model, norms are given by the respective logics. One can thus err in violating these norms while performing a reasoning task for which the selected logic is in itself apt. But another way to go wrong in the multiple logics model is to select the wrong kind of logic, that is, to violate knowledge about optimal means-ends relationships. With Gigerenzer's model errors can also come about through mistaken application of cognitive mechanisms but *not* through the violation of a norm, as in most cases the selected heuristic is not 'logical'.

The crucial take-away point for the present investigation is, what we can learn from cognitive science about means-ends relationships when the ends are presented by the theoretical virtues. The multiple logics model is more strongly normative and appears to fit our type-occurrence distinction better than Gigerenzer's ecological approach to rationality. It would be interesting to delve deeper into these issues and explore the decision-making of past scientists through both theories of these cognitive psychologists. This is however beyond the scope of this thesis.

I conclude this section with a summary of the suggested approach. We work with a set of virtues, with weak definitions of each individual virtue on the type level. This set of virtues must be taken from past scientific practice and it must be possible to update the set, should the need for this appear. As long as there is no evidence to the contrary we do not assume a hierarchy between the virtues in the set. All this leaves a lot of room for divergence on the occurrence level both in the

[456] Gigerenzer takes our cognitive mechanisms to be simply a product of human evolution. Stenning and Van Lambalgen do not share this opinion and argue that, with the emergence of language, humans became capable of formulating multiple logics for planning and reasoning which they started to use in all aspects of life. Ways of logical reasoning thus can emerge independently of a particular type of problem solving and only later turn out to be apt for that particular task as well. This is not evident with Gigerenzer's approach.

concrete interpretation of the individual virtues and in terms of preferential order among them.

This does not lead to a full naturalism, and hence to relativism, for two reasons. First, although variation in interpretation of the same virtue occurs, this does not lead to complete incommensurability, because the boundaries set on the type level ensure comparability. The thin parameters set at the type level provide the boundaries with reference to which theory choice has to be made in order to count as rational.

Second, how the promotion of particular virtues relates to particular circumstances, such as the stage of a research programme or the nature of the research problem, is by and large an empirical matter. Any normative guideline indicating the pursuit of which virtues has to be preferred given typical circumstances need to be built up from this empirical information.[457] A number of ideas in order to gain grip on typical circumstances underlying decisions of theory choice have been discussed, i.e. Kuhn, Darden, Daston and Galison, and insights from cognitive psychology. Interpreting theory choice in the diachronic context of developing research programmes will provide further clues: this is the topic of section 4.

In any case, when a number of virtues are involved, evaluating whether a theory is better than a competitor can be equivalent to a highly complex weighting procedure. This procedure also has to be related to other determining factors in the history of science, cf. the tentative 'new relationalist' model presented in chapter 2. Science is a very complex activity and accounting for it properly may have to reflect this. Complexity is not an argument against evaluation. On the contrary, I believe that the virtue approach represents the only approach to past science that allows for history to play a highly significant role while at the same time retaining a thrust of normativity.

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[457] Paul (2012) p.375 advocates a similarly broad virtue approach to the profession of history itself. According to him historiography can be evaluated through "hierarchies of intellectual virtues depend on historiographical situations, that is on the interaction between (1) the genre of writing, (2) the historian's research question, and (3) the state of literature."

4.The history of science as a collection of research programmes

In this section I will expand on the discussion in section 3 and consider an approach to account for past science over longer periods of time. This approach involves a perception of past science as a collection of research programmes. In my view research programmes are kept together by central problems of research, general aims, ultimate goals and/or guiding metaphors. These function as 'glue' and attract whatever appears relevant: methods, models, ideas, instruments, experiments, etc. Sometimes this leads to disciplinary organization; sometimes new programmes are formed within a discipline; sometimes the research programme remains outside the structures of academic disciplines.[458]

At the onset of a new research programme there are a lot of uncertainties, both about the phenomena one is trying to understand and about the appropriate ways to do so. Mostly there is a more or less clear problem and a few encouraging results, and the manner in which these were achieved provides a positive heuristic to work with. Gradually, in the course of time, which may significantly vary from programme to programme, uncertainty decreases.

A research programme ends when the aims or goals are met and/or the problems are satisfactorily solved. It is then time to move on to something else. But it can also be the case that a research programme comes to an end because acquired certainties are cast into to doubt, which leads to a rethinking of the existing programme.[459] In both cases,

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[458] I have developed my own view on specialization into disciplines in Karstens (2012). There I consider the formation of disciplines as a process of hybridization. Constituting elements stemming from all kinds of directions (humanities, sciences, general culture) fuse together to create new disciplinary structures. This 'elements and relations' approach proved to be very useful for my case study, which focused on the new way to study language that came about under the aegis of Franz Bopp (1791-1867). The focus on disciplines has the advantage that it allows one to consider the emergence of cognitive and social structures as interrelated wholes. A weak form of demarcation is drawn between a discipline and its context but this does not require a sharp distinction between rational (internal) factors and social (external) factors. Although a research programme is a broader notion than a discipline, I think research programmes should be studied in roughly the same way.

[459] Scientific problems can be quite persistent. Think about paradoxes of motion and time or about deep questions about the nature of gravity or the

one steps from a state of relative certainty to a new state of relative uncertainty and a new process of gradual decrease of uncertainty will commence.[460]

This view of past science as a collection of successive research programmes helps our project of articulating an acceptable evaluative historiography of science in three ways. First, the pursuit of virtues can be tied to phases of research. Typical phases of research programmes may require typical preferences for virtues. If this is the case it becomes possible to assess whether research strategies that have been selected reflect these virtue preferences and hence whether these selections have been appropriate.[461] A number of scholars point in this direction and their work will be discussed below in section 4.1.

A second advantage of the suggested approach is that it allows for assessments of scientific theories in the longer run. One can compare the value of one theory with subsequent ones, or one can study the continuity of different versions of a theory, or different theories altogether if these continue to exist next to each other, and compare them in terms of predictive success, explanatory force, etc. [462]

The third advantage of the suggested approach to diachronic historiography is that we can use the benefit of hindsight to treat the various phases of past research programmes as connected wholes. It is here that the shift in focus from (in)determination of theories to (un)certainty in persons proves useful. With the primary analytical focus on the gradual decrease of uncertainty we are not forced to interpret every point of choice in past science as a so-called ‘ strong’ decision. If we do not demand full closure of every scientific controversy at every junction of science, the sting can be taken out of the problem of underdetermination because we are not forced to do draw the same conclusions from it as in SSK (see chapter 2). This argument is developed in section 4.2 below.

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nature of consciousness. For more examples of persistent problems see Hanlon (2007).

460 As said above, complete certainty in my view is hard to attain. Richard P. Feynman once put it like this: “What we call scientific knowledge today is a body of statements of varying degrees of certainty. Some of them are most unsure; some of them are nearly sure; but none is absolutely certain.”

461 Virtues, which are ultimately expressed by our theories of the world, are promoted via methods and selection of aims. In this sense one can also speak of the pursuit of virtues in relation to methods and aims.

462 For the same point see Schickore (2003) p.268.

My plea for a focus on past science in terms of a collection of research programmes may make the reader think of Lakatos' methodology of scientific research programmes. While there certainly are a number of similarities with his ideas, there are important differences as well. In Lakatos' methodology, a research programme is interpreted as a string of theories. A research programme can be progressive in two ways: theoretically and empirically. This is expressed through the pursuit of two virtues. Theoretical progress is achieved by better predictions, empirical progress is given by the confirmation of novel predictions and hence by an increase in empirical adequacy. It is irrational not to choose successor theories within the programme that promise theoretical and/or empirical progress. Lakatos' methodology allows for temporary regressions or stagnations of the research programme, but these cannot last long. A programme will start to degenerate when the heuristic of the programme becomes exhausted. If this happens, a new programme has to be initiated to keep science going.[463]

The notion of heuristic 'force' is important. Lakatos argued that theory choice hinges on an appraisal of heuristic force on three levels: (1) prediction of new facts (theoretical progress), (2) empirical confirmation of theoretical predictions (empirical progress) and (3) methodical and systematic development of research. When a new theory, or new approach, in a particular domain promises all of these three, this theory or approach is likely to be accepted by the scientific community. According to Lakatos it can, from the present perspective, be objectively established in each historical situation whether these three conditions have been fulfilled. When choices 'against' them have been made these can also be criticized from a presentist perspective.

Thus Lakatos divided past science into a collection of research programmes and he evaluated progressiveness of these programmes in terms of virtues. However, the similarities between his and our approach end there. Where Lakatos focuses on two virtues only, in our approach a whole set of virtues must be taken into account in evaluating past science. Moreover Lakatos appears to work with an absolutist understanding of the virtues in question, while I think they should always be understood comparatively. With respect to historiography of science Lakatos' methodology stands in service of

[463] See Lakatos (1976) and Howson (1976).

projects of rational reconstruction. The development of research programmes should follow the three conditions mentioned above in order to count as rational. For Lakatos this has to be captured in an internal historiography. When the course of science deviated from this internal path (i.e. was irrational) this should be accounted for through a supplemental external historiography.

As we have already seen in earlier chapters, this sharp distinction between realms internal and external to science leads to problems. One of the problematic effects of the approach is that the reconstruction becomes more important than the actual course of history. Lakatos even advocated that the rational reconstruction should replace the actual course of history as soon as irrational decisions are confronted. To relegate the real history to the footnotes in this way is not tenable. Further, in Lakatos' methodology of scientific research programmes there is an ambiguity about the place of errors. Sometimes he opposed rationality and error outright, in other places he suggested that the problem of deviation from the correct path could be solved internally.[464] These difficulties come about because Lakatos' framework does not leave enough room for variation in past decision-making and also because of a dominant preoccupation with the determination of theories.[465]

4.1 The pursuit of virtues and the various stages of research

In this section I draw upon the work of various authors who have connected research strategies and the pursuit of virtues to specific stages of research. As we have seen in chapter 6, Boon (1983) interprets scientific rationality as a set of strategies and selective procedures, which are efficacious only in relation to particular challenges. The type

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[464] See chapter 4.

[465] Note that Lakatos had difficulties to explain when and how a historical actor can tell whether the programme he or she is working in is starting to become degenerative, as such things can often be seen only with hindsight. The problem for Lakatos was that he could not accept this and had to deliver conditions of choice *in situ*. His model was attacked for its inability to do so. This, however, is not our problem. Hindsight is exactly what the historian possesses and therefore he or she can clearly distinguish one string of theories from other strings of theories. Hence it is possible to study the past accordingly.

of selection pressure depends on the problem situation and the phase of development of the research programme. Boon supports this analysis by augmenting the group-grid theory stemming from Mary Douglas. He specifies four distinct phases of research, a romantic, a pragmatic, a ritualistic and a dogmatic phase, which every scientific research programme will have to go through in that order.[466] Each phase poses specific problems for scientists and the effectiveness of rational means has to be assessed in relation to these problems. In general, different research strategies are required in each of the four phases.

The romantic phase is the starting phase of a new research programme. Individuals find the framework in which they work too restrictive and are attracted to promising alternatives. Typically such individuals do not have much to lose, such as young researchers, scholars with low status (outsiders) or with an unclear career pattern. Boon argues that new ideas are often confronted with counter-examples, paradoxes and uncertainty. Innovators however, are not discouraged by this, and even embrace such incongruences, as long as promising results are in store. The starting phase requires an open mind to all possible ideas. In short, the romantic phase has the following features: there is little perspective in the old system; migration from it is the only way forward. Individual action strategies are dominant. There is a tendency to grasp phenomena intuitively. New ideas are often incoherent, but in the new climate there is a high tolerance for error so that incoherence is not immediately devastating.

After the romantic phase a pragmatic phase starts. A number of alternative ways to move forward compete for dominance. In this phase not all freedom of movement disappears, but hardening of social and cognitive structures occurs. This also leads to a decrease of tolerance for error. When the pragmatic phase transforms into the ritualistic phase this process of hardening has been rounded off. Research now follows established norms and rules. Tolerance for error is confined to accepted margins. Scientists are occupied with a systematic elaboration of the main theory. A collective strategy is now dominant; hence cooperation, patience and conformism are called for.

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[466] He presents empirical evidence that science actually progresses according these four phases through an interpretation of the history of molecular biology along the lines of Douglas' model.

The ritualistic phase can become unbalanced in two ways: when the heuristic of the programme starts to become exhausted or through an external impulse. Although these make the cognitive grid significantly weaker, the group is still strong, and the research programme lingers on for a while in the so-called dogmatic phase. It is precisely because of the cognitive weakening that stronger demands are placed on keeping the group together. The dogmatic phase is hardly productive anymore. It is characterized by fundamentalism, stagnation, lack of tolerance for other opinions, and zero tolerance for error. This situation cannot last and leads to individuals breaking out of the existing framework. If this happens the cycle starts all over again with a new romantic phase.

The group-grid theory allows for an analysis of aptness of strategies depending on the phases of research. Strategies that work in one phase may be counter-productive in another phase. The romantic phase requires speculation; hence expected fruitfulness and scope will be the dominant virtues in the choice for initial theories and methods. It can be harmful to ask for too much precision and clarity at too early a stage of research. In other phases, however, precision, consistency and simplicity are what is called for. Maintaining a speculative attitude for too long hampers the further development of the programme.

In general the heuristics of a programme lose effectiveness when theoretical explanation loses contact with empirical research. A clear example of this is the later phase of Einstein's career. For decades Einstein sought to find a theory to unify gravity and electromagnetism. He stuck to a purely mathematical approach, which he had promoted as the ideal way of doing physics.[467] The approach turned out to be by and large unsuccessful, which can be explained as a mistaken preference ordering of virtues. Einstein valued the virtue of predictive accuracy (or saving the phenomena) over explanatory force. Physics however faced a new and very hard problem, which can be seen as the start-up phase of a new research programme. This called for 'out of the box' thinking and not to hold on dogmatically to established approaches.

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467 For an in-depth study of this episode see Van Dongen (2010). Van Dongen argues that Einstein deluded himself in the promotion of the ideal way of doing physics by downplaying the role of experiments in the earlier phase of his career.

Kaiser (2011) argues that a similar 'shut up and calculate' attitude was prominent in post-war quantum physics. It took until the 1970s for a group of physicists to start asking fundamental questions again and to adopt a speculative attitude in order to come up with explanations for 'spooky' consequences of the quantum theory such as action at a distance through entanglement.[468] These examples show how historians can make use of the fitness of typical strategies and preference for virtues with respect to typical demands of research, in order to explain episodes in past science. It is on such information that a judgemental stance of the historian of science can rest.

Perhaps even more thorough is the classification of strategies offered in Darden (1991). She has focused on strategies used in 1.theory finding, 2.theory change and 3.theory assessment. Her analysis yields a list of heuristic strategies for theory finding and theory change consisting of: reasoning by analogy, matching exemplars to type (invoking a theory type), making use of interrelations (to other fields), moving to another level of organization, making use of representation: a symbolic system or a model can serve as a substitute for the natural system, extrapolation, overpatterning, first generalizing-then specifying, first simplifying (for example by eliminating problematic components of a theory) then complicating, starting with a vague idea and then refining.

Next to this Darden relates strategies of theory assessment to features of theories. These include problem-solving efficiency, generality and scope, simplicity, lack of adhocness, empirical adequacy, explanatory adequacy, predictive adequacy, number of additional problems raised, internal consistency (lack of tautology), systematicity and modularity, clarity, extendability and fruitfulness. In short, she defends a virtue approach when it comes to theory assessment. Her approach is also naturalist because she takes her strategies from historical case material, but there is an extension as well because she takes them to have wider applicability, when similar situations of theory choice occur. Her approach thus comes close to what we have been arguing for above. Unfortunately this initial study has, as far as I

[468] See Kaiser (2011) on counterculture physics in the 1970s. Speculation went quite far because the physicists took active part in hippie culture and sought inspiration in Eastern mysticism.

know, not led to much follow-up.[469] The same holds for Boon's adaptation of the group-grid theory. However, both these analytical approaches to stages of research programmes and aptness of strategies become highly relevant again in light of the present aim to set up a platform in service of comparative evaluations of past science.

Philosophers of science have largely ignored decomposing science into stages of research because most of these stages seem to belong to the so-called 'context of discovery'. The discovery process is thought to be too diverse and unpredictable to allow for systematic analysis. Social constructivists have seriously questioned the distinction between discovery and justification because justification for them rests on acceptance of a theory and this is perceived as a social process. If this is indeed true the qualitative distinction between discovery and justification disappears, as the discovery process simply becomes part of the gradual acceptance of a theory.

I side with the group of sociologists and philosophers who think that is not useful to make a sharp distinction between the context of justification and the context of discovery. Research programmes often consist of strings of theories, including series of modifications of similar theories. Discovery is a gradual process and in order to understand it properly one should include the various phases of development of the research programme. The formulation of a new theory often starts tentatively and changes through the contribution of many researchers.[470] I believe however that the virtue approach can be used *both* for evaluation of the discovery process and for an evaluation of the end products of science. After all we are interested in the quality of past science and this includes all aspects that are relevant to it. Relating strategies of research and the preference for virtues to particular stages of research, as Boon and Darden do, presents itself as a fruitful approach. In the next section I show how this approach can

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[469] An exception is Kvasz (2008). Kvasz identifies three strategies in 19th-century mathematics by which changes have come about, namely recoding, relativization and reformulation. The uptake of his study is that these strategies had the potential to transcend existing research programmes.

[470] Some philosophers, e.g. Hudson (2001), Achinstein (2001), MacArthur (2011), interested in discovery have studied this context with the aim of pinpointing exactly what counts as a discovery, when a discovery was made and who can be credited for it. I don't see how these studies help to understand past science, as such an approach neglects the gradual nature of scientific discoveries and the formation of scientific theories.

also help to mitigate the consequences SSK has drawn from the problem of underdetermination (see chapters 1 and 2).

4.2 The gradual decrease of uncertainty and the problem of underdetermination

Because of the degree of uncertainty in scientific research, especially in the early stages, Kuhn argued that relying on virtues is indispensable:

> "Recognizing that criteria of choice can function as values… allows the standard criteria (i.e. the five virtues, BK) to function fully in the earliest stages of theory choice, the period when they are most needed but when, on the traditional view, they function badly or not at all."[471]

As I have already explained above, in the absence of absolute certainties, we continue to rely on virtues in later stages as well. But this does not mean that the degree of uncertainty defies measurement. On the contrary, during the course of a research programme it is possible to establish to what degree uncertainty decreases, precisely by comparing scores on theoretical virtues.

A good example to illustrate this point is Hacking's account of the gradual discovery of the characteristics of an electron.[472] According to Hacking we gradually discover characteristics of an entity and refine blurry theories into more exact representations in the course of time. Thus Johnstone Stoney (1891) was the first to use the name 'electron' for a unit of electricity, J.J. Thomson applied the term to subatomic particles with negative charge (which had been postulated by Lorentz) and determined the mass of an electron, in the 1920s angular momentum (spin) was added to the concept. Over a period of a few decades the theory of the electron changed a number of times and no one can be credited as the one and only discoverer of the electron. Hacking defends the thesis that the episode can be treated as a connected whole (i.e. as one research programme) because the scientists were talking about the same entity all the time. He resorts to Putnam's causal theory of reference to rule out the problem that

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[471] Kuhn (1977) p.331.
[472] Hacking (1982).

changes in theory over time also lead to changes in reference. Indeed, the comparability of subsequent theories hinges on the assumptions of stability of phenomena.[473] This assumption needs to be part of our platform (see also section 5 below).

Another example of the gradual decrease of uncertainty was given in chapter 4 on the case study of the globule hypothesis. The participants in the earlier situation agreed that something in their explanation was amiss but could not tell what it was. Eventually one version of the globule hypothesis was rejected, which led to a decrease in uncertainty, but at the same time many obscurities remained.

Darden (1991) also stresses that theories are almost never built up in one go. Instead they are the result of a gradual process consisting of incremental steps. Her main case study, in molecular genetics, spans over a period of roughly 30 years after the rediscovery of Mendel's gene theory. The theory was 'finished' in its modern form in 1926 with the publication of T.H. Morgan's *The Theory of the Gene*. During this period a number of scientific controversies were fought and eventually several fields of study contributed to the 'final' theory. All these episodes should be studied as phases of development towards the final theory that has acquired relative stability afterwards. The phases should not be studied as isolated, or localist, historical events, but instead as connected wholes.

Recall that SSK forces a definite closure at every step in science. Every controversy over theoretical choice needs to find a definitive settlement. As it is clear that these cannot be given on unquestionable rational grounds, truth or success, the argument is that we have to look at social factors in order to explain how choices were eventually made. But putting strong demands on every situation of choice does not adequately reflect what happens in science. As Giere (1988) argued, when there still are a lot of obscurities, taking 'strong' decisions on correctness is generally not possible. Only 'weak' decisions on pursuit worthiness and approximate estimations of reliability can be made. The strong decision moment needs to be saved for the moment when the fog is starting to dissipate.

So, as long as uncertainty is too great only weak decisions can be made, but these can still be revoked if needed, or a next step will take an earlier dismissed path into account again, etc. This can go wrong, as

[473] See also Chang (2011).

the demand for a strong decision can be present in historical situations as well. According to Giere it was the too early demand for a strong decision, what held back the mobilist theory of the Earth for such a long time. But it is more important to see that for the explanation of past science SSK's logic becomes far less compulsive when we consider research programmes as a collection of mostly weak decisions. At the end the score on all the virtues becomes compelling, and hence we do not have to resort to social factors to explain theory choice.

In support of this thesis I would like to offer four illustrative examples. The controversy between Hobbes and Boyle was closed according to Shapin and Schaffer with a choice for the experimental programme. They argue that this happened because of social factors. If we isolate this controversy there is possibly no other way to account for the choice that was made in that situation. But if we place the controversy in a wider time frame it can be interpreted differently. In the interpretation of the Scientific Revolution that Cohen (2010) has to offer, the Hobbes-Boyle controversy is no more than a phase of a much larger transformation of three forms of natural inquiry (experimental, mathematical and metaphysical) which underwent shifts on their own but more importantly, which merged together for the first time in the course of the 17th century. Cohen argues that it is this achievement that makes the period so unique, and hence deserves to be called a revolution.

When Hobbes and Boyle quarrelled with each other, the three forms of natural inquiry were not fused together yet. Hobbes was mainly part of the classical natural philosophy, whereas Boyle was part of the group that started to connect speculative philosophy to experimental research. Seen from the wider perspective, Boyle won the conflict not just because of social factors, but because his experimentalism eventually fitted the Newtonian synthesis. As Cohen argues, the 'Baconian concoction' consisting of active principles and ether mechanisms with which Boyle worked, still had to be replaced with Newton's theory of forces. Hence, it is not the case that all of Boyle's ideas were accepted. Later mergers of the three ways of natural inquiry were required to remove further obscurities. Closure of the transformations of the forms of natural inquiry was achieved only when Newton 'rounded off' the Revolution. Hobbes' mathematical approach to natural phenomena found a place in this synthesis as well.

Shapin and Schaffer ask for closure too early and this is why they resort to social factors to explain the choice for the experimental programme. If this controversy is interpreted as a phase of a wider research programme, that is, as a series of strongly interrelated gradual steps, with the Newtonian synthesis as an endpoint, then the attribution of the choice for Boyle's experimental programme in full to social factors is lacking motivation.

Another case study, which illustrates the point, is offered in Rudwick (1985) on the Devonian controversy in the history of geology. The book is about the development in thinking about dating layers of the Earth. Rudwick investigates all the steps that were made in this process. At the onset, opinions differed widely but through a number of crucial transformations consensus emerged. Rudwick concludes that in the end, because of the incremental accomplishments of research, it was irrational not to join the consensus, which two geologists stubbornly continued to do. SSK scholars Collins and Pinch attacked him on this crucial point.[474] They argued that it was equally rational to defend each of the three positions (the consensus and the two deviant ones) that remained at the end of the story. I agree with Rudwick that this is not a correct evaluation of this episode because it does not do justice to the gradual nature by which scholars arrived at the consensus, which involved many 'weak' decisions.[475]

Lets look at two more illustrative examples of how comparative evaluation in historiography of science might work. One is about the so-called Heyerdahl-hypothesis and the other about the persistence of astrology in scientific discourse, which lasted at least until the end of the 18th century.

The dominant theory in anthropology has since long been that the inhabitants of the Polynesian islands originally migrated from Asia. This theory is supported by linguistic and archaeological evidence. Even though there are debates over the period migration started, where exactly migration started from, and the pace by which the 'Polynesians' spread over the islands, the hypothesis that migration started from Asia is generally accepted. There exists only one genuine

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474 Pinch (1986), Collins (1987). See also chapter 2 where this discussion was mentioned too.

475 See also the flow chart of all the decision moments in Rudwick's book. I believe this conclusion would also apply to the Pflüger-Minkoski controversy over the cause of diabetes, which was discussed in chapter 4.

alternative hypothesis, which has been put forward by the Norwegian zoologist, geologist and anthropologist Thor Heyerdahl (1914-2002). Heyerdahl got interested in Polynesian culture in the 1930s while living on the isolated island Fatu Hiva. There he came up with the theory that the inhabitants of Polynesia did not stem from Asia, but instead originally came from South America. With this theory he could explain the similarity between the legendary Polynesian 'Tiki' and a well-known Inca hero from ca. 500 A.D. 'Kon-Tiki'. According to legend 'Kon-Tiki' had to flee South-America because he was threatened with murder. Couldn't this have started the migration into the Pacific? Another piece of linguistic evidence was given by the Polynesian legend of the mythical homeland 'Hawaiki'. Next to this Heyerdahl also noted that island plants such as papaya, breadfruit, pineapple, sweet potato, pumpkin, and wild cotton were native to South America. Early European explorers had noted these plants already growing in the Polynesian islands when they arrived, so Heyerdahl saw their presence as evidence that ancient seafaring people had come from South America to Polynesia.

The South America theory thus had an appeal in terms of explanatory force. It was however rejected by most scientists because the theory lacked an explanation how the ancient seafarers could have reached the islands using only simple rafts. Without proper sailing boats they could never have traversed the ocean over a vast distance, which is much larger from South America in comparison to Asian starting points. Heyerdahl's theory was thus rejected because it could not provide a satisfactory explanatory mechanism. This resembles the debate in geology over de mobilist theory of the Earth masses. This theory could explain a lot of phenomena more elegantly than the stabilist theory, but it could only be accepted when a mechanism for the transportation of large landmasses became available.

Heyerdahl suggested that because the current was in the right direction, even with simple rafts the Polynesian islands could be reached from South America. This was seriously doubted because of the risks involved with bad weather conditions, potential shortage of water and food, etc. Heyerdahl however was determined to proof that it was possible to reach Polynesia from South America and he set up an expedition with a ancient raft model he wittily called the Kon-Tiki. When shortage of food and water occurred the crew had to live on rainwater and fish. To make a long story short, the expedition left in 1947 and indeed succeeded and reached the Polynesian islands (landing on the atoll Raroria) after traversing nearly 7000 km.[476]

The success of the expedition (which was later repeated with the same result) did not prove Heyerdahl was right, but at least made the South America hypothesis more credible. However, because there was much more linguistic and archaeological evidence in support of the Asia hypothesis and because Heyerdahl's theory was less simple as it required more assumptions, the vast majority of the scientific community kept rejecting the idea that Polynesians originally migrated from South America.

While it is essential for scientific development to allow dissenters to follow 'wild' ideas which promise to be fruitful, such alternatives to the mainstream should after a while start to score higher on the set of theoretical virtues, otherwise they lose their attractiveness and credibility. This is what, for example, happened to the mobilist theory, but not to Heyerdahl's migration theory. Yet, especially after the expedition succeeded, Heyerdahl's theory could not be rejected and deserved to be treated with respect. However, it goes to far to say that both theories were equally credible, as an SSK historian would do. The theories in question were comparable on a number of virtues and on balance the older Asia hypothesis scored better and continued to do so.

The position of the Asian theory has been strengthened in 2005 by a biological argument revolving around mitochondrial DNA.[477] Mitochondria reside in the cell cytoplasm, the fluid-filled space between the cell nucleus and the outer membrane. There are thousands of mitochondria in each cell and each has its own small circle of DNA, the so-called mtDNA. mtDNA is inherited through maternal line only. This means that while a person's nuclear DNA comes from a large number of ancestors, mtDNA can be traced back to a single ancestor. mtDNA is so abundant in cells that traces can still be found in human remains many thousands of years old. Therefore, with mtDNA the maternal lines of living people in different parts of the world can be connected. In theory, every person should have a copy of

[476] The ins and outs of the journey can be seen in a documentary about the expedition, which Heyerdahl issued in 1951 and which was dramatized in the movie *Kon-Tiki* in 2012.
[477] Trejaut (2005).

mtDNA identical to this original ancestor. In practice, this is not the case because random errors occur in the replication process. Different populations will experience mutations at different locations in their mtDNA, and these will be passed on to future generations. The result is that some groups of people will end up with mtDNA that is very different from another group. By comparing how much mtDNA different populations have in common, an ancestral relationship can however be determined and dated.

mtDNA research provides a link between Polynesians and 9 indigenous tribes from Taiwan. Today, roughly 2 per cent of the inhabitants of Taiwan are direct descendants of the island's indigenous people and have a unique culture, language, and genetic makeup. While Chinese immigrants colonised Taiwan 400 years ago, archaeological records show that Taiwan may have been inhabited for the last 15 000 years. The researchers found that the indigenous Taiwanese, Melanesian, and Polynesian populations share three specific mutations in their mtDNA that do not occur in mainland East Asian populations. Furthermore, they showed that there were enough different mtDNA mutations between the mainland Chinese population and the aboriginal Taiwanese to support the archaeological findings suggesting a long period of habitation. These results show that Polynesian migration most likely originated from people identical to the aboriginal Taiwanese. The findings provide the first direct evidence for the common ancestry of Polynesians and indigenous Taiwanese, and suggest that Taiwan genetically belongs to that region of insular Southeast Asia that might have been the point from where Polynesians started their migration across the Pacific, followed by later cultures that developed from their descendants in East Indonesia and Melanesia.

Is this the final word in the debates over the origin of the inhabitants of the Polynesian islands, not just discarding the South America hypothesis, but also a set of alternative Asian hypotheses? I believe that conclusion goes too far. Because of mutations in mtDNA no more than a degree of resemblance can be established between people living now and their ancestors, and this makes precise dating of historical periods uncertain. Further, it has not been established beyond doubt that Taiwan has indeed been inhabited for thousands of years. As always, a ray of uncertainty shimmers on.

Yet even without definitive proof we can see that the two theories are not equivalent all the way. The scale of trustworthiness, dependent on relative scores on a set of theoretical virtues, has always pointed in the direction of the Asian hypothesis and with the latest DNA research this has even more strongly been reinforced.

The benefits of the evaluative approach suggested here can be demonstrated with another example. Historiography of science has yet to find a way to explain how it is possible that astrology continued to exist until the end of the 18th century as a genuine scientific practice. How can Galileo, Kepler and Boyle, who have played such an important role in the scientific revolution and the birth of modern astronomy, at the same have been active drawers of horoscopes and performers of elaborate astrological calculations? This question is very much like the question why Newton, next to his scientific activities spent so much time on alchemy and biblical studies. Historians of science have come to understand this as an ill-directed question. All Newton's activities can be understood as being part of the same religiously motivated worldview and we must be careful not to equate his natural inquiry too easily with modern science. I believe we can make a similar argument with astrology, although this will be a bit more difficult.[478] To the modern reader astronomy and astrology appear as two mutually exclusive approaches to the same kind of phenomena. How can the same people ever have practiced these at the same time without running into serious contradiction? I believe our evaluative approach can both explain this *and* offer an explanation why astrology eventually became marginalized in favour of astronomy.[479]

The first thing to realize is that astrology and astronomy are comparable. The incentives to do astrology are present in astronomy as well. Astrology is first about the connection between macro and micro. There is supposed to be a direct relation between the position of heavenly bodies and the lives of individuals on Earth. Secondly astrological calculations are performed to predict the future and hence to gain control over what happens. They reduce unbearable feelings of uncertainty. In Newtonian mechanics the laws of gravity relate all

[478] This historiographical problem was the topic in Darrel Rutkin, 'How to Accurately Account for Astrology's Marginalization in the History of Science and Culture: The Essential Importance of an Interpretive Framework', (public lecture, Utrecht 2015).
[479] In science of course, popular culture is another matter.

bodies in the universe to each other. In a completely different way to be sure, there is thus also a connection between macro and micro in Newtonian mechanics. Moreover mechanical determinism allows one to predict the future. If position, momentum and forces of bodies are known one can predict the position of the bodies after a period of time. This predictability was taken as a sign of divine power, with the laws of nature as an expression of God's will. Recall that the occurrence of unexpected events could be taken as a lack of divine control.

It would, in my view, be wrong to treat astrology and astronomy as two competing systems of thought, where the former eventually gave way to the latter for social reasons. Equally, it would be a mistake to say that astrology was one of those irrational medieval practices, which soon lost its appeal after the advent of rational mechanics. At least in part, early modern astronomy could be considered as serving the same goals as early modern astrology.[480] For this period we must treat both of them with equal respect. Was it not rational to expect that, even in the absence of a clear connection, the two theories were complementary, and together led to greater possibilities of prediction and control of life on Earth and a deeper understanding of the Divinity?[481] And yet, comparability means that theories can be weighted according to a set of virtues. Here I would argue that Newtonian physics gradually edged out drawing horoscopes because it continuously and increasingly scored better on virtues such as predictive accuracy and explanatory adequacy. But only when this body of evidence emerged, and when no proof of any connection between Newtonian physics and astrology could be given, was astrology turned into a pseudo-scientific phenomenon. It is therefore not surprising that natural philosophers until the end of the 18th century continued to be seriously involved in astrology as well.

Golinski has perceptively argued that symmetrists are unable to capture science in terms of successive stages of a research programme because this requires a little backward historiography, not directly from a presentist point of view, but from the point of view of the endpoints of gradual processes of finding solutions to scientific

[480] See also Aaron Spink, 'The Mechanical Resilience of Astrology' , conference paper Utrecht 2015.

[481] For changes in astrology caused by the scientific revolution see the excellent Von Stuckrad (2003).

problems.[482] A collection of research programmes provides historiography with a collection of narrative plots, which the purely forward-writing historian cannot use for historical reconstruction. I have defended the thesis that denying access to these plots is unnecessarily restrictive and can even be counterproductive. The examples show that with the distinction between strong and weak decisions we can take the sting out of the underdetermination argument that underpins SSK. What this requires is first, the assumption that we can make use of hindsight to identify the beginning and endpoints of past research programmes. We can see when theories of past research programmes have achieved relative stability, and treat them as if they were a planned whole. Second, we need a shift in perspective to the gradual decrease of uncertainty as the main driving force in science, so that we can study the development of research programmes and assess them accordingly. As I hope to have shown, there are good arguments to defend both these assumptions. The gain of this approach is that we can treat conflicting theories with equal respect and yet are also able to normatively compare them over longer periods of time. With such a comparative approach we can account for both the persistence and eventual demise of scientific theories, as in the cases of Heyerdahl's hypothesis and the stabilist theory in geology, and of more general approaches to natural phenomena, as in the case of astrology.

4.3 The succession of research programmes

In closing section 4, I want to address the issue of the succession of research programmes. Koyré once asserted that thought can only make progress through the obscure and the confused and not from clarity to clarity as Descartes had proposed.[483] We have framed the idea that thought proceeds from obscurity to clarity in terms of the notion of certainty with the moderation that thought proceeds from a high degree of uncertainty to a high degree of certainty. It is very likely that one day received certainties will be challenged again and then a transition to a state of high degree of uncertainty will occur.

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[482] Golinski (2005) pp. 186-206.

[483] Koyré (1978).

A recent example of experimental results inducing a new 'wave' of uncertainty comes in the field of astrophysics. The Icecube detector stationed on the South Pole has the unique ability to detect neutrinos from outside our solar system because it can separate these from the ones emitted by the Sun. The theory of gamma flashes in astrophysics predicts neutrino emissions from heavy X-ray outbursts outside the solar system. The Icecube however does not detect these at all. This null result thus yields uncertainty about the existing theory of gamma flashes.

The history of science is full of examples that show how hard it is to adjust one's theories of the world in light of discordant information. Just to mention a few examples. In the beginning of the 17th century Cremonini refused to look through the telescope because he had no reason to doubt his cosmology, certainly not when a deceptive new instrument would engender this doubt. Voltaire scorned the idea that there could be fossils. Lavoisier told the Academy of Science in Paris in 1769 that only uncivilized peasants believed that stones fell from the sky. Spaceflight was considered complete fantasy until the 1930s.[484] Questioning certainties is sometimes the hardest thing to do, not just because one wants to hold on to long-trusted opinions, possibly connected to vested social interests, but also because certainties can be taken so much for granted that they do not come into question because that are not consciously realized.[485]

In other cases however scientists realize flaws in their theories and openly communicate about them. Newton, for example, knew about a number of problems with his theories. His theory of the moon could not be made compatible with the then available data. Because he could not accept the wave theory of light, he had to explain periodicity of colour phenomena in an ad-hoc manner. Further, his ether theory just reduced one force to another and did not shed any light on the working of force over distance.[486] These problems could not be solved during Newton's lifetime because alternative theories were not available.

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484 See Alvegren (2010).

485 On this discussion a key text is Wittgenstein's *On Certainty*.

486 Cohen (2010) pp.239-239. See also the series of open questions Newton included in his *Opticks*.

In the case of Lord Kelvin's calculation of the age of the Earth there was eventually an alternative available. In 1904 Rutherford presented an alternative calculation, which included radioactivity as a source of heat. Kelvin had recognized the Sun as the sole a source of incoming energy. With the extra amount of heat and the same forces responsible for the cooling process, the age of the Earth could be estimated as much older. As discussed in chapter 4 on the notion of retrospective error, is often possible to establish what precisely is at fault in an existing theory only when an alternative explanation presents itself. This holds both for situations in which problems are recognized and those where anomalies are explained away.

Kuhn has rightly called the start of doubt the essential tension in science. However we cannot accept his theory of paradigm change because this theory narrows down the possibilities for inter-theoretic comparison too much. I think that the transition between research programmes is better explained through an 'elements and relations' approach. It is not very likely that each romantic phase (to use Boon's expression) will start things completely anew. Rather things go as Chalmers has aptly put it:

> "The general idea then is that any part of the web of aims, methods, standards, theories and observational facts that constitute a science at a particular time can be progressively changed, and the remaining part of the web will provide the background against which a case for change can be made. However it will not be possible for changing everything in the web at once, for then there would be no ground on which to stand to make such a case."[487]

Studying change against a stable background is exactly the point of Laudan's reticulation model with aims, methods and theories, discussed in the previous chapter. Other approaches decomposing research programmes in a number of elements can be found in the idea systems of Amsterdamska (1990), the brick model of Galison (1988) and my own hybridization perspective on discipline formation given in Karstens (2012). These approaches share the idea that we should look at the relations between all the constitutive elements and explain change in terms of changes in relations. Change is thus nearly always a stepwise and gradual process. This dynamic approach is well suited to capture instances in science in which previously discarded ideas

[487] Chalmers (1999) p.170.

resurface. It also deals well with instances in which transfer or ideas and methods from one field to an on-going research programme of another occur.[488] This is much more difficult to account for with Kuhn's model of scientific change in terms of paradigm shifts.

Also, when the 'core' (the central problem, aim, goal or metaphor) of the research programme changes, and we thus get a transition from one research programme to another, not all aspects of the old programme are thrown away. Acquired insights may still find a place in the new programme, even if the new research is not a fully cumulative continuation of the old. Such a perspective has to accept a degree of contingency in the development of science as the great number of interactions, relevant to its development, does not follow clearly ordered patterns. How science proceeds depends on the way the elements are fused together and on the number of existing alternatives. Provided the eventual choices have been rational, that is, when they have followed the pursuit of one or more theoretical virtues, historians of science simply have to follow how things went without being judgemental about this course.

Such historical naturalism does however not pre-empt all possibilities for judging the quality of science that is produced through the execution of the research programmes. Even if we can often not judge whether the correct new research programme was chosen, once chosen however, processes *within* the development of research programmes can be assessed. With naturalism first, we have said farewell to the absolute and instead chosen 'the typical' as our standard of comparison. What is typical sits between the absolute and the relative and can be made part of the platform. Thus assessments of past science hinge on a comparative evaluation of typical virtues and typical strategies, which can hopefully be related in more detail to typical phases of research. Although a number of studies provide hopeful indications, it is still very much an empirical issue whether we can establish more such typical inference patterns, which, when available, can be used for evaluative purposes. Thus like section 3, section 4 itself

[488] A much-studied case in this respect is the influx of physicist such as Delbrück and Crick into biological research. See Boon (1983), Darden (1991), etc. A case study in point is also my account of a new approach to the study of language, namely historical and comparative linguistics, which incorporated, among other things, ideas from comparative anatomy and physics (see Karstens 2012).

strongly suggests a research programme for history of science that promises to be very fruitful, but so far has hardly been carried out.

5 Anachronisms and the use of present-day scientific knowledge

Next to the set of typical virtues and strategies of research, our platform has two other constituents, namely anachronisms and present-day knowledge. In this section I want to indicate how historians should use these, as they can easily be misused. Because of this, many historians of science are nowadays reluctant to use either anachronistic language or present-day insights in their explanations of past science. I hope to show that the fear to commit 'Whiggish' sins can however be allayed. Moreover my aim is to demonstrate how access to anachronistic concepts and present-day knowledge underpins a sophisticated form of evaluative historiography.

5.1 On the use of anachronistic concepts in historical explanation

In the discussion of the use of anachronisms in historical explanation we should distinguish between at least two types of anachronistic language. The first type consists of terms referring to things in the world, such as the entities postulated in science. The second type is given by the use of analytical terms such as disciplinary categories. With respect to the first category extreme caution is required. Historians should never attribute thoughts about the world's ontology to past actors, which they did not have. The concept of atom for example was already known in Ancient Greece. The concept referred to something that cannot be divided any further. Things, which did not contain further parts, were seen as the fundamental building blocks of the world. Nowadays atoms are no longer seen as the fundamental building blocks of the world, as we think that there are also sub-atomic particles and who knows what may lie beyond them. Instead, atoms are pretty well defined entities consisting of a nucleus which contains protons and neutrons and which is much heavier than the surrounding electron cloud. It does not make sense to assert, as some writers do, that the old Greek views on what atoms look like and how they behave were incorrect, because the Greeks were not thinking about a comparable entity at all.

Mistaken usage of anachronisms also comes about through origin hunt and explaining the past as a preparation for the present. An example is asserting that while 18th-century chemists made use of the concept of phlogiston, what they actually meant was oxygen. This is misleading for several reasons. To think in terms of oxygen requires a wholly different view of the structure of matter, the phlogiston theory was intended to do more work than the concept of oxygen in explaining fire, and as such there was more than one interpretation of the phlogiston theory in competition. Moreover it is unclear what kind of oxygen concept we are talking about, as that of Lavoisier differed markedly from the present-day understanding of the concept.[489] Brushing over all of these nuances can only have a negative impact on historiography of science.

Historians of science thus have rightfully developed the utmost sensitivity to such nuanced differences. However, it does not follow that all anachronistic use of referring terms should be avoided. Following Hacking, I think that the problem of reference *within* the execution of one research programme is not that great. In his example of the electron given above, gradually more and more characteristics are attributed to the concept of the electron. Interpreting every change in descriptive properties as a change in reference is not very useful. I think that, at least within one research programme, we can assume that subsequent theories referred to the same entities in the world. They were all contributions to an understanding of the same set of phenomena and this happened against the background of a host of shared assumptions.[490]

A second way in which modern concepts of entities can be allowed is when current understanding of natural phenomena helps to gain deeper insight into historical contexts. On the assumption of the continuity of natural phenomena, the thought and actions of past scientists, struggling to explain them, can be better understood if we

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[489] See Chang (2009).

[490] As we saw, Hacking finds support for his view in Putnam's causal theory of reference. But see also the discussion of Wilson's principle of charity in interpretation in chapter 3. The original principle of charity was created precisely because we do not want to let reference change with every change in description. The solution Wilson offered was a 'best fit' of an entity with a description. If there are descriptive changes the same entity can still be the best fit.

make an assessment of the differences between their theories and ours. In this way, present-day knowledge is not used to translate past terms into ours or mould past thoughts into present ones, but instead functions as a comparative standard. Differences between past and present theories can reduce the number of acceptable historical interpretations. But they can also lead to an expansion of our cognitive horizon. Both these effects will be discussed in section 5.2 below, as this discussion is effectively about the use of present-day knowledge in historiography of science.

Lets now turn to the second category of anachronisms involving the use of analytical concepts with which we explain, classify and judge the past. To what extent can such concepts be allowed if they were unknown to past actors? And to what end should we use them? To some extent, the use of language unknown to past actors is unavoidable because historians necessarily have to write in the language of today. However, the real point of discussion is whether we can support historical interpretation through the use of anachronistic analytical categories, such as for example, scientific disciplines. The term 'biology' for example, most probably came into its modern use with the six-volume treatise *Biologie, oder Philosophie der lebenden Natur* (1802–22) by Gottfried Reinhold Treviranus who defined the discipline as follows:

> "The objects of our research will be the different forms and manifestations of life, the conditions and laws under which these phenomena occur, and the causes through which they have been effected. The science that concerns itself with these objects we will indicate by the name biology [Biologie] or the doctrine of life [Lebenslehre]."[491]

Given that the term 'biology' came into use only from around 1800 onwards, can we say that Aristotle, who certainly never used similar terminology, was a biologist? And what about Linnaeus, who did use the term 'biologi' on occasion in his writings?

Scholars warning against the identification of Aristotle and Linnaeus as biologists do so in order to avoid the error of fitting past actors into straitjackets. It is thought that in such historiography the past is carved up before it can speak to us, and this has the effect of missing out on all kinds of relevant historical particularities. Cunningham and Williams

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[491] Given in Richards (2002).

(2003) argue that instead of looking for the origins of modern science we should be looking for the modern origins of science. They argue that only from 1800 onwards, natural inquiry started to resemble present-day science qua disciplinary nomenclature, organization, institutionalization, and the important place it occupies in a secularized civil society. The strange effect of this argument is that the term 'science' acquires an even sharper qualitatively distinct meaning, compared to the view that modern science started somewhere in the period of the Scientific Revolution. While the aim of these authors is clearly to fight such essentialism and to take away the 'sanctity' of modern science, they actually run the risk of making science even more clearly stand out as a very special activity.

In my view it is counter productive to work with a list of characterizing properties which together define an analytical concept, and then inquire whether past activities fit this concept or not. We should work with a thin definition of the analytical terms we intend to use. The purpose of applying anachronisms in this way is to make sense of past activities, which otherwise appear as incoherent or not meaningful at all. But the point is also to gain a deeper understanding of the very analytical terms we started out with.[492]

This is most clearly argued for in Jardine (2000). Jardine makes a distinction between vicious and legitimate uses of anachronisms. Vicious anachronisms lead to incoherent (or simply incorrect) historical interpretations. Jardine gives the example of condemning Tycho Brahe's conduct, in a dispute with Nicolaus Reimers Baer over the heliocentric hypothesis, as intemperate and irrational, given the violence and threats he uttered in private and the refusal to meet his adversary in public. Jardine points out that Tycho, as a highly ranked nobleman, was acting in accordance with the rules of social conduct of the time in his duel with a much lower-ranked opponent. Jardine concludes: "To apply our bourgeois categories of temperance and rationality to such conduct within an honour-based courtly social formation is surely to commit gross anachronism."[493]

Legitimate use of anachronisms requires a proper attention to the material, psychological, social and institutional conditions in which past science took place. According to Jardine, the problem with

[492] For a comparable view with reflections on Aristotle see Hull (1979).
[493] Example and quotation taken from Jardine (2000) p.260.

anachronisms stems from insensitivity to these conditions, and not from an insensitivity to actor's categories. If historical explanation is sensitive to these conditions anachronisms can legitimately be used. Applying them can lend past activities a sense of coherence that it would be difficult to establish without the application modern categories.

Identifying Aristotle as a biologist would be mistaken if we meant by this that he was *solely* a biologist. Aristotle was engaged in a great variety of activities and there were probably no strict lines of division between these activities. The identification would be mistaken also if it were supposed to mean that Aristotle's investigations were embedded in a disciplinary infrastructure (with Linnaeus one feels that this is far less a problem).[494] Still, some of Aristotle's natural investigations may fall within the broad definition of biology as 'the study of living organisms.' If we start with this identification we can then proceed to find specific aspects of Aristotle's research and compare this to later research. This would allow us first to uncork implicit assumptions and background beliefs in the historical situation under study and second, through the comparative procedure, gain a deeper insight in the concept of biology itself.

Jardine too points out that we can start historical research by using an anachronism, but this may lead to clarification of the very presuppositions surrounding this category:

"In most cases we cannot first ascertain the presuppositions of a disciplinary category and then, armed with the list of presuppositions, check out the historical record to see where and when they were first realized. Rather in the course of historical investigation the presuppositions of the disciplinary category and the conditions of the emergence of the discipline are progressively clarified."[495]

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[494] Bod (2013) projects present-day humanistic disciplines on past periods in which these did not exist. His analysis of past contributions to the humanities requires a comparative analytical framework, and this is why he projected present-day disciplinary boundaries onto the past. Jardine is quite critical about such anachronistic application of disciplinary categories because he thinks that once we speak of disciplines we presuppose a clearly institutionalized infrastructure. If handled with care I believe however that Bod's use of disciplinary categories is defendable. See also my review of the Dutch version of his book in Karstens (2011a).

[495] Jardine (2000) p. 262.

Legitimate application of present-day categories in historiography thus forces a circle of interpretation, which was discussed extensively in chapter 3. We first use a modern category, like a disciplinary name, and from there we start to study differences between past and present in the activities that are supposed to fall under the same heading. This is similar to the approach to the principle of charity defended in chapter 3, which first supposes agreement between past and present and only then starts to investigate differences. In fact, using anachronisms this way is just an instantiation of this general principle. Proper use of anachronisms thus equally requires sensitivity towards the full range of presuppositions (material, social, psychological and representational) attached to *our own* categories.

The whole interpretation process can lead to conclusions about the question whether or not our analytical terms have indeed helped to clarify and/or to classify the past in a fruitful way. This presumably also holds for the application of colligatory concepts, i.e. concepts binding a set of otherwise disparate facts, such as the Renaissance, the French Revolution or the Second World War, which are of fundamental importance to historical understanding.[496] One of the ideas of 'historical epistemology' as it is defended in Rheinberger (2010) is to trace the history of the very analytical concepts we use in the study of science such as objectivity, rationality, positivism, empiricism, etc.[497] If done properly such research too will be an instantiation of the general circle of interpretation.

The distance between our linguistic categories, presuppositions and patterns of thought and the ones of the past can be surprisingly great. Mistaken interpretations, resting on assumed similarities are therefore quickly made. However, restricting oneself solely to actor's categories in historical explanation is a self-defeating remedy to this problem. If we need elaborate hermeneutic circles to gain a deeper understanding of our own categories, why should we assume the actor's categories were always crystal clear to past participants?

Adding extra-contextual information can actually help to gain a deeper understanding of a particular context, including the categories

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[496] See Koster (2009).

[497] There is a strong parallel to Koselleck's project in *Begriffsgeschichte*. See his introduction to a collection of *geschichtliche Grundbegriffe*, published in 1972. A clear example of such research is of course the work of Daston and Galison on the changes in meaning of the notion of objectivity.

that were used in this context. First, in broadening the horizon we can put the contributions of past scientists in perspective. This timeframe cannot be too large because then we are indeed involved in interpreting the past as a preparation for the present. But, as has been argued above, with a temporally restricted focus on research programmes, it is justifiable to interpret specific historical episodes as phases belonging to the same programme. Secondly, the use of a circle of interpretation avoids the danger of producing essentialist historiography.

5.2 On the use of modern knowledge in historical explanation

The use of modern knowledge, if applied for the right reasons, is another way to include extra-contextual information in accounts of past science. The first topic I want to address is the use of present-day knowledge in historical explication, on the evaluative assumption that present-day theories about the same phenomena are better than past theories. The second topic involves the research method of restaging experiments and the role this method can play in evaluative historiography.

On the first topic we can be brief because it has already been discussed in earlier chapters. Positive examples of the use of present-day knowledge have been given in chapter 1, section 8, involving interpretations of Galileo (puzzling relation between air resistance, speed and period of oscillation of pendulums), Aristotle (puzzling claims about human bodies) and Thomson (puzzling experimental measurements on electrons). Modern understanding of the natural phenomena in question reduces the number of possible interpretations. Modern understanding actually strongly suggests a new interpretation of the role of experimental evidence in the work of Galileo, that Aristotle must have been relying on textual evidence, and that Thomson must have been relying on his first set of measurements. Historical investigation that makes use of present-day knowledge in a careful way can thus enhance insight in the thoughts and actions of past actors, in the theories they defended, the problems they found pressing, the doubts they had, etc.

The purpose of this is not to blame people for missing things, but to gain a deeper understanding of their motivations, actions and thought patterns. It is possible to be judgemental, while maintaining the utmost respect for past participants and the theories they defended. This was shown in chapter 4, section 3, in relation to the notion of 'going amiss'. On the assumption of agreement between past and present on a set of basic cognitive attitudes we can treat all past practitioners in a scientific controversy as rational, while still allowing for the assessment that one theory was better than another. Possibly, it also requires taking into account all the phases of the research programme of which the controversy was part.

A deeper understanding of historical context can also be acquired through posing comparative questions such as the Needham question, why modern science did not arise in China. Such questions are evaluative, because they involve judgements on the desirability of certain historical developments. Comparisons can lead to useful insights in the determining factors responsible for historical change. On the basis of this we can possibly further the project of evaluative historiography, if it is possible to infer typical positive and negative conditions for scientific progress.

Historians do not need to be acquainted with all details of modern knowledge in order to use it for historical explanation. In the example of the history of geology given earlier, if one knows that a mechanism has been found that can account for the movement of large land masses, perhaps only a bit more detail about the workings of this mechanism is all that is required to use it in accounting for the controversy between the stabilists and the mobilists. Proper use of modern knowledge does require cooperation between historians and scientists, and this can surely be intensified. Scientists can benefit from this cooperation too, as historical scholarship can reveal forgotten lines of research or discover things by restaging past experiments.

Restaging past experiments is a very interesting method of investigation, which historians are beginning to use more often, also for evaluative reasons. I attended a session on restaging experiments in alchemy at the ICHSTM 2013 congress in Manchester. A number of speakers in this session, most notably Hasok Chang, argued that restaging of experiments is not just about historical replication, i.e. to be as accurate and authentic as possible. Physical replication of phenomena, possibly aided by means not available to the historical actors, can be sufficient for recovery of the experiment.

Restaging experiments can have a number of beneficial effects. The very process of experimenting can bring us closer to grasping what past actors went through, the thoughts they must have had and which problems they had to solve. Restaging can increase historical understanding of the techniques that were used but were never written down. What had been left implicit can in this way be made explicit. This may lead to better insight in the experiments than past participants had, because for them a lot of things went without saying.[498] There are situations in which the help of modern aids can put us in a better position than the past actors were in, and this makes it possible to come to different evaluations of experimental results obtained in the past.

Finally, restaging experiments can be an act of discovery in itself. It can lead to digging up phenomena that have been lost or possibly discarded as irrelevant at the time, but which are not irrelevant from a modern perspective. This may help to gain insight into the grounds on which experimental results were selected in the past, which then leads to more just assessments of these selections. In general, restaging experiments can revive lines of research, which have been lost. The history of science can it this way have a function complementary to present-day scientific investigation.[499] Such feedback on modern practice again makes on think about the circle of interpretation. We use modern knowledge for the physical replication of experiments, and the results we get in some cases bear upon this body of knowledge again.[500]

This last point shows how close the application of anachronisms and the use of modern knowledge in historical interpretation actually are. In order to apply both of these in a concise manner similar conditions need to be met. In most cases the point of using present-day categories is to create a useful standard of comparison. The first step is evaluative

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[498] One must be careful here, as written-down procedures may also have been deliberately indeterminate to allow a degree of freedom to the experimenter. Also the written-down procedures in alchemy were in use for demonstration purposes only and not for explorative purposes. These are precisely the kind of presuppositions Jardine has been calling attention to.
[499] Chang (1999) argues for history of science as a complementary science in this way. See also Chang (2004).
[500] Relevant literature on restaging experiments can be found in many leading publications on the subject by Peter Heering. Also relevant is Sibum (1995).

in character but it triggers an interpretation process which has to attend to all specific aspects of historical contexts and which may feed back into the assumptions we started out with. Only in this sense anachronisms and present-day scientific theories can be part of our platform.

6. Conclusion

The platform may at first sight look like a disparate bunch of factors. However, the constituting elements are treated in basically the same manner. All the elements involve a specification in both type and occurrence. Moreover, the evaluations of past science they allow for is in all cases of a deeply comparative nature: we can evaluate something only in comparison to something else. Rational factors are brought back into historical explanation in terms of the pursuit of a set of theoretical virtues. This set of virtues is loosely defined on the type level. On the occurrence level, interpretations of the virtues in question may vary. Also the hierarchy between them is not predetermined and hence can vary from one historical context to another. Not all virtues need to be taken into consideration in all situations of theory choice. Some virtues may simply be irrelevant in particular instances of theory choice. This degree of relevance may vary as well.

Taken together, this probably is the thinnest approach possible to the concept of rationality interpreted as the pursuit of theoretical virtues. This makes the approach strongly naturalist: we have to attend to historical particulars in sufficient detail in order to make proper sense of theory choice. Only in this way can we come to a sophisticated comparison of theories. The approach does however retain a thrust of normativity. The thin parameters set at the type level provide the boundaries within which theory choice has to be performed, in order to count as rational. Although *prima facie* no hierarchy in the set of virtues is assumed, it is to be expected that such preferences can be generalized from historical case studies. This would strengthen the normative thrust of the approach, as choices made in the past can be evaluated on the grounds of desired patterns of inference. The suggested approach thus provides history of science with a research agenda. That this is still a novel direction can also be inferred from Daston and Galison. They write:

> "To claim that there are multiple virtues is very different from the claim that all virtues are equally well-grounded and that whim may decide among them. It is a commonplace in politics and ethics that hard choices sometimes need to be made but this idea is something of a novelty in epistemology."[501]

There are no absolute principles to which we can resort to decide what the correct choice is. How the virtues hang together is by and large an empirical issue that must be studied on a case-to-case basis. In any case this will produce a complex comparative weighting procedure. As we have seen the same holds for historical interpretation that includes the use of anachronisms and present-day knowledge. Complexity itself is however not an argument against the normative side of the approach that is defended here. Science is an enormously complex activity and the mistake of philosophers of science has been to create a highly simplified picture of it. However, I believe that the complications of evaluative historiography, based on the platform of this chapter, should not be exaggerated and that the analytical improvement it is supposed to produce remains tractable.

We have sought to occupy a middle ground or a golden mean between absolutism and particularism. This was found in specifications of what is typical in a platform. This platform serves as comparative ground for assessments of past science. It is this approach that in my view allows for a matured evaluative historiography. Contextualist historians can wholeheartedly embrace it because it only helps them to enrich their historical narratives.

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[501] Daston and Galison (2007) p.376.

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.[502] 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.[503]

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-empts 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".[506]

For posthumanists the blurring of the distinction between the natural and the social is not news, as they have been arguing all 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. [507] 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.[508]

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[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.[509] 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.[510] 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).

[510] Daston and Galison (2007) p.377.

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.

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Samenvatting in het Nederlands

Pluralisme binnen Parameters: naar een volwassen evaluatieve wetenschapsgeschiedschrijving

Wetenschapsgeschiedenis raakte in de 20e eeuw geleidelijk geïnstitutionaliseerd als aparte discipline. Aanvankelijk was het vak sterk normatief van aard. Wetenschapshistorici stelden zich ten doel de goede wetenschappelijke traditie te scheiden van storende invloeden daar op. De gedachte daarachter was dat, gegeven de centrale positie van wetenschap en techniek in de moderne samenleving, er de noodzaak bestond om de krachten van wetenschap en techniek in goede banen te leiden. Omdat wetenschap nooit 'af' is (de schok van het omvallen van het lang als vaststaand aangenomen Newtoniaanse wereldbeeld liet dat maar al te duidelijk zien) werd voor de vraag hoe het proces van verandering als een proces van vooruitgang te begrijpen een historisch geschoold perspectief van groot belang geacht. De prestaties van 'helden' zoals Kepler, Galileo en Newton werden ten voorbeeld gesteld aan huidige wetenschappers. Zulke voorbeelden waren ook sterk moreel geladen. Eigenschappen die nodig waren voor de juiste wijze van wetenschapsbeoefening zoals oprechtheid, het streven naar zuiverheid, het uiterste van jezelf vergen, nederigheid en het delen van kennis, werden tegelijk gezien als essentiële menselijke deugden. George Sarton sprak zelfs van wetenschapsgeschiedenis als de belangrijkste culturele behoefte van zijn tijd. Met het wegvallen van traditionele machtsstructuren, zowel religieus als adellijk, zag hij wetenschappelijke kennis als de belangrijkste machtsfactorAlleen een sterk wetenschapshistorisch besef zou een brug kunnen slaan tussen wetenschappelijke kennis en culturele ontwikkeling, tussen generalisten en specialisten en uiteraard tussen heden en verleden, en zo maatschappelijke vooruitgang waarborgen.

In de afgelopen decennia is het vak wetenschapsgeschiedenis qua taakopvatting en doelstellingen volledig veranderd. De meeste wetenschapshistorici zien wetenschap niet langer als uniek, maar in plaats daarvan, als een gewone activiteit net als andere menselijke activiteiten. Bijgevolg is wetenschappelijke kennis nooit helemaal zuiver maar altijd verbonden aan sociale belangen en cultureel-maatschappelijke waarden. De zorg over de rol die wetenschap en techniek in de maatschappij spelen is een centraal punt van aandacht gebleven maar het idee dat zuivere wetenschap automatisch tot een betere samenleving leidt heeft men laten varen. Ook 'goede' wetenschap kan gevaarlijk blijven. Het streven naar het bewaken van de eenheid van kennis heeft men ook opgegeven. De wetenschapsgeschiedenis laat immers grote breuken in denkkaders zien, en ver uiteen gelegen vakgebieden staan voor even zover uiteenlopende perspectieven op de werkelijkheid die moeilijk op elkaar aan te sluiten zijn. In plaats van normatief is het vak haast volledig beschrijvend van aard geworden. Evaluaties van bijdragen aan wetenschap worden gezien als tijd -en plaatsgebonden en moeten als zodanig begrepen worden. Overkoepelende evaluatieve categorieën die eerder dienst deden als analytisch gereedschap zijn in onbruik geraakt. Kort gezegd is de leidende gedachte dat oordeelsvorming begripsvorming in de weg staat. In het hoofdstuk één ga ik op zoek naar de motivaties en argumenten achter deze opmerkelijke verschuiving naar dit verregaande contextualisme, dat vandaag de dag duidelijk de boventoon voert in wetenschapsgeschiedschrijving.

Veel van deze argumenten en motivaties klinken plausibel. Wellicht gaat het te ver om de eerste generatie wetenschapshistorici te beschuldigen van 'Whiggism', toch kenmerkt hun werk zich vaak toch door storend presentisme en hagiografie. Daarnaast is het beeld van wetenschapsgeschiedenis ook erg beperkt tot theorievorming en theorieopvolging (internalisme). In de afgelopen decennia zijn er enorm veel andere aspecten van wetenschaps onder de aandacht gekomen zoals materiele cultuur, experimentele praktijken, communicatie, vormen van kennisrepresentatie, locaties van onderzoek, het hele institutionaliseringsproces van het systeem van moderne wetenschappelijke disciplines, de interactie tussen wetenschap en het brede publiek, 'self-fashioning' van wetenschappers, vertrouwensrelaties en autoriteitsrelaties in het wetenschappelijke bedrijf en tenslotte de verwevenheid van politiek en wetenschap, sociale omstandigheden en wetenschap én economie en wetenschap. Dit alles heeft het beeld van wetenschap enorm verrijkt en wetenschapshistorici tonen de wetenschap nu in de volle complexiteit die haar eigen is.

Het is begrijpelijk dat een bevrijding van knellende normatieve condities nodig was om respect op te brengen voor anders denkende historisch actoren die op hun manier worstelden met problemen van natuuronderzoek. Naast de bevrijding uit knellende normatieve kaders heeft ook het streven naar zo zuiver mogelijk empirisch werken een hoge mate van contextualisme veroorzaakt. Abstracties zoals het uitoefenen van invloed mogen niet in algemeenheden blijven hangen maar concreet worden aangetoond in interactiepatronen en hun causale verbanden.

Toch moeten we ons afvragen of wetenschapshistorici niet te ver zijn doorgeschoten in het afzweren van *a priori* analytische categorieën. Aan de vraag naar hoe onze kennis over de wereld in de loop der tijd verbeterd komt wordt nauwelijks nog aandacht besteed. Dat is eigenlijk merkwaardig omdat de drang tot verbetering, of beter zijn dan concurrenten, een belangrijke drijfveer is van veel wetenschappers. Het is nobel om het verleden onbevooroordeeld tegemoet te treden om het zelf tot ons te laten spreken maar in antwoord waarop moet dat eigenlijk nog gebeuren? Het kan niet de bedoeling zijn om het verleden in zijn geheel nog eens op papier te gaan herhalen. Het doel van wetenschapsgeschiedenis is om verklaringen te geven waarom het verleden gelopen is zoals het gelopen is. Ruimdenkendheid is een groot goed maar een teveel daar aan kan ons ook met lege handen doen komen te staan. Door het verlies aan kritische functies lijkt de wetenschapsgeschiedschrijving zijn relevantie te verliezen.

Dat is precies de reden waarom een onderzoek naar wetenschapsgeschiedenis met een evaluatieve dimensie geboden is. Met een model van vooruitgang van wetenschappelijke kennis én verbetering van het wetenschappelijk bedrijf kunnen historici weer bijdragen aan de geloofwaardigheid van wetenschap. Zij kan zo ook aansprekend zijn voor huidige wetenschappers en helpen met het vergoten van inzicht in verschillen van opvatting en het plaatsen en evalueren van nieuwe ontdekkingen en ideeën (zijn die wel zo nieuw?). Sterkere selectieve criteria zijn ook nodig om een debat over de waarde van historische interpretaties goed te kunnen voeren, naast een gepaste waardering voor de verworvenheden in de geschiedenis van de wetenschap zelf. Met een evaluatieve houding kan men middels de confrontatie met het verleden zowel zaken in het verleden *alsook* in het heden bevragen en waar nodig aan de kaak stellen. Wetenschappelijke kennis van nu gebruiken in historische verklaringen is gevaarlijk maar er zijn duidelijk gevallen waarin huidige inzichten juist sleutels geven tot een beter begrip van gedrag en gemaakte keuzes in het verleden. In die zin staan oordeel en begrip niet tegenover elkaar maar vullen elkaar juist aan.

Al deze zaken geven een sterke motivatie voor het onderzoek naar de mogelijkheid de wetenschapsgeschiedenis weer uit te breiden met een evaluatieve dimensie. Die sterke motivatie is hard nodig omdat dit doel op voorhand moeilijk te bereiken is. De mogelijkheid tot het vellen van oordelen bestaat alleen bij een modus van continuïteit en contextonafhankelijkheid, terwijl wetenschapshistorici vandaag de dag juist particularisme en discontinuïteit benadrukken. Hoe dan een evaluatieve laag aan te brengen zonder daarbij geweld te doen aan al de nieuwe inzichten die de laatste decennia hebben opgeleverd aan de veelzijdige en complexe activiteit die wetenschap is, en zeker zonder terug te vallen op de oudere normatieve benadering die terecht achter ons is gelaten?

Daar komt nog bij dat de antipathie tegen normatieve geschiedschrijving gegoten kan worden in een aantal sterke filosofische argumenten die zowel de wenselijkheid als de mogelijkheid van evaluaties van kennisaanspraken uit het verleden in twijfel trekken. Ik heb deze argumenten in vijf hoofdtypen ondergebracht nl. ‘theorieafhankelijkheid’: evaluatieve standaarden zijn nooit neutraal maar tijdgebonden en afhankelijk van onze eigen conceptuele schema’s, stand van wetenschappelijke kennis en wereldbeeld, ‘presentisme’: wanneer men huidige terminologie op het verleden projecteert ontstaat er een vertekend beeld, ook is het verklaren van ontwikkelingen op basis van latere uitkomsten verwerpelijk, ‘incommensurabiliteit’: betekenisverschillen tussen conceptuele schema’s zijn niet goed te vertalen en blokkeren derhalve een betekenisvolle vergelijking tussen kennisaanspraken in verschillende historische contexten, ‘onderdeterminatie’: theoriekeuzes zijn alleen achteraf te bestempelen als rationeel en succesvol, in de tijd zelf kan een keuze op die gronden niet gerechtvaardigd zijn geweest, en het 'regels volgen' argument: de wetenschapsgeschiedenis laat geen stapsgewijze cumulatieve kennisopbouw zien, dus kunnen we ook niet uit eerdere stappen afleiden wat de juiste volgende stap zou moeten zijn. Het volgen van regels voor theoriekeuze berust op afspraak en is tijdgebonden.

In het proefschrift wordt onderzocht op welke manier deze argumenten onschadelijk te maken zijn. In de loop van deze zoektocht wordt duidelijk in welke vorm evaluatieve geschiedschrijving nog mogelijk is en wat er aan innovatieve benaderingen nodig is om dit mogelijk te maken. Deze zoektocht begint in hoofdstuk twee en drie met het bekijken van twee interpretatieve principes waarop de contextualistische geschiedschrijving voor een belangrijk deel is gestoeld, nl. het 'principle of symmetry' en het 'principle of charity'.

Het symmetrie principe werd in de jaren '70 geformuleerd door aanhangers van de 'Sociology of Scientific Knowledge' (SSK). Het zegt dat 'juiste' overtuigingen enerzijds en 'onjuiste' overtuigingen anderzijds *niet* bepaald worden door verschillende determinerende factoren. Voordien werd over het algemeen gedacht dat het erop na houden van een juiste overtuiging het gevolg was van rationeel denken en het erop na houden van een onjuiste overtuiging veroorzaakt werd door beperkende denkkaders van religieuze, sociaal-maatschappelijke of filosofische aard. Het symmetrieprincipe draagt ons op om alle overtuigingen op dezelfde manier te verklaren. Met name rationaliteit is geen speciale categorie meer. Wat geldt als rationeel of irrationeel in een bepaalde historische context wordt zelf iets dat verklaard dient te worden. Bij het postuleren van symmetrie verschuift er derhalve altijd een bepaalde factor of categorie van middel om onderzoek mee te doen naar het doel van het onderzoek zelf. Het gevolg is dat met het verder oprukken van het symmetrieprincipe, zoals in de loop der jaren is gebeurd, het analytisch gereedschap van de historicus steeds verder uitdunt.

Met een beroep op overkoepelende rationale factoren, of een meta-methodologie, kan de geschiedenis van de opeenvolging van theoriekeuzes op een formalistische manier worden geanalyseerd. Het symmetrieprincipe neemt daar afstand van en brengt er een causaal, of ook wel naturalistische, manier van verklaren voor in de plaats. Wat wij voor kennis houden is *uiteindelijk* altijd het gevolg van sociale interactieprocessen. Dit verandert de visie op de rechtvaardiging van kennis en dus op wat kennis is, nl. van een ware overtuiging naar een geautoriseerde overtuiging. Later werd dit idee uitgebreid in het posthumanisme door het symmetrieprincipe te generaliseren. Bij SSK bleven sociale factoren nog als speciale categorie over. Ook deze worden bij het posthumanisme tot onderwerp van onderzoek verklaard. Aan het interactieproces nemen nu zowel menselijke als niet-menselijke actoren deel. Natuurlijke en sociale structuren moeten gezien worden als netwerken die allemaal tegelijk ontstaan in één groot alomvattend interactieproces. Er ontstaan stabiele structuren in de loop van de tijd, maar we moeten niet vergeten dat ook deze van tijdelijke aard zullen zijn.

Posthumanisme en sociaal constructivisme worden vaak op één hoop gegooid maar dat is niet terecht. Het posthumanisme staat voor een scherpe ontologische en epistemologische wending, ook al is dat soms aan door posthumanisten geschreven wetenschapsgeschiedenis niet direct af te lezen. Interessant genoeg is er op dit terrein een duidelijke overeenkomst tussen posthumanisme en het monisme van iemand als Davidson, wiens 'principle of charity' in hoofdstuk 3 grotendeels zal worden ondersteund.

Voor evaluatieve wetenschapsgeschiedenis is het belangrijkste effect van het symmetrieprincipe dat er geen beroep gedaan kan worden op context-onafhankelijke rationele factoren en die zijn daar een *sine qua non* voor. In hoofdstuk 2 geef ik aan dat het lastig is de benaderingen gestoeld op het symmetrieprincipe op argumenten te verslaan. Wel kunnen een aantal onwenselijke gevolgen voor het schrijven van wetenschapsgeschiedenis worden aangeduid. Wetenschapsstudie wordt bijvoorbeeld volledig 'lokaal'. Het is erg moeilijk om plaats en tijd te overstijgen en te verbinden aan andere historische contexten. Een vergelijkende analyse is sowieso slecht mogelijk omdat verschillende historische contexten andere werelden zijn en je kunt geen appels met peren vergelijken. Bij SSK is ook de eis van definitieve afsluiting van wetenschappelijke controverses op elke punt van theorieontwikkeling knellend. Voorts heeft SSK moeite met het verklaren van het onafhankelijk van elkaar opkomen van dezelfde ontdekkingen en ideeën in verschillende lokale contexten. Hoe kunnen verschillende sociale en culturele factoren dezelfde uitkomsten produceren?

Het posthumanisme heeft ten dele oplossingen geboden voor deze problemen doordat het netwerkconcept niet gebonden is aan de begrenzing van specifieke locaties, de tijdelijkheid van uitkomsten van controverses wordt beklemtoond en er minder dogmatisch naar determinerende factoren in de wetenschapsgeschiedenis wordt gekeken. Toch roept het posthumanisme weer haar eigen problemen op. Zij beveelt een strategie van 'volg de actoren' aan maar het is niet duidelijk waarom we dit moeten doen. Maakt het eigenlijk nog uit tot welke uitkomsten de actoren gekomen zijn? En kan werkelijk alles in de wereld relevant zijn om de uitkomsten in de wetenschap te verklaren? Mijns inziens wreekt hier het gebrek aan selectieve criteria zich. Samen met het verbod op cognitieve factoren in explicatie van wetenschapsgeschiedenis en het probleem dat de netwerken worden zowel *explanans* als *explanandum* kunnen zijn, verliest het vak teveel houvast en dreigt het ook intellectueel steriel te worden.

Ik kom tot de conclusie dat symmetrie het best vervangen kan worden door een heterogene benadering. Deze benadering is ook relationalistisch maar niet hiërarchisch. We hoeven niet aan te nemen dat een bepaalde factor altijd de doorslag geeft in de loop van de wetenschap. Dit geeft de ruimte om rationaliteit opnieuw op te nemen als een zelfstandige determinerende factor in de wetenschapsgeschiedenis. Zoals verderop nog zal blijken is het onderscheid tussen type factor en concreet optreden daarvan in de werkelijkheid een belangrijk idee dat van de symmetristen overgenomen moet worden.

Hoofdstuk drie is gewijd aan een discussie over het 'principle of charity'. Dit is de analytische pendant van de hermeneutische traditie in de continentale wijsbegeerte en geschiedschrijving. Over het 'principle of charity' bestaat grote verwarring. Het doel ervan is de afstand tot mensen die anders denken en anders handelen (inclusief talig handelen) te overbruggen. Waar andere interpretatiemogelijkheden falen draagt dit principe ons op de overeenstemming met 'de ander' te maximaliseren. Dit gegeven kan op diverse manieren worden uitgelegd. Enerzijds wordt gedacht dat 'charity' inhoudt dat we andersdenkenden eenzelfde hoeveelheid respect moeten toebedelen als we onszelf toebedelen en we dus niet onze denkcategorieën aan hen moeten opleggen. Anderzijds wordt het principe zo begrepen dat interpretatie van andere juist noodzakelijkerwijs moet beginnen vanuit onze eigen concepten en kennis van de wereld. Dit is wel een verkapte vorm van imperialisme genoemd.

De tweede vorm is m.i. de enige juiste interpretatie van het 'principle of charity'. De eerste vorm is in feite geen toepassen van 'charity' maar van het symmetrieprincipe. Ik laat dit zien aan de hand van het voorbeeld van het gebruik van het 'principle of charity' in *Leviathan and the Air-Pump* van Shapin en Schaffer. De tweede vorm moet echter wel op de juiste manier begrepen worden. Maximalisering van overeenstemming met anderen moet alleen gezocht worden in aannames over wat te verwachten intenties en vormen van rationeel handelen en denken zouden zijn, *gegeven* de situatie waarin de ander verkeert. Het moet niet toegepast worden op de semantische inhoud van begrippen zoals Quine heeft voorgesteld. Dit leidt niet tot onwrikbaar presentisme, of 'imperialisme', omdat het maximaliseren van de overeenstemming geen eindpunt is maar een startpunt van een vergelijkend interpretatieproces.

Er kleeft een modicum van evaluativiteit aan dit startpunt wanneer het 'principle of charity' wordt toegepast op de wetenschapsgeschiedenis, omdat we van onze intenties en ideeën over de wereld uitgaan. Dit is echter nodig omdat er anders geen referentiepunt ontstaat waardoor een vruchtbaar interpretatieproces niet op gang kan komen. Het 'principle of charity' doet om die reden zijn werk veel beter dan het vergelijkbare 'principle of humanity'. Begrip krijgen van een andere tijd, of van andersdenkenden, is een kwestie van verbindingen leggen tussen conceptuele schema's. Dit begint telkens opnieuw bij ons eigen conceptuele schema. Ook al praat verleden niet terug, toch kunnen we spreken van een dialoog die na dit startpunt ontstaat. In die dialoog kan blijken dat we onze eigen concepten moeten aanpassen of dat de eerste vormen van overeenstemming beter vervangen moeten worden door het leggen van andere verbanden. Interpreteren is dus een kwestie van continu balanceren. We kunnen dit alleen accepteren als we ook accepteren dat interpretaties nooit definitief zullen zijn maar altijd, hoe klein soms ook, een open einde karakter zullen behouden

In hoofdstuk vier wordt een ander belangrijk interpetatiehandvat voor evaluatieve geschiedschrijving bekeken nl. de notie 'fout'. Ik onderzoek hoe de notie fout is geconceptualiseerd in wetenschapsstudies (waar wijsbegeerte, sociologie, antropologie en geschiedenis onder vallen). Ik kom tot een onderscheid tussen twee hoofdbenaderingen ten aanzien van fouten. De eerste is fouten te zien als obstakels van wetenschappelijke vooruitgang. In deze opvatting wordt het wetenschappelijke bedrijf primair gemotiveerd door het opruimen van verkeerd aannames, theorieën en ideeën. Wetenschap is dus in essentie een proces van foutcorrectie. In de tweede opvatting worden fouten gezien als gefaalde pogingen om kennis te worden. Wat als goed en fout wordt aangemerkt is telkens de uitkomst van een lang onderhandelingsproces waaraan diverse actoren deelnemen (welke daarin als doorslaggevend worden beschouwd hangt af van de specifieke benadering, zie daarvoor hoofdstuk twee). Wie de huidige wetenschapsgeschiedenis leest komt vooral dit laatste tegen. Het is alsof er nooit echt fouten door mensen zijn gemaakt, zij hebben alleen soms de slag verloren en dat had ook anders kunnen zijn.

De hoofdbenaderingen hebben een aantal sterke punten maar kunnen niet bevredigend zijn. Beide opvattingen schieten tekort in het vatten van de volledige reikwijdte van de interactie tussen conceptuele schema's en de experimentele praktijk. In de eerste opvatting is er te weinig aandacht voor de historische context waarin kennis geproduceerd wordt en gaat het haast uitsluitend om fouten op theorieniveau. In de tweede opvatting zijn we het idee van kwalitatieve vooruitgang op de langere termijn verloren. In beide gevallen kan er weinig aangevangen worden met fouten *in retrospectief*. De opvallende conclusie daaruit is dat de wetenschapsstudies in feite zonder een goede theorie van de notie fout zitten! Deels komt dit doordat de aandacht om positieve kennis zeker te stellen lang dominant is geweest. Deels komt dit ook doordat de verschuiving van kennis als ware overtuiging naar geautoriseerde overtuiging een geheel ander epistemologisch debat heeft geëntameerd.

Om tot een geschikte theorie over fouten te komen stel ik ten eerste voor deze onderdeel te maken van een bredere filosofie van het experiment, waarin alle fasen van experimenteel onderzoek zijn opgenomen. Ten tweede stel ik voor niet het verdrijven van fouten als de primaire stuwende kracht achter wetenschapsontwikkeling te zien maar het verdrijven van onzekerheid. Onzekerheid is een eigenschap die mensen bezitten en niet direct toepasbaar op wetenschappelijk theorieën. In de beoordeling van theoriekeuze in het verleden is het nuttig deze stap opzij te maken naar de personen die de keuzes hebben moeten maken. De mate van onzekerheid over bepaalde verklaringen, de deugdelijkheid van bewijsmateriaal, etc. is nooit abstract maar kan gegeven een bepaalde onderzoekssituatie worden uitgetekend. Historici zijn bij uitstek bedreven in het uitbeitelen van het samenspel van de voor die situatie relevante factoren. Deze mate van onzekerheid moeten we een primaire rol laten spelen in de beoordeling van keuzes die in het verleden zijn gemaakt. Deze verschuiving in perspectief heeft t.o.v. sterk op theoriegerichte benaderingen het voordeel dat er een veel bredere visie op leren kan ontstaan. Onzekerheid kan immers op meer manieren worden verminderd dan de het determinatiegehalte van een theorie, bijvoorbeeld leren als gevolg van een al dan niet gezochte confrontatie. Ik laat zien dat dit ruimte geeft aan de notie dat fouten ook vruchtbaar kunnen zijn. Daarnaast kunnen we zo het belang van de recent opgekomen analytische idee van 'going amiss', dat tevens in belangrijke mate rust op een toepassing van het 'principle of charity', op de juiste waarde schatten. Dit idee is zo belangrijk omdat er maximaal respect voor historische actoren mee kan worden opgebracht en er tevens toch, in retrospectief, over kwaliteitsverschillen van in het verleden verdedigde opvattingen kan worden gepraat.

De bevindingen, opgedaan in hoofdstuk twee, drie en vier, worden in hoofdstuk vijf bij elkaar gebracht en langs de vijf hoofdargumenten tegen evaluatieve geschiedschrijving gelegd. Het tegengif waarmee deze argumenten onschadelijk kunnen worden gemaakt komt van het idee determinerende factoren niet hiërarchisch maar relationeel te behandelen, een formulering van het concept rationaliteit zonder een scherp onderscheid te maken tussen sociale en rationele factoren, het ontwikkelen van een werkbaar diachroon perspectief op het verleden, het centraal stellen van de rol van onzekerheid als drijvende kracht achter wetenschapsontwikkeling en historische interpretatie als een voortgaande dialoog opvatten waarin ruimte bestaat voor aanpassing van onze eigen categorieën. Ook moet er plaats gemaakt worden voor het gebruik van moderne wetenschappelijke kennis in historische verklaringen wanneer we uit kunnen gaan van vergelijkbare natuurlijke fenomenen waar men toen en nu mee heeft geworsteld.

De meeste van deze zaken zijn slechts aangestipt en dienen nog verder ontwikkeld te worden. In hoofdstuk 6 en 7 gebeurt dat. In hoofdstuk 6 komen twee benaderingen aan bod die uitgaan van de naturalistische aanpak maar toch ook een evaluatieve dimensie hebben. Ik noem deze benaderingen 'extended naturalism'. De eerste is het normatief naturalisme en de tweede is de evolutionaire epistemologie. We zullen zien dat beiden uiteindelijk toch tekort schieten en dat er een alternatieve vorm van 'extended naturalism' nodig is. Deze wordt in hoofdstuk 7 gepresenteerd, uitgaande van de notie van het platform.

In hoofdstuk 5 wordt aangetoond dat de positivistische, ofwel formalistische, manier van wetenschapsstudie centrale aannames *deelt* met post-positivistische, ofwel naturalistische benadering. Deze aannames komen voort uit de positivistische wetenschapsfilosofie en zijn, ondanks alle kritiek daarop, in feite onaangeroerd gelaten. Neem bijvoorbeeld het positivistische idee dat wetenschappelijke vooruitgang gewaarborgd wordt door een context-onafhankelijke meta-methodologie. De kritiek hierop is geweest dat zo'n meta-methodologie niet gegeven kan worden *en dus* kunnen we niet goed meer praten over wetenschappelijke vooruitgang. De aanname van het verband tussen beide is daarmee dus niet verworpen maar blijft intact. Deze oppositie van 180 graden laat zien waarom het lang zo moeilijk is gebleken om een middenweg te bewandelen: zie bijvoorbeeld de grotendeels onvruchtbare 'science wars' van de jaren '90. De belangrijke winst van hoofdstuk 5 is dat het juist deze aannames zijn, die in verborgenheid nog steeds een stempel drukken op de wetenschapsstudies, die we moeten verwerpen. Het alternatief dat zich dan direct presenteert komt precies overeen met de *desiderata* die we hebben geformuleerd om de argumenten tegen evaluatieve geschiedschrijving te ontwapenen. Dit geeft een sterke bevestiging dat we op de goede weg zijn.

In het normatief naturalisme wordt een evaluatief kader gehaald uit de geschiedenis zelf door aan te nemen dat bewezen succesvolle methoden en evaluatiestandaarden algemene geldigheid hebben. Deze verzameling van methoden en standaarden is echter zelf weer empirisch controleerbaar: mocht tegenbewijs zich aandienen dan moet de verzameling worden aangepast. Laudan neemt bijvoorbeeld 'probleemoplossend vermogen' als het belangrijkste beoordelingscriterium voor wetenschappelijke theorieën. Wat een probleem is, welke oplossingen er aangedragen worden en zelfs op welke grond enkele van deze oplossingen het beste gevonden worden kan allemaal worden overgelaten aan historische contexten zelf, dwz. deze zaken kunnen op een naturalistische manier worden onderzocht. Dat is aantrekkelijk maar toch blijft het normatief naturalisme op een bepaald punt een scherpe scheiding tussen sociale en rationele factoren nodig hebben om haar normatieve kracht te kunnen waarborgen. Dit leidt dan in iets aangepaste vorm tot de in hoofdstuk vier verworpen 'sociology of error'.

In de evolutionaire epistemologie wordt er gebruik gemaakt van een analogie van het wetenschappelijke proces met het proces van evolutie. Cognitieve producten zijn de varianten waarop selectiemechanismen worden losgelaten. Deze selectiemechanismen vormen de evaluatieve dimensie van de evolutionaire benadering. Deze analogie alles is op veel manieren vorm te geven, zowel Popper's falsificationisme, als Kuhn's model van paradigmawisselingen, als het posthumanisme kunnen als een evolutionaire epistemologie worden opgevat! De formulering die het beste aansluit bij de doelstellingen van deze dissertatie vind ik in een relatief onbekend werk van Louis Boon getiteld *De List der Wetenschap*. Hij presenteert een aantrekkelijk gelaagd model waarin diverse selectiemechanismen, ieder op hun eigen terrein, werkzaam zijn. Desalniettemin blijft er een probleem bestaan met de analogie tussen het evolutie en wetenschap. Het evolutionaire proces is namelijk uni-directioneel. Dat wil zeggen dat er geen ruimte is om de wetenschapsgeschiedenis te vatten als een maaswerk van divergerende en convergerende paden. Dit is echter wel nodig om wetenschapsontwikkeling accuraat te kunnen portretteren.

De oplossing voor beide problemen zie ik in het uitbreiden van de vergelijkingshorizon. Vergelijkende wetenschapsgeschiedenis werd en wordt sterk geassocieerd met eurocentrisme en westerse dominantie en heeft mede om die reden nooit veel aanhangers gekend. Dat betekent dat er nog veel winst te behalen valt met de vergelijkende methode. Aspecten van verschillende historische perioden kunnen duidelijker worden door ze met elkaar te vergelijken en bijvoorbeeld leiden tot conceptuele verheldering. Vergelijkingen kunnen in de historische wetenschap dienen als equivalent van het empirisch testen van hypothesen. Door vergelijkbare situaties, met bijvoorbeeld andere uitkomsten, naast elkaar te leggen kunnen we beslissende factoren voor die uitkomsten op het spoor komen. En tenslotte kan middels de vergelijkende methode een modus van evaluativiteit gevonden worden die niet in strijd is met de naturalistische benadering.

In hoofdstuk 7 wordt alleen dit laatste aspect van vergelijkende wetenschapsgeschiedenis verder uitgewerkt. Als eerste formuleer ik de 'dunste' benadering die m.i. mogelijk is ten aanzien van rationaliteit. Rationaliteit vat ik op als het afwegen van theoretische deugden. Daarbij moet gedacht worden aan empirische adequaatheid, precisie, consistentie, eenvoud, heuristische waarde, reikwijdte, voorspellend vermogen, probleemoplossend vermogen, coherentie en verklarende kracht. Om een vergelijking op grond van deze deugden over langere perioden mogelijk te maken is er een onderscheid nodig tussen losse en beperkte definities op type niveau en concrete invullingen in de context waarin ze worden gebruikt ('type-occurence' onderscheid). In het typische vinden we wat tussen het universele en het particuliere in staat.

Voorts neem ik geen hiërarchie aan tussen deze theoretische deugden, afhankelijke van de situatie en van het stadium van het onderzoek kan het rationeel zijn doorslaggevend gewicht te verlenen aan meer dan één type deugd. Het is ook niet nodig dat alle deugden bij elke instantie van theoriekeuze meespelen. Tenslotte bestaat er de mogelijkheid om, mocht dit nodig blijken te zijn de lijst van deugden uit te breiden ofwel scherper te rangschikken. In de geest van het normatief naturalisme moeten alle deugden open staan voor empirische testen.

De evaluaties die op grond van deze deugden uitgevoerd kunnen worden kunnen altijd alleen maar in vergelijking tot alternatieven worden gemaakt. Absolute oordelen zijn niet te geven: we moeten een verregaande comparatieve houding aannemen.

De verzameling van deugden op type niveau is het eerste element van ons platform voor evaluatieve geschiedschrijving. In hoofdstuk 7 wordt dit nog verder uitgebreid met drie andere elementen. Ten eerste beargumenteer ik dat het vanuit het heden mogelijk is om begin –en eindpunten van onderzoeksprogramma's in het verleden te zien. Deze programma's moeten in samenhang worden bekeken. De programma's zijn op te delen in fasen met typische eigenschappen en benodigdheden. In de loop van de tijd laat een onderzoeksprogramma pas reductie zien van de onzekerheid die er aan het begin nog bestond omtrent het op te lossen wetenschappelijke probleem. Als we nu aannemen dat er alleen op het einde sterke beslissingen omtrent theoriekeuze gemaakt moeten worden dan zijn de eerdere beslissingen als zwak, en dus als niet definitief, te karakteriseren. Dit haalt m.i. de angel uit het probleem van onderdeterminatie dat de voornaamste pijler onder SSK en het posthumanisme vormt. Tevens geeft dit diachrone perspectief de ruimte om het fenomeen fout in retrospectief (bijvoorbeeld via 'going amiss') te begrijpen.

De andere twee elementen van het platform dienen met de grootste voorzichtigheid te worden toegepast. Ten eerste gaat het hier om het gebruik van anachronistische concepten in historische interpretaties. Deze zijn m.i. toegestaan mits er zoveel mogelijk aan de concrete historische omstandigheden recht gedaan is en mits zij de trigger vormen voor een interpretatiecyclus zoals verdedigd is in hoofdstuk 3. Ten tweede doel ik op het gebruik van moderne wetenschappelijke inzichten in natuurlijke fenomenen. Deze mogen ook gebruikt worden onder de aanname dat onderzoekers in het verleden met ongeveer dezelfde fenomenen worstelden. Dit is evaluatief omdat latere inzichten als beter dan eerdere worden verondersteld. Het doel daarvan is om meer en betere mogelijkheden van historische verklaring te krijgen, bijvoorbeeld over de manier waarop mensen in het duister hebben getast en welke acties ze daarbij hebben ondernomen. Grote voorzichtigheid is hier geboden: het verleden is niet een voorbereiding op het heden en het gevaar voor presentisme ligt al zeer snel op de loer. Het beste is om huidige wetenschappelijke kennis soms als zulk gereedschap dienst te laten doen in aanvulling op een reeds substantieel ontwikkelde historische interpretatie.

Hoofdstuk 7 bevat een aantal voorbeelden waarmee de werking van dit evaluatieve platform, dat uit vier elementen bestaat, werkt. Zo zijn er uiteenlopende redenen om eenvoudige theorie te prefereren boven een complexere. Ook is de ene deugd soms sterker vertegenwoordigd dan de andere. Het Darwinisme scoort bijvoorbeeld sterk op verklarende kracht maar zwak op voorspellende kracht. De astronomie is sterk in het voorspellen van posities van hemellichamen maar was lang zwak in het geven van een verklaring waarom de hemellichamen bewegen zoals ze doen, en in feite wordt de zwaartekracht ook vandaag de dag nog steeds onvoldoende begrepen. Verder laat ik theoriekeuze over langere termijn binnen uiteenlopende onderzoeksprogramma's zien zoals in de 'Devonian' controverse in de geologie in de eerste helft van de 19e eeuw (Rudwick), de strijd tussen de mobilistische en stabilistische theorie van aardmassa's in de 20e eeuw, de carrière van Heyerdahl's hypothese m.b.t. de bewoning van de Polynesische eilanden en hoe we het aanhouden van astrologische speculatie onder wetenschappers en intellectuelen tot aan het eind van de 18e eeuw kunnen begrijpen zowel als het uiteindelijke verdwijnen ervan in het wetenschappelijke discours.

De hier voorgestelde benadering is sterk pluralistisch van aard. Bijna op elke niveau (determinerende factoren, onderzoeksprogramma's en lijst van theoretische deugden) stel ik een relationisme voor met een verzameling elementen en relaties tussen die elementen. Voorkeur voor die elementen kan verschuiven, we moeten daarin simpelweg het verleden volgen. Toch blijft dit pluralisme zich afspelen binnen bepaalde parameters zoals die in het hedendaagse platform zijn neergelegd. Dit platform presenteert zich als de gulden middenweg tussen formalistische en naturalistische studie van wetenschap.

Ik ben me er terdege van bewust dat het een smal platform is en dat de normen die ermee opgelegd kunnen worden 'zacht' zijn. Toch denk ik dat het platform sterk genoeg is om een vergelijkende evaluatieve wetenschapsgeschiedenis mee te faciliteren. Of er meer systematiek aangebracht kan worden in de verzameling theoretische deugden, en of er bijvoorbeeld hiërarchische patronen zijn aan te brengen, gerelateerd aan specifieke eisen van ontwikkelingsfasen van onderzoeksprogramma's is een empirische vraag. Dat geldt ook in het algemeen voor de strategieën die ons helpen onzekerheid te verlagen. Historici kunnen hier te rade gaan bij inzichten uit de cognitieve psychologie. Meer systematische kennis over de voorkeur van theoretische deugden kan ook praktisch van nut zijn in het op de juiste manier stimuleren (inclusief allocatie van financiële middelen) van lopend en toekomstig onderzoek.

De wetenschapsgeschiedenis is gebaat bij een evaluatieve 'turn'. Maar omdat de hier voorgestelde benadering nog zo in de kinderschoenen staat, kan wetenschapshistorisch onderzoek, dat is ondernomen vanuit het hier voorgestelde perspectief, zelf ook helpen dat evaluatieve perspectief verder te ontwikkelen, en daarmee een substantieel bijdrage leveren aan de verdieping van ons begrip van het fenomeen fout en het proces van vooruitgang van wetenschappelijk kennis.

Curriculum Vitae

Bart Karstens was born in Zierikzee, the Netherlands, on March 25th 1975. After completing his secondary education at the Rijksscholengemeenschap Professor Zeeman in Zierikzee he studied Cognitive Artificial Intelligence at Utrecht University. He obtained an MA degree with the specialization 'Computational Linguistics and Logic' in 2003. Next to this he had started Bachelor studies in History, also at Utrecht University. In 2006 he switched to a two-year research master programme in History of Science, which he studied in Utrecht, Berlin and Amsterdam. In 2009 he obtained a master's degree *cum laude* with a thesis on the genesis of comparative linguistics in the 1st half of the 19th century under the aegis of Franz Bopp. From 2008 until 2012 he worked as a Ph.D. student at the University of Leiden on the NWO sponsored project 'Philosophical Foundations of the Historiography of Science' headed by dr. James W. McAllister. He was associated to the Institute of Philosophy at Leiden University as a teacher from 2012 to 2014, obtaining the BKO-certificate in 2013. Since 2014 he works as a postdoc/ research employee on the project 'Legal Structures' at the University of Amsterdam. He lives in Utrecht with his partner Cecilia Kocsis, a black dog and a black cat.