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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,

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.
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.”

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

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

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. 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

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. 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.

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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. 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. 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

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. 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.

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 \textit{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.\footnote{See chapter 4, section 2 and chapter 6.} 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
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

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. 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. 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.

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. 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.\footnote{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.}

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.\footnote{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 deserves 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.

Let’s 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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438 Daston and Galison (2007) pp.39-53 give an extensive discussion of what strengths of their study is that analysis is connected to the study of scientific use of the term in the distinction epistemic vs. theoretical virtues. One of the findings they call epistemic virtues, which should not be confused with the more formal historical virtues.

439 For Daston and Galison preferences for virtues also count as moral preferences because they express what scientists should aspire for, and hence the sense of objectivity can be connected to the promotion of the virtue of problem solving.

440 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 'fear' of doing something wrong, associated with the pursuit of a dominant virtue, also has to change. 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 deserves 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.

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. 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. 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

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’.

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. 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.

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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442 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.

443 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-empt
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.”  

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.”

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

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447 Kuhn (1977) p.330
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.

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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 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. This is an angle that has not often been used in historiography of science and can possibly do with more attention. 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.

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.” 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. 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

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

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.\footnote{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.}

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 \emph{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
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.

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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. 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 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.  

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. 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.  

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.

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

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. 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.

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.\(^\text{467}\) 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.

\(^{467}\) 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 ad hocness, 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.\textsuperscript{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.\textsuperscript{470} I believe however that the virtue approach can be used \textit{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

\textsuperscript{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.

\textsuperscript{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.”

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. 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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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

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\(^{471}\) 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. 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.

Let’s 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
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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 time 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. 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.

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

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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. 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? 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

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481 For changes in astrology caused by the scientific revolution see the excellent Von Stuckrad (2003).
problems. 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. 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.

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.

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 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, it 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.”

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

resurface. It also deals well with instances in which transfer or ideas and methods from one field to an ongoing research programme of another occur.\textsuperscript{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 \textit{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

\textsuperscript{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. 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.

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.

Let's 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].”

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

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).

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). 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.”

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 defensible. See also my review of the Dutch version of his book in Karstens (2011a).

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

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 one 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

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.”

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.