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

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# Chapter 8: An Exposition of Hermeneutic Philosophy of History of Science

## 8.1 The Problem of Understanding

The larger part of this thesis has been devoted to critical assessment of positions held by others with regard to the way in which nature plays a role in history of science. In the current chapter, I will develop my own position, which I would like to dub a 'hermeneutic' perspective on philosophy of history of science.

The crucial term, 'hermeneutic', has a notoriously complex meaning and history, and it is not because I seek to avoid doing justice to this complexity that I will begin by making a few simple statements about what I intend to convey by it. By hermeneutics I understand philosophical attempts to clarify what it is to understand something or someone.<sup>595</sup> We have seen earlier (e.g. sections 4.2.2; 7.3.3) that historical accounts of science, even if their goals are explanatory, are likely to have a hermeneutic component in this sense: there is something to be understood.

This is also the case for historiography in general, but in history of science, there seems to be something special going on. First, the people whose activities and products we seek to interpret are dealing with nature in such a way that it is impossible to interpret them without involving in our interpretation some understanding of this 'nature' with which they are dealing. This in itself is not too peculiar, since there are no human actions that we understand in isolation from our understanding of (parts of) the non-human world. In this case, however, we also need to come to terms with the fact that our understanding of the natural world is indirectly what we try to understand, and that it is itself the result of a possibly contingent historical path.

I take this to imply that in principle, our understanding of science and nature can change in the process of history of science.<sup>596</sup> The potential critical and corrective role that history of science can play with respect to science therefore does not follow from its independence of science, but is related precisely to its dependence on its history.

## 8.2 Science and Tradition

### 8.2.1 Tradition and Transcendence

In the previous chapter, we have spoken about the possibility of looking at science as a lineage rather than a class or a kind; this was one of the things we took from David Hull's evolutionary account. For historical purposes, science is identified not by a set of essential features, but by genealogical links between generations. According to Hull, as we have seen, this lineage involves an interplay between replication and interaction, which means that the

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<sup>595</sup> Cf. e.g. Bruns(1992, 1).

<sup>596</sup> Apel (1999).

lineage evolves over time. Already in the previous chapter, we compared this to the notion of a tradition, and in this section we will look deeper into what it would mean to see science in history as a tradition.

Tradition and science have been related on many occasions, and the notion of tradition has been used both in the singular and the plural, and on many scales. Alistair Crombie has talked about “styles of scientific thinking in the European tradition” – a singular tradition that starts in ancient Greece and is transmitted to early modern Europe.<sup>597</sup> Usually, identifying a Western tradition goes together with stating that this tradition instantiates a scientific rationality, thereby relating it to science as a kind again. This is the case, for instance, when Karl Popper identifies a rationalist tradition that leads from the Presocratics to present-day science, and is defined by its critical attitude rather than by its genealogy.<sup>598</sup> On the other hand, there are diachronic theories about research programmes or research traditions by, for instance, Imre Lakatos and Larry Laudan – though in Lakatos’ case, research programmes are essentially defined by their hard cores.

On both scales and both in the singular and the plural, the relation between traditionality and ‘scientificity’ is often rather complicated. What is at stake here is the question whether the scientific tradition embodies something that is in principle history-transcending. We saw this tension already when we discussed historians such as Koyré or Bernal, who oppose the weight of tradition rather sharply to what came to science from reason or nature and therefore from outside this tradition. Both find the genuinely scientific part of science to be non-traditional.

One approach that largely drops the attempt to declare science history-transcending, and in which being part of science therefore comes to be identified with membership of a tradition rather than with exhibiting universally defined qualities, is Thomas Kuhn’s work on scientific revolutions, with its focus on the paradigms of “normal-scientific traditions”. Scientists’ commitments to paradigms involves commitment to specific scientific laws, concepts and theories, and preferred types of instrumentation, as well as certain kinds of metaphysical attitudes.<sup>599</sup> It is through being initiated in a scientific paradigm, or tradition, that one can start doing scientific work and contributing to scientific progress at all.<sup>600</sup> This is the ‘essential tension’ between tradition and innovation, and it extends not just to normal science but even to revolutionary science: “work within a well-defined and deeply ingrained tradition seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved.”<sup>601</sup>

Nonetheless, at the highest level of generality, Kuhn gives a minimal definition of what it is to be a scientist, which builds on ahistorical criteria that are already much more

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<sup>597</sup> Crombie (1994, 19-30).

<sup>598</sup> Popper (1963, 120-135, 136-165).

<sup>599</sup> Kuhn (1962, e.g. 40-41).

<sup>600</sup> Kuhn (1959).

<sup>601</sup> Kuhn (1959, 234).

demanding than David Hull's – namely: trying to understand the world with increasing precision and through detailed study, and refining one's theories or techniques in the face of apparent disorder.<sup>602</sup> Normal-scientific traditions are different local and historical instances of this kind.<sup>603</sup> Moreover, science progresses partly through moments of especially strong discontinuity which are explicitly opposed to the kind of traditionality involved in normal science. The break with the past in scientific revolutions may not be absolute, but it is momentous enough for Kuhn to say that "the proponents of competing paradigms practice their trades in different worlds."<sup>604</sup>

These cautious remarks by Kuhn raise the question to what extent and in what sense 'the world' itself is subject to historical change and constituted by the results of historical scientific activity. Ian Hacking, worrying about the ambiguity of Kuhn's formulations in this respect, has proposed to resolve it by distinguishing between a "world of individuals" that does not change as a result of scientific revolutions on the one hand, and a "world of kinds" on the other – which is the world scientists actually work in and with, and which does change.<sup>605</sup> The world stays the same; it is the way we carve it up that is subject to historical difference.

To Kuhn, this feels too easy. He reminds Hacking that scientists in different paradigms do not just disagree about words, but about things as well; not just about how to classify phenomena, but also about causal expectations.<sup>606</sup> This touches upon our own considerations later in this chapter: Kuhn rightly avoids any attempt to neatly split up our world into a neutral, necessarily inter-paradigmatically stable part and a paradigm-dependent part, whether it is data versus theory or individuals versus kinds. In principle, *every* aspect of the world as known by science must be viewed as in some sense constituted by a scientific paradigm or tradition. However, there seems to be a tension between this principle and the seeming history-independence of the world. We will return to this question in section 8.5.2.

In a different way than Kuhn, Paul Feyerabend is grappling with the relationship between tradition and other organizing categories in science as well. In *Against Method*, Feyerabend speaks about science as a unitary tradition held together by rationality, albeit in a negative tone, when he says that "it is thus *possible* to create a tradition that is held together by strict rules, and that is also successful to some extent. But is it *desirable* to support such a tradition to the exclusion of everything else?"<sup>607</sup> In *Science in a Free Society*, the hierarchy between rationality and tradition seems to be reversed: there Feyerabend aims to show "that rationality is one tradition among many rather than a standard to which

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<sup>602</sup> Kuhn (1962, 42).

<sup>603</sup> See also Dear (2012a, 426-427).

<sup>604</sup> Kuhn (1962, 150); cf. also Kuhn (1962, 111-135).

<sup>605</sup> Hacking (1993, 306).

<sup>606</sup> Kuhn (1993, 319).

<sup>607</sup> Feyerabend (1975, 19).

traditions must conform".<sup>608</sup> This still contains a suggestion that rationality is something special that can be found in one specific tradition – for which reason this tradition is, according to Feyerabend, worth rebelling against.<sup>609</sup> Nonetheless, here we can see a clearly historical and 'lineage-like' account of scientific traditions.<sup>610</sup>

We have also seen Bloor, together with Barnes and Henry, invoke the notion of a local interpretive tradition that informs the interpretation of Millikan's oil-drop experiment.<sup>611</sup> As we discussed in chapter 5, they regard this tradition as providing Millikan with inherited systems of classification. They explicitly contrast this tradition-boundedness of Millikan's work with interpretations of it that put the explanatory weight upon his 'being right' or 'being rational'. What these approaches have in common is that they look upon being part of science not as a result of the following of certain specific methodological requirements or rules, but more as some sort of membership. In the case of Kuhn and Bloor, this membership is seen primarily in a sociological way: for Kuhn, the choice between paradigms is a "choice between incompatible modes of community life."<sup>612</sup>

Perhaps because of this association between the removal of the link between the scientific tradition and history-transcendence (in the form for instance of rationality) on the one hand and sociological accounts of traditionality on the other hand, such accounts are often perceived as undermining the status of the scientific tradition or of the research traditions they are dealing with, sometimes in spite of the intentions of their authors – Kuhn, of course, never meant to subvert science, and Bloor explicitly wanted his own programme to be scientific.<sup>613</sup> Nevertheless, it seems that sociological discourse about what it is to be part of a tradition is often in competition with what it is for members to be part of a tradition. We have also seen that such discourse easily turns to the suggestion that science is determined by social forces *rather than* by the things it is about – hinting that these two explanatory factors (nature and society) are in competition.

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<sup>608</sup> Feyerabend (1978, 7).

<sup>609</sup> The main criticism of Feyerabend by Chalmers (1999) is that "individuals are born into a society that pre-exists them and which, in that sense, possesses characteristics they do not choose and cannot be in a position to choose." (158). Chalmers thinks that Feyerabend needs to ignore this fact in his anarchism. That Feyerabend can also provide a somewhat more positive account of what traditions do may be seen in Feyerabend (1976, 75); in general, his singling out of science for attack seems to be not because science is a tradition, but because he interprets it as pretending to be more than that – claiming to transcend tradition through method.

<sup>610</sup> Feyerabend (1978, 17).

<sup>611</sup> Barnes (1996, 18-45).

<sup>612</sup> Kuhn (1962, 94).

<sup>613</sup> Otherwise, there would be an "irony at the very heart of our culture. [...] it would mean that science could not scientifically know itself." (Bloor 1976, 40)

### 8.2.2 A Gadamerian Account of Tradition

It is also possible to conceive of traditions in a slightly different sense, in which the competition between ‘social’ and ‘natural’ causes is less pronounced, but the other features of the abovementioned accounts of traditions remain: that they are individual lineages rather than kinds, that they provide a framework for scientific activity that does not make recourse to transcendental entities such as rationality, and that being part of a tradition consists in interacting with it rather than with sharing essential features with it.

For instance, Patrick Heelan has written about the relation between “traditions of interpretation” from a hermeneutic perspective, where Kuhn-like discontinuities of meaning can take place within a tradition without involving the destruction of old meanings.<sup>614</sup> In general, the hermeneutic philosophical tradition may have a perspective on the relation between traditionality and science on offer that overcomes a binary opposition between the two.

The perspective outlined in Hans-Georg Gadamer’s *Truth and Method* is especially worthwhile in this respect. This work is meant to liberate the humanities from a natural-scientific ideal of methodical objectivity, as well as from a subjectivist view of understanding, according to which historical interpretation simply entails reconstruction of the state of mind of the author. Understanding, says Gadamer, is “not a mysterious communion of souls, but participating in a shared meaning”.<sup>615</sup> It has an element of *Sachlichkeit*, which literally translates as *objectivity* but is explicitly distinguished from it by Gadamer;<sup>616</sup> rather than removing the subjects of understanding from the equation, it views them as being engaged with the same thing or the same question.

Understanding always takes place from within a tradition. This is an element in Gadamer’s thought that has often been considered conservative or elitist,<sup>617</sup> but in fact, Gadamer’s way of talking about tradition does not imply its identification with ‘high culture’. Rather, tradition signifies the sum of influences that the past holds over us, and that we cannot completely escape from. All understanding is historically conditioned, and historical awareness consists precisely in realizing and clarifying the working of history in ourselves.<sup>618</sup> In this sense, our finite horizon and our position in history are precisely what makes understanding possible. This is one reason why Gadamer opposes the “enlightened prejudice against prejudice”:<sup>619</sup> it is not by erasing one’s pre-understandings that communication and understanding with regard to another perspective become possible, but precisely by bringing them to the conversation. In short, historical understanding requires us to be aware of the historicity of our position, rather than neutralizing and transcending it.

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<sup>614</sup> Heelan (1997, 19-20).

<sup>615</sup> Gadamer ([1960] 1986, 297).

<sup>616</sup> Gadamer ([1960] 1986, 457).

<sup>617</sup> E.g. Jardine (1991, 76).

<sup>618</sup> Gadamer ([1960] 1986, 307).

<sup>619</sup> Gadamer ([1960] 1986, 275). See also Bernstein (1987, 13).

Another reason for this is the possibility that there may be legitimate authority in tradition. This, too, looks suspiciously complacent at first sight, but Gadamer explicitly contrasts his own account of traditionality with a romantic anti-rationalism. The point is precisely that reason does not reside outside history but only within it – in the sense just mentioned, that all rational thinking is already dependent on what is historically given to it – and that therefore the whole contrast between rationality and history is misleading. In fact, we find within the tradition in which we stand things that still speak to us, and that are still applicable.

These two elements may work to summarize our reception of Gadamer's philosophy in a hermeneutic perspective on the history of science: the notion that past and present science are *about* something, and the notion of the positive contribution of traditionality and prejudice to *understanding*, both in science itself and in historiography of science.

There are two main reasons why employing precisely Gadamer's hermeneutics in reflections upon historiography of science may seem odd. First, as Nicholas Jardine has also noted,<sup>620</sup> Gadamer's view of understanding seems opposed to the ideal of knowing the past for its own sake and on its own terms, which in turn seems to be supposed in academic historiography. There is, indeed, a tension here, though Gadamer recognizes that, for instance, a legal scholar has a different intentional relation to a past law than a legal historian.<sup>621</sup> That he seeks to get behind these differences and expose their common hermeneutical position – understanding law in history from within history – does not belie the possibility of historiography.<sup>622</sup>

Second, Gadamer on multiple occasions explicitly excludes the natural sciences from his account of understanding. In natural science, he says, the relation between scientific progress and the historical moment in which this progress took place is of secondary importance, since the scientific method makes sure that science is determined by the object of its knowledge, not by its historical conditions.<sup>623</sup> In this sense, Gadamer precisely does make the binary opposition between science and tradition that we seek to undermine here.<sup>624</sup>

However, it is more likely that Gadamer buys into a positivist myth of the objectivity of natural science because it increases the rhetorical strength of his attempt to liberate the *humanities* of an ideal of method and objectivity that, he can say, is essentially alien to them precisely because its ideal of alienation does not fit the notion of

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<sup>620</sup> Jardine (1991, 71-72).

<sup>621</sup> Gadamer ([1960] 1986, 331-332).

<sup>622</sup> In general, on the relation between the inescapability of interpretation and the possibility of truth in Gadamer, see Gadamer (1957) and Wachterhauser (1994a).

<sup>623</sup> Gadamer ([1960] 1986, 287-288).

<sup>624</sup> See also the disappointingly simplistic contrast between the role of authority in the natural sciences and the humanities in Gadamer (1953). Cf. Weinsheimer (1985, 1-33).

understanding from within history.<sup>625</sup> His intervention, we should keep in mind, is aimed not at the natural sciences but at the humanities, and writing two years before Kuhn's *Structure*, Gadamer can be forgiven for not anticipating the half century of historicizing science that came after *Truth and Method*. Moreover, he did retract his inevitabilist statements about natural science in the notes to a later edition.<sup>626</sup>

If we are contingentists about science – that is, if we believe, unlike Gadamer in the first edition of *Truth and Method*, that history matters to scientific understanding as well, because the scientific method does not progressively transcend its tradition-boundedness in a movement towards objectivity – then there is no reason why a Gadamerian hermeneutics would not be applicable to the history and historiography of science.<sup>627</sup> I have already hinted at what I believe this would entail: an insistence on both the situatedness of scientific and historical understanding, and on its *Sachlichkeit* – on the fact, that is, that historical understanding of science means understanding it to be *about* something.

In the following sections, we will zoom in a bit more on the role hermeneutical perspectives upon science have been brought to play in the philosophy of science. This serves both to illustrate what the translation of hermeneutic discourse from the humanities to the sciences (in which Gadamer himself is not of much help) involves, and to prepare our own statement with regard to the historicity of historiography of science, and the role of the world in this historiography.

### 8.3 Language and Lifeworld

Robert Crease has summarized the contribution of hermeneutical philosophy as that of “supplying the philosophical foundation for reintroducing history and culture into the philosophy of the natural sciences.”<sup>628</sup> He lists three organizing principles of a hermeneutical perspective on science, which are a good starting point for surveying the breadth and the internal tensions of this hermeneutic perspective. They are, first, the priority of meaning over technique: “science is wholly mischaracterized as solely consisting of *praxes*, of the application of techniques or calculational methods, because data, results, and laboratory events come into being by interpretation and will be mistakenly described if interpretation is poorly done.” Second, the primacy of the practical over the theoretical. “The framework of meaning in terms of which phenomena are interpreted is not comprised merely of tools, texts, and ideas, but involves a culturally and historically determined

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<sup>625</sup> E.g. Gadamer ([1960] 1986, 70-71, 479-580).

<sup>626</sup> Gadamer ([1960] 1986, 288n209). Important here is Gadamer (1986), where he explicitly acknowledges that there is a hermeneutical dimension to natural science (esp. 432-435), though concluding that an unbridgeable gap remains between the scientific and the historical world.

<sup>627</sup> Cf. Also Abadía (2011).

<sup>628</sup> Crease (1997b, 1).

engagement with the world which is prior to the subject and object separation.” Third, the priority of situation over abstract formalization.<sup>629</sup>

Let us start by noticing a certain tension between the first and the second. Whereas the ‘priority of meaning over technique’ targets practice as incomplete and requiring interpretation, the ‘primacy of the practical over the theoretical’ seems precisely to point out that meaning is subsumed wholly by the domain of practice. There is no downright contradiction here – it is conceivable and even probable that the ‘praxes’ mentioned in the first principle are construed in a different way from the ‘practical’ mentioned in the second, which turns out to be more a general ‘engagement with the world’ – but we do see a hint of a divergence here. At one extreme, hermeneutics may denote a kind of theoretical holism (we may think here of the idea that all statements, including observation statements, acquire their meaning from relations to networks of other statements), which according to Rouse points in the direction of analytic pragmatism. At the other extreme is a “Heideggerian hermeneutics of practice”.<sup>630</sup> There seems to be a difference between a hermeneutics oriented more upon linguistic structures and one oriented more upon a pre-linguistic being-in-the-world.<sup>631</sup>

Patrick Heelan has explicitly applied to science the idea that all theoretical entities are not simply theory-laden but primarily praxis-laden: what a thing is, is derived from its meaning in human life – the lifeworld or the ‘manifest image of the world’.<sup>632</sup> The role of the lifeworld is summarized by the idea that our primary relation to the world is not epistemological but ontological: we start not by knowing the world but by being in it, and knowing it makes sense only in relation to our being in the world.<sup>633</sup> This premise leads to the realization that science as much as any other human activity is connected to life. In slightly elevated language, Heelan’s contribution has been summarized as “highlight[ing] the fore-structuring of scientific phenomena in the living-worldly horizons of laboratory everydayness.”<sup>634</sup>

John Compton illustrates how we may imagine this fore-structuring. It is crucial, he says, that “we find ourselves within the natural world, we engage it in all manner of daily ways, we interact with others within it, long before we have ever heard of science.”<sup>635</sup> Compton encourages philosophy to invoke our prescientific understanding of nature, and to show how scientific concepts and practices “may be seen to *refer back* to the prescientifically

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<sup>629</sup> Crease (1997b, 4).

<sup>630</sup> On Rouse cf. Ihde (1997, 114-115).

<sup>631</sup> See also the discussion between Robert Crease and Martin Eger (1995).

<sup>632</sup> Heelan (1997, 24-26). The first term is taken from Husserl and Heidegger, the second from Sellars (1963, 1-40). For Sellars, the contrast between the manifest image, which is “the framework in terms of which man came to be aware of himself as man-in-the-world”, and the scientific image is not too radical to begin with (and Sellars himself thinks the duality can be transcended [40]); it rests mainly on the kinds of entities that both images postulate.

<sup>633</sup> Cf. Crease (2002, 39-40).

<sup>634</sup> Ginev (2002, 44-45).

<sup>635</sup> Compton (2002, 195).

known natural world.”<sup>636</sup> In the life-world, he explicates, we find an active interplay between our embodied selves and other bodily beings. This prescientific engagement underpins the criteria of explanatory validation in the sciences – principles like consistency and simplicity, Compton claims, are implicit in our perceptive and active encounters with the world.<sup>637</sup>

I feel slightly uncomfortable with the substantive theory formulated here, which suggests that criteria for scientific theory choice reflect rather straightforwardly attitudes that apparently necessarily accompany our relation to the life-world. Taken to its extreme, such a theory would undermine the sense that precise practices within science are the result of historical development (and are therefore possibly contingent) rather than natural. Moreover, it seems that what we engage with ‘pre-scientifically’ is not just the natural world, but society and culture as well; and that isolating ‘nature’ from our life-world seems like an abstraction which – however justified it might be – assumes concepts whose availability and precise meaning are themselves a product of history.

But after these critical remarks, Compton’s point that scientific activity is embedded in a lifeworld, which includes a pre-scientific engagement with natural objects, still stands:

To say that natural science ultimately refers to and coheres with pre-scientifically experienced nature is not at all to say that its theoretical models must simply duplicate the everyday world; nor is it to say that these models may not specify space-time curvatures, discontinuous trajectories, causal indeterminacies, or contain some other unusual features. It is only to say that such ‘world-variations’ must have some limits and that theoretical models must share some structures with perceived realities if they are genuinely to be taken to specify aspects, parts, or structures of the natural world.<sup>638</sup>

We may note that the primacy of the practical, in the sense of being in the world, converges rather nicely with more naturalistic perspectives, or even specifically with evolutionary perspectives, in which our knowledge, even systematic knowledge, evolves in causal contact with the world. (It also converges with the Marxist claim we saw Bernal make, that “one basis for life and another for science is *a priori* a lie”.) What the hermeneutic perspective adds is a layer of meaning; and with that, the idea that the development of knowledge is not only something that can be explained by the interactions between an individual and an environment, but something that can be understood through its relation with the life-world.

If we want to employ this ontology in a philosophy of science (or history of science), we need to ask how we conceptualize this life-world. McGuire and Tuchańska point out some different conceptualizations, by showing how Heidegger’s understanding of Dasein failed to thematize sociocultural relations ‘ontologically’; they credit Gadamer with

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<sup>636</sup> Compton (2002, 195).

<sup>637</sup> Compton (2002, 197-198).

<sup>638</sup> Compton (2002, 200).

replacing “the Heideggerian concept of being-in-the-world with the idea of our being-in-culture understood as constituted by language, tradition, and history.”<sup>639</sup>

McGuire and Tuchańska add that as far as they are concerned, we need to go beyond Gadamer’s ‘linguisticism’. It should be noted of course, that for Gadamer, having a language and having a world go hand in hand, and that to the extent that his hermeneutics is linguisticist, it is because worlds become understandable through language, not because language is content-free.<sup>640</sup> Nonetheless, the tension between perspectives focusing on a linguistic fore-structuring of understanding versus those focusing on a non-linguistic experiential fore-structuring, which we see repeated here, does not need to be resolved completely. It is important to note that under both perspectives, life-world experiences and relations can change historically. This is easier to recognize when we regard being-in-the-world as mediated by a tradition in a Gadamerian sense – for it is intuitively clear that human institutions such as language and sociocultural communities that constitute these traditions are historical – but it is no less the case for the relations between people and things, regardless of whether we regard these relations as mediated primarily by language. Importantly, there may be all kinds of feedback from science to the life-world – either through influences on language or through altering the relations between people or between people and things. It is the historical distance that results from this change that leads to a hermeneutical problem,<sup>641</sup> and thus to the possibility and necessity of historiography.

## 8.4 Traditionality, Contingency, and Nature

### 8.4.1 Circling and Dialogue

Joseph Kockelmans has looked at canonical figures in the scientific revolution, such as Kepler, Galileo and Newton. He concludes that though most of these figures have a reputation of rejecting all arguments that appeal to tradition and authority, in their research they relate their observations to “frameworks of meaning” that they have accepted independently of their scientific work, “determined in part by religious and metaphysical speculations.”<sup>642</sup> According to Kockelmans, “even though this way of thinking is scientifically unacceptable, it nonetheless shows at the same time, that the discovery of the Kepler laws was the result of a work that was inherently hermeneutic in nature.”<sup>643</sup>

This shows us something about the difficulties in isolating the innovative aspects of a self-consciously hermeneutic perspective: often, the ‘hermeneutic nature’ of scientific research is presented in direct opposition to a rather simplistic rationalistic view of science, to which the hermeneutic perspective is presented as the only alternative.<sup>644</sup> We have seen

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<sup>639</sup> McGuire and Tuchańska (2000, 72).

<sup>640</sup> Gadamer ([1960] 1986, 445).

<sup>641</sup> Cf. Gadamer (1971a, 57-58).

<sup>642</sup> Kockelmans (1997, 48).

<sup>643</sup> Kockelmans (1997, 48).

<sup>644</sup> This is true as well for McGuire and Tuchańska (2000).

that Koyré had no trouble identifying the philosophical metaphysics informing the thought of his heroes, or in showing to what extent their scientific work remained connected to traditional frameworks – only, he gave these traditional frameworks a less positive spin and talked about ‘inertia’ rather than the relating of observations to frameworks of meaning. Nor can SSK scholars be accused of thinking that scientists single-handedly construct their theories on the basis of observations, without any categories handed to them by structures preceding them. Sometimes, different alternative ways of looking at science in history are in dialogue only with one particular version of rationalism, rather than with each other.

Nonetheless, we find a different tone in Kockelmans than in SSK:

The important thing to note here is that all scientific work is done within a hermeneutic circle, which no science can ever overcome. This, however, does not mean that scientists would be unable to make true statements about what is; yet it does mean that none of these statements will ever be absolute or eternal, definitive or comprehensive.<sup>645</sup>

More than with, for instance, Bloor or Collins, we get the impression that the dialogue of scientists with the tradition that precedes them is not a deficiency; that it is not something that leaves a gap (a gap that needs to be closed by extra-scientific factors, according to Collins), but something that is connected to the very nature of science or of human activity in general.

We find this intuition voiced explicitly by Martin Eger. In 1993, Eger wrote a series of programmatic articles presenting “the case for hermeneutics in the appropriation of natural science – that is, in every kind of presentation, study, and understanding of what a particular science is saying to us.”<sup>646</sup> Eger’s argument in these articles is based among other things on supposed resemblances between developments in ‘historical-literary hermeneutics’ and philosophy of science – Eger notes that a cautious hermeneutic attitude can be harmonized with the Popperian idea of ‘prejudgment’ as a kind of probe. However, the role later thinkers ascribe to tacit preconceptions goes a bit further: “Polanyi, Kuhn, and others elevated the role of such preconceptions to a level of importance in science comparable to that given them by Heidegger in hermeneutics. On the one hand we cannot completely rise above such bias; on the other, because of its positive role, it is now clear that we cannot wish to do so.”<sup>647</sup>

The point of overlap between the development in philosophy of science and hermeneutic thought is that while in the classical understanding of the hermeneutic circle, the circling movements in the end necessarily converged towards a predetermined point, the twentieth century brought a more radical interpretation of hermeneutics: “a drastic

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<sup>645</sup> Kockelmans (1997, 55).

<sup>646</sup> Eger (1993a, 2).

<sup>647</sup> Eger (1993a, 9).

*reinterpretation of interpretation as constructivist*”.<sup>648</sup> The circle ceases to converge.<sup>649</sup>

Translating this metaphor into terms that we have discussed previously, we can say that literary hermeneutics moved from inevitabilism to contingentism: the number of possible end-points of interpretative activity was no longer one. An analogy can indeed be drawn between this and a denial of convergent realism in philosophy of science.<sup>650</sup>

Eger moves from this to the question about the role of interpretation in the understanding of science. He argues that even in the more radical hermeneutics of the twentieth century, interpretation is *not* invention – “there is something *there* to interpret”<sup>651</sup> – and that the notion of interpretation can be applied to things as well. “‘Things’ are, in this sense, not ‘dead’. They put forth, or present, or ‘have’ a meaning that is *theirs*, a part of *their being* (in relation to us).”<sup>652</sup> I am in doubt about the extent to which we ought to embrace this formulation in its far-reaching symmetry between humans and non-humans, but here it may help to think of hermeneutic interpretation in part as a ‘speaking on behalf of’.<sup>653</sup>

Eger has written lucidly as well about the difference between a hermeneutic perspective and a constructivist perspective. In part, this difference is a difference in language: the language of constructivist sociologists, Eger says, features a lot of terms such as ‘deconstruction’ or ‘un-doing’ or the political metaphor of ‘negotiation’. This is what is presented as the alternative to objectivism; it is shown, for instance, that scientists ‘sacrifice’ crucial parts of a theory in their ‘negotiations’. However, we can talk as well about scientists ‘playing’ or circling hermeneutically.<sup>654</sup> In saying this, Eger is not just putting a more positive spin and redescribing the claims that sociologists like Harry Collins make in less derogatory terms; he is challenging the idea that, since science does not conform to a naïve objectivist image, there is a deficiency that needs to be repaired by extrascientific factors. Why, Eger asks, does Collins talk about an ‘experimenter’s regress’ rather than a ‘hermeneutic circle’? It is because according to Collins such a circle is necessarily vicious, and there must be something that breaks it. However, from a hermeneutic perspective, circling is precisely what we would expect. Pointing out the feedback between scientists’ pre-understandings and their findings is not un-making science; it is illustrative of how scientific understanding properly works, namely by

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<sup>648</sup> Eger (1993a, 6).

<sup>649</sup> Eger (1993a, 9-10).

<sup>650</sup> Laudan (1981).

<sup>651</sup> Eger (1993a, 13).

<sup>652</sup> Eger (1993a, 15). Cf. Suchting (1995, 165), who suggests that Eger could be accused of animism if it were not for the scare-quotes in his exposition.

<sup>653</sup> Van der Heiden (2012).

<sup>654</sup> Eger (1997, 87-93).

precisely the dialogue between scientists and their tradition, their history. [...] I would say that the interaction between current science and its tradition should certainly be one of the great themes of a hermeneutic approach to science.<sup>655</sup>

Scientists are part of a tradition, and they understand the world proceeding from the horizon of this tradition. In this tradition, Eger distinguishes the language of the scientific discipline of which the scientist is part from the structure of the life-world. He complains about a contrast between the interpretation of the human world and that of nature, according to which the natural sciences would know only a single hermeneutic whereas scholars dealing with language would be concerned with a double hermeneutic (for example in Habermas' thought):

It is implied in all such treatments that whenever a natural scientist comes on the scene to work on a new project, he finds no pre-interpreted world, no language there already in being. What social philosophers have in mind, when contrasting natural and human sciences in this way, is an imaginary situation in which the physicist, say, always faces the phenomenon of nature *ab novo* and directly, unmediated by any symbol system other than that of the life-world. But since the seventeenth century at least such a thing has rarely happened. Of course the scientist finds a language already in being - he or she finds the language of the particular science within which the new project belongs.<sup>656</sup>

The embeddedness of the scientist in a tradition for Eger consists in her dialogue with the science already in existence. Eger is thinking here primarily of the results of scientific thought, but conceptualizations focusing more on material contexts already in existence, such as instruments and practices, are also conceivable.<sup>657</sup> This in no way – and this is also an important difference from the SSK perspectives dealt with in chapter 5 – impinges on the extent to which science engages with the world.

However, in this hermeneutic perspective the world figures not as something to which science converges (as all perspectives we saw in chapter 4 imply), but as something with which it is as if it were in dialogue. Natural objects are things the confrontation with which modifies our thoughts and actions; in that sense, they, as in the naturalist perspectives dealt with in chapter 7, play a causal role: they influence what happens in science without rendering developments in science inevitable in the sense that all possible histories of science that involve the same natural objects converge. Natural objects co-determine the history of science, but the way in which they do so depends on previous interactions between people and objects; in the language of Latour's Actor Network Theory as described in chapter 6, the way in which entities are 'added' to networks will depend on the relations already present in that network.

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<sup>655</sup> Eger (1997, 99-100).

<sup>656</sup> Eger (1993b, 306).

<sup>657</sup> Eger (1995).

### 8.4.2 Contingency and the History of Science

One potentially unsettling aspect of the tradition-boundedness of science is the possible contingency of the development of one's tradition. We have seen in section 3.4 and chapter 4 that many existing arguments in favor of presentism related to science assume that in the development of science, accidental features not pertaining to the object of research or the essence of the scientific project will be filtered out in due course; but we have also seen that, though a strong inevitabilism with regard to science is a conceivable option, it is hard to support by independent argument, and it is not the most attractive option for historians.

If we are contingentists about science to the extent that we believe that our current scientific beliefs *could* have been different, what would this entail? Richard Rorty is famous for voicing his pragmatist evaluation of contingency. He discusses

a fundamental choice which confronts the reflective mind: that between accepting the contingent character of starting points, and attempting to evade this contingency. To accept the contingency of starting-points is to accept our inheritance from, and our conversation with, our fellow-humans as our only source of guidance. To attempt to evade this contingency is to hope to become a properly-programmed machine.<sup>658</sup>

For Rorty, much like for Gadamer, our contingent historical starting points are not something to be transcended methodically, but something to be accepted or even embraced as a starting point for conversation. In 'Science and solidarity', he summarizes this as saying that we must be 'ethnocentric', by which he means "simply to work by our own lights. The defense of ethnocentrism is simply that there are no other lights to work by."<sup>659</sup> The desire for an ahistorical perspective disappears in the face of the idea that we are historical subjects engaged in conversation with other historical subjects.

Rorty's emphasis on the priority of conversation with other people, which he associates with hermeneutics,<sup>660</sup> sometimes makes him contrast the omnipresence of this conversation with the possibility of constraints posed by natural objects: "conformity to social norms is not good enough for the Platonist [who is the imagined opponent of the pragmatist in this paragraph]. He wants to be constrained not merely by the disciplines of the day, but by the ahistorical and nonhuman nature of reality itself."<sup>661</sup> Rorty's pragmatism, on the other hand, is "the doctrine that there are no constraints on inquire save conversational ones – no wholesale constraints derived from the nature of the objects, or of the mind, or of language, but only those retail constraints provided by the remarks of our fellow-inquirers."<sup>662</sup> How are we to evaluate these statements, having argued in previous

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<sup>658</sup> Rorty (1980, 726).

<sup>659</sup> Rorty (1987, 43). Cf. also Biagioli(1996, esp. 196-202).

<sup>660</sup> E.g. Rorty ([1979] 2009, 318).

<sup>661</sup> Rorty (1980, 725).

<sup>662</sup> Rorty (1980, 726).

chapters that it is very hard *not* to ascribe a role to nature – to a ‘nonhuman’ reality whose historicity may have a different character from that of humans – in science?

In fact, Rorty’s ideas do not amount to a conversation in which objects are uninvolved or completely powerless – though there is certainly no talk of ‘agency’ as we would find it in Latour or other Actor Network Theorists. Rorty’s point is only that there is nothing in natural objects that makes some state of science inevitable – we “drop the idea that inquiry is destined to converge to a single point”<sup>663</sup> – and that there are no normative reasons why it should.<sup>664</sup> There is no reason for Rorty to deny that in this hermeneuticist, conversational, and contingentist perspective on culture in general and scientific inquiry as a part of it, nature can still play a causal role<sup>665</sup> – it is just not one with only one possible outcome.

As one of only a few thinkers who have applied hermeneutical ideas to our understanding of science, Rorty also applies these insights to the role of our current understanding of the world in history of science, in a paragraph that I endorse wholeheartedly:

To say that the study of the history of science, like the study of the rest of history, must be hermeneutical, and to deny [...] that there is something extra called ‘rational reconstruction’ which can *legitimize* current scientific practice, is still not to say that the atoms, wave packages, etc., discovered by the physical scientists are creations of the human spirit. To buy in on the normal science of one’s day in constructing the largest possible story to tell about the history of the race is not [...] to say that physics is ‘objective’ in some way in which politics or poetry may not be.<sup>666</sup>

## 8.5 The Hermeneutical Position of History of Science

### 8.5.1 General Thesis

We are now prepared for some general statements about the relation between the world, science, and history of science.

The world causally plays a role in science by what it does in response to what science does in attempts to understand and explain it. What it does, then, is historically

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<sup>663</sup> Rorty (1987, 44).

<sup>664</sup> Rorty (1987, 45-46).

<sup>665</sup> See, however, Justin Cruickshank’s (2015) criticism of Rorty’s combination of a causal, ‘adaptationist’ view of the relation between (in this case) language and reality with an insistence on the self-justification of language games. According to Cruickshank, Rorty cannot but fall back into a dualist epistemology where “the conventions and rules of the community or language game become completely separated from the world beyond those rules and conventions and these conventions and rules cannot then be reunited with the world [...] One could not refute a language game by holding that it failed to help us adapt to a problem.” (81). To the extent that this criticism assumes that according to Rorty, lack of ‘adaptive’ value cannot be employed as an argument within a language game, it does not apply; to the extent that it implies that there ought to be a way in which we can invoke such an argument against our own language game from without it and ‘refute’ it, I believe it fails.

<sup>666</sup> Rorty ([1979] 2009, 345).

variable; it is not trivial that it responds to air-pumps in a different way than to particle accelerators, and that its responses are interpreted in a different way by 17<sup>th</sup>-century natural philosophers than by 21<sup>st</sup>-century particle physicists. Locally, the relation between science and nature is best conceived of as a network of human and non-human entities in a complex causal interaction (cf. Latour's Actor Network-Theory as discussed in chapter 6).

Are we to understand and explain the diachronic development of this relation, we need to see that the responses of nature to scientific practice have a differentiating effect upon the proliferation of different scientific beliefs and practices. They have so only contextually, to be sure; the meaning of the response of air-pumps to experiments is not decided only by the objects themselves, but arises through the interpretive practices of the natural philosophical community, which are rooted themselves in a specific historical condition or life-world.

Nonetheless, the behavior of air-pumps is part of the selective environment of the content of science in a certain time and place (cf. David Hull's evolutionary perspective on scientific development as discussed in chapter 7). Theories, practices, instruments, and goals may change depending crucially on the feedback that previous dealings with nature receive from its selective environment – which comprises both natural and cultural objects. Based on this, science will look different in the next generation.

This means that there is historical difference between interpretive practices, which is important to the position of history of science. First, there can in principle be no categorical answer to the question what causal role the world plays in the history of science, other than that this role is historically conditioned and that we ought therefore to reject a global inevitabilism (as in chapter 4) or a categorical exclusion of causal relevance of the world (as in chapter 5). Second, it means that understanding the historical context of science becomes essential to our understanding of the science itself; our knowledge of the world does not suffice, as Steven Weinberg believes, for the understanding of proper science. Our own interpretive practices, or our canons of rationality, do not automatically tell us how past science would have responded to nature; there is a hermeneutic distance to be bridged.

In this enterprise, our own historical situation must be taken into account. Historiography does not transcend history any more than science does. We take up our hermeneutic and explanatory challenge at a point in history where science as well as the writing of its history have had a great many iterations of interaction with their objects. This history works on us, in a way that we can clarify partly through historical study, but that we can never transcend completely. This seems, apart from the most plausible, the most internally consistent position for history of science: it would be paradoxical to maintain inevitabilism with regard to the content of historiography of science but contingentism with regard to the content of science.

We have seen that a denial of inevitabilism does not amount to a denial that science is *about* the world (or that history of science is about the history of science). In this respect, we appeal to Gadamer's distinction between objectivity and *Sachlichkeit*: while denying that

science in history can be understood solely through its object, we maintain that it is vital to understanding it that it *has* an object – the world – and that we can understand science in part by understanding it as an effort to understand this world.

That this is a diachronic effort, and that we resist inevitabilism, means that there is no reason – as the inevitabilists discussed in chapter 4 would say – to oppose the working of the world in science to its traditionality and historicity. Rather, science evolves in interaction with feedback from nature, and this means that later generations have absorbed previous interactions with nature.

This is crucial to understanding our hermeneutic position: it means that the tradition in which we stand, the way in which history has determined our condition, is not ‘pure culture’ as *opposed* to pure nature; it is not the product of free-floating human social interaction and cultural creativity, but the result of a long series of causal interactions between humans and nature. The influence of the world is causally integrated in the history of science; and precisely because the manner of its integration depends, at every moment, on a precise constellation of human and non-human factors (a fact that does not in any way negate the ascription of universal and timeless regularities to nature, or to human culture for that matter), its cumulative effect cannot be disentangled from this history.

Trying to disentangle it nonetheless – for instance, notably, in the interest of creating a historiography that is independent of the science whose history it seeks to describe – would mean undoing in thought the entire chain of events leading from the past we want to describe up the present from which we set out to understand it. But this rests on a naïve ideal of historical understanding, and on precisely the same dehistoricized ideal of objectivity that such an endeavor would need to deny to the natural sciences.

Rather, we proceed from the Gadamerian idea that understanding in history is more closely related to translation and application than to reconstruction. Saying that historiography is historical in this sense does not deny that it is meaningful for it to try and say true things about the past – any more than saying that science is historical means denying that it is meaningful for it to try and say true things about the natural world. On the contrary, we understand both activities as being about something, and this is precisely the role that their objects play in them. This summarizes the perspective outlined in this thesis: the world as we understand it is what we understand science as being about.

### 8.5.2 The Limits of Historicity

One puzzling aspect to this thesis is that by historicizing science and its historiography so thoroughly, it may seem that there we have made it impossible to talk about nature itself as something outside history. To what extent are we actually subsuming nature under history here?

As Hooykaas was aware, both human and natural history are unique and happen only once. In this sense, nature clearly is itself historical. However, this does not mean that we cannot ascribe law-like regularities to what happens in human history or in nature, nor

that we cannot, once we have identified them, project these regularities back upon the past – assuming that they were as true then as they are now.

For instance, when we interpret what early modern thinkers had to say about the solar system, we imagine they were referring to (roughly) the *same* solar system that we inhabit; we also ascribe to this solar system and its workings a large amount of independence of what either early modern natural philosophers or our own astronomers have to say about this solar system. In this sense the solar system is ahistorical: it is something in *nature* that we need, nonetheless, in order to understand 16<sup>th</sup>-century pamphlets about comets and debates about Copernicanism. We cannot, in this sense, reduce nature to history: if we say that the solar system we know is itself a product of human history in the sense that it did not exist in the 16<sup>th</sup> century, we are removing important handles we have on understanding what was going on in the 16<sup>th</sup> century. We would still have the historical sources about the 16<sup>th</sup> century, to be sure, but these sources will significantly underdetermine any claims about what the universe was like in those days.

On the other hand, there is also no escaping the fact that when we talk about our solar system or take our knowledge of it for granted when studying early modern debates, we do not have access to this solar system independently of the possibly historically contingent knowledge that is available to our culture about this solar system. This is not restricted just to its accidental features (like the precise size of the planets, the number of their moons or their orbital velocities), but it also concerns its existence and its being a solar system.

This does not mean that the distinction between ‘what we believe about the solar system’ and ‘what is actually the case about the solar system’ collapses. With respect to any culture, we can see that there is a difference in meaning between ‘what this culture believes about the solar system’ and ‘what is actually the case about the solar system’. There is no reason why these two statements should become identical once we refer to our own culture. The only peculiar thing when it comes to beliefs that *we* hold about the solar system is that it would be paradoxical to say that we believe what is actually the case about the solar system to be something different from what we believe about the solar system (whereas it is not paradoxical to say that we believe what is actually the case about the solar system to be something different from what *others* believe about the solar system).

We understand the difference between belief and truth and we can grasp, in this abstract sense, the possibility that our beliefs are not true. Or to rephrase this without using the word ‘truth’: we can understand that the solar system is something different from beliefs of others about the solar system, and in this abstract sense we can understand that the solar system is something different from our own beliefs about it. We also understand, in this same abstract sense, the possibility that we *could have held* other beliefs and that these beliefs could have had certain epistemic virtues to the same, or even to a higher degree than our actual beliefs. (There is a structural similarity between our historical contingentism and our belief in the history-independence of the natural world, in that both historically possible

alternatives to our own beliefs and the real world which our beliefs seek to trace are inaccessible to us.) We cannot, however, be completely certain and in total agreement about the superiority of a certain belief to our own, and still not assent to this other belief.

In general, we cannot provide content to the notion of 'a belief that *we* could equally well have held' without this content immediately becoming an actual competitor to our currently held beliefs. If we say that a non-Darwinistic evolutionary biology could have developed, we cannot substantiate this claim by providing an actual non-Darwinistic evolutionary biology, since such a biology, if it were as plausible *to us* as a Darwinistic evolutionary biology, would function only as an actual competitor to Darwinistic evolutionary biology in the present.

It is possible to claim that there is an alternative to our culture in which a Darwinistic evolutionary biology would never have developed, or would have had lost the competition to a non-Darwinistic competitor, and that it was historically possible for previous stages of our own culture to develop into such an alternative; and that in this sense, 'we' (in a rather loose sense of the first person plural) could have held a non-Darwinistic evolutionary biology. In this case, however, we are speaking not of specific possible alternatives to scientific theories or schools *within* our current culture, but of possible alternatives *to* our current culture.

To summarize: there are things about which we believe that their existence is independent of human history, but about which we also believe that there are beliefs about them that are impossible for us to hold, while historically possible alternatives to our culture could have held them.

### 8.5.3 An Illustrative Example

We will evaluate some illustrative examples from recent historiography later, but at this stage an idealized example may be clearer. How does a philosophy of history of science proceeding from these premises deal with the interpretation of past science and past scientific debates, e.g. the controversy between vacuism and plenism?

Air-pumps and vacuums are a well-known example because of Shapin and Schaffer's *Leviathan and the air-pump*.<sup>667</sup> The following is a modelling, a simplification that does not do justice to the complexity of either historical reality or of Shapin and Schaffer's account. The simplification is intended to make visible to what extent our own beliefs about nature and our beliefs concerning the historical contingency of those beliefs matter to our historical explanations.

My claims are the following.

First, 'truth' as such is not a historical explanation: even though we would summarize our own beliefs in the proposition that a vacuum is possible, the victory of the

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<sup>667</sup> Shapin and Schaffer (1985).

vacuists over the plenists was not because the vacuists held beliefs to which we would also assent. Our beliefs do not causally explain 17<sup>th</sup>-century beliefs.

Second, we need to be aware that between the vacuists and the plenists, neither represents '21<sup>st</sup>-century science as it is in the actual world' or its opposite – no more than any party in the 16<sup>th</sup>-century Reformation represented 'Western modernity' or its opposite. When we are interpreting scientific cultures that are genuinely in the past, they are different; they are primarily other cultures, the understanding of whose categories and beliefs requires translation.

Third, there may still be a significant historical connection to our own culture: simplifying, the vacuists may have 'won' (just like the Reformers 'won' in certain parts of Europe) and our own scientific culture may be the result of a series of historical transformations that could take place only (or had a better chance of taking place) in an environment in which the vacuists won, rather than one in which the plenists won. In that case, in explaining who 'won', we are still – albeit partially and indirectly – explaining our own beliefs.

Fourth, it may be that we believe the vacuists won partly because of the rhetorical significance, in the specific cultural circumstances (in which, for instance, specific work had to be done to develop and maintain the rhetorical force of witness accounts of certain events)<sup>668</sup> of specific outcomes of trials in air-pumps. That there were air-pumps at all, and that trials held with the aid of these pumps bore this significance is a fact that is itself in need of historical explanation. This explanation proceeds in part through an understanding of contemporary beliefs; in Nicholas Jardine's terms, we need to render the questions asked to nature, and the local reality to which they are connected, intelligible to our 'scene of interpretation'.<sup>669</sup> This requires something of a fusion of horizons; in particular, it requires showing how doing an experiment with air-pumps could be a meaningful way of posing a question to nature.

Fifth, the actual outcomes of these trials may have been important here, and given our own beliefs about air-pumps, we believe that they will have behaved in a certain way. For example, we believe that birds would indeed have died when the air was removed from a vessel. (Incidentally, we ourselves may find this behavior to be better explicable by vacuist-like theories than by plenist-like theories; but our own explanations of why the birds died may differ from those of both the vacuists and the plenists in the 17<sup>th</sup> century.)

Sixth, how the resulting observations influenced the fate of different scientific theories may not be immediately understandable except through familiarity with the life-worlds of the natural philosophical community. As in step four, we need to explain what happens to scientific beliefs, practices, instruments, goals, *etc.* in part by understanding the meaning of the resulting findings. Again, this requires an awareness of continuities and

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<sup>668</sup> Shapin (1984).

<sup>669</sup> Jardine (1991, 69).

discontinuities between the respective worlds of the natural philosophers we study and ourselves.

For though nature may come into the picture explicitly only when it is asked an explicit question through an experiment, science's being-about-nature is implicitly supposed throughout: after all, that the experiment happens, and how its results are interpreted, depend not just on individual human fancy or the cultural accumulation thereof, but on an accumulation of earlier *interactions* between nature and cognitive and practical dealings with nature. For instance, the behavior of air-pumps resonates with ancient metaphysical debates about the possibility of a vacuum that have acquired a new meaning with Descartes' mechanistic philosophy; but these natural philosophical debates themselves only make sense as attempts to deal cognitively with the world we live in.

Of course, one metaphysical system may truthfully be said to be more 'speculative' than the other; but speculative natural philosophy does not take place – pardon the pun – in an experiential vacuum, and in this sense, Boyle's air-pump trials stand in, and respond to, a tradition that concerns a world that we live in, too. 17<sup>th</sup>-century natural philosophy tries to make sense of rainbows, human memory, the movements of the planets, magnetic attraction, the circulation of the blood – and all these phenomena are presupposed, both in the formulation of new questions to nature, and in the interpretation of the answers. We can understand these questions and interpret the answers only because we are aware of these phenomena. We need to realize that when Boyle is talking about the spring of the air, he is not uttering a 21<sup>st</sup>-century scientific truth, but he *is* talking about the same world that we are familiar with, and the fact that he does so is vital to the possibility to render his doings – as well as their impact, which requires an understanding of his place in the history of science – explicable at all.

## 8.6 Four Possible Objections

### 8.6.1 Introduction

I hope to have presented this account in such a way that it seems like a simple matter of common sense: we already believe certain things about nature and science, and when we study their history, we naturally bring those beliefs with us, there being no compelling reason why we should categorically subject these beliefs to a different treatment from any other beliefs that we hold. All of our suppositions about continuities and discontinuities between the past we study and the present we inhabit are relevant to our interpretation of this past and of our position in history; suppositions that pertain to the world that science is about are not excluded from this, and are indeed all the more vital to our interpretive efforts. Indeed, as far as I am concerned, there is nothing drastic about my proposal, and I believe that it fits better with what historians of science already do than most of the more radical approaches advocated by SSK and ANT.

Nonetheless, I do not think there are no interesting or plausible objections to my account, inspired by SSK or ANT or by more general considerations. I will present the

objections I ‘fear’ the most here – as uttered by hypothetical critics – and then deal with them more extensively one by one.

- 1) The paradoxical nature of saying that the truth about something in nature (e.g. the solar system) is something different from what ‘we’ believe about nature does not arise for me when I don’t identify with what my culture believes about nature. I can set out to explain what science believes about the solar system in a mode where I talk about this science – about what you call ‘our’ scientific culture – only in the third person.
- 2) This relates to a broader objection: that your whole approach seems to imply a rather slavish following of the authority of the scientific tradition and of a scientific culture that you, apparently, see as a unity. This is a general problem with Gadamerian hermeneutics: that it fails to thematize the tensions and contradictions inherent in any tradition, overlooking these because it needs to ascribe to this tradition an amount of unity that it does not, in fact, possess. You fail to be *critical*, for the simple reason that the way you construct and conceptualize ‘the scientific tradition’ will render you incapable of making claims that significantly challenge the epistemic authority of this tradition.
- 3) This also means that you could have spared yourself the complicated 6-step account just now, by admitting in step 1 that you *do* in fact mean that we hold our current opinions because they are the best – and that in any controversy that influenced the current situation, the best party won. Yes, you admit that things could have gone differently, but this denial of inevitabilism will not make your position any less Whiggish. If our current views about what nature is like decide what we believe nature contributed to the resolution of a controversy at a crucial point, this contribution will, unsurprisingly, turn out to be in favor of the ‘side’ that won. A synoptic explanation of any scientific development in your approach will always be: “because this was right/true/etc.” This is simply a very careful step *back* to the good old triumphalist historiography.
- 4) Apart from uncritical and triumphalist, your history of science is also impotent and irrelevant: it can never find anything that significantly changes how we look at past or current science, because it is decided in advance that any finding will conform roughly to the existing interpretative structure, *and* that history of science, in your view, is simply the handmaiden of science. This severely limits not just the plausibility that you will see something new, but also the force that any historical finding can exert upon our notion of natural science: just like medieval philosophy would never draw conclusions that challenged Christian theology, and *if* it did that, theology would still trump philosophy, so your history of science will never find anything that challenges science, and *if* it did, then the ‘authority of the scientific tradition’ would still trump this finding.

### 8.6.2 Playing the Stranger

My imaginary critic objected that:

The paradoxical nature of saying that the truth about something in nature (e.g. the solar system) is something different from what 'we' believe about nature does not arise for me when I don't identify with what my culture believes about nature. I can set out to explain what science believes about the solar system in a mode where I talk about this science – about what you call 'our' scientific culture – only in the third person.

Obviously, it is possible in some sense to talk about your own culture as if it were another culture, to 'play the stranger' as Shapin and Schaffer have called it. But Shapin and Schaffer understood very well that it is not possible to *be* this stranger.<sup>670</sup> There is an inescapability to our historical standpoint that cannot be wished away.

Of course, there are cultures about which we can talk in the third person, because they are not ours. It is important to remember, however, the possible arguments for omitting or 'relativizing away' our own knowledge of nature as dealt with in chapter 5. We dealt there with almost all reasons not to involve nature in our explanations of past science. The only one that we did not dismiss at that stage was the argument of circularity: the intuition that, if we use our own beliefs in the explanation of our own belief, there must be some sort of vicious circularity. The hermeneutic perspective outlined in this chapter does deal with this objection, by denying both the viciousness of this circularity and its escapability. Both in the case where we are studying another culture and in the case where we are studying our own (or its history), there is no reason why we should artificially suppress our own knowledge of the world, to the extent that our reason for doing so was the supposed circularity that would arise in this way.

It remains possible, however, that a scholar simply disagrees with a scientific consensus: it is conceivable that the weight of scientific dealings with geology in the past century is very much in favor of the theory of continental drift, but that you, for whatever reasons, stand outside this consensus. What would that entail for your historiography? In that case, when you study the history of the debate about continental drift, your hermeneutic point of departure will be slightly different from that of the other historians. Your prejudgments about the truth of continental drift, however, are only one aspect of your many beliefs about what the world looks like, many of which you will still share with your peers.

Your conversation with your fellow historians would become difficult only where your heterodox ideas about continental drift influence your opinions about what the scientists you are studying could potentially have encountered in their research. In this case, you may still agree with many of your orthodox colleagues on the nature of the available evidence during a given episode. You may even agree with them that given the available

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<sup>670</sup> Shapin and Schaffer (1985). Cf. also Shapin (1992, 357-360).

evidence at a certain time, the development of a scientific consensus about continental drift is understandable and explicable (even if it does not convince you). There is no reason why your difference of opinion with your colleagues about the truth of the scientific theory whose history you are writing would matter *more* to the historiographical debate than other possible differences, for instance about the precise nature of power relations in the relevant scientific networks, or the relevant cultural context of the debate.

The difference between you and your colleagues would come to weigh more heavily if your scientific opinions about the current scientific consensus were more unorthodox, and came to influence more your particular opinions about what past scientists could have seen and done. If you believe that the earth is flat and set out to write a history of the science of geology consistent with that opinion, it will turn out quickly that neither you nor your colleagues are methodological relativists about this theory.

### 8.6.3 Following a Tradition

The next question is whether these reflections on the inescapability of prejudice and tradition-boundedness imply an unquestioning acceptance of the authority of some scientific tradition. My imaginary opponent voiced this as his second objection:

Your whole approach seems to imply a rather slavish following of the authority of the scientific tradition and of a scientific culture that you, apparently, see as a unity. This is a general problem with Gadamerian hermeneutics: that it fails to thematize the tensions and contradictions inherent in any tradition, overlooking these because it needs to ascribe to this tradition an amount of unity that it does not, in fact, possess. You fail to be *critical*, for the simple reason that the way you construct and conceptualize ‘the scientific tradition’ will render you incapable of making claims that significantly challenge the epistemic authority of this tradition.

Indeed, the notion of traditionality seems to have overtones of complacency, and an attitude that embraces this ‘tradition’ seems much less exciting than an attitude which shows how it is an imagined tradition, resulting from selective canonizations of the past. Science is not simply one tradition.<sup>671</sup> Terry Eagleton has objected to Gadamer that it assumes the existence of a single ‘mainstream’ tradition which is to be cherished, making history into a ‘club of the like-minded’: “tradition holds an authority to which we must submit: there is little possibility of critically challenging that authority, and no speculation that its influence may be anything but benevolent.”<sup>672</sup>

Luckily, this is a wholly unfair characterization of Gadamer’s project as well as of the current thesis. Certainly, genuine criticism of the tradition we find ourselves in is possible, but such criticism precisely requires elucidation of our historical condition. What it

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<sup>671</sup> Cf. Jonker (2011); Feyerabend (1978, 33).

<sup>672</sup> Eagleton (2008, 63).

means to criticize the tradition to which one's understanding is simultaneously bound,<sup>673</sup> as it happens, was also the subject of an exchange between Gadamer and Habermas in a volume about the relation between *Hermeneutik und Ideologiekritik*, which I will review in the remainder of this section, to show how Gadamer dealt with Habermas' demand for a more radical criticism than Gadamerian hermeneutics seemed to permit.<sup>674</sup>

Habermas' point is that structures of prejudice made transparent cannot continue to function as prejudice as before; reflection could lead to rejecting the claims of traditions.<sup>675</sup> Consciousness, though situated within the contingent structure of traditions, should not let itself be reduced to 'sublimating' all social processes into cultural tradition.<sup>676</sup> After all, there are social processes that are not embedded in normative structures but that do co-determine the shape of traditions; language as tradition also becomes a medium of power and violence.<sup>677</sup> It is not enough to understand everything as covered by tradition, then.<sup>678</sup>

I can imagine my imaginary interlocutor saying something like this when I, in appealing to the existence of a horizon of interpretation inescapably shaped by the scientific tradition, seem to absolutize this tradition: that in the history of science there are power relations at work that have left us with a discourse about science that is in some sense tilted; and that the exposure of the processes behind this could work in an emancipatory way, by unhinging the apparent legitimacy of the tradition from which our understanding indeed first proceeded – for Habermas does not deny the situatedness and fore-structuring of understanding by tradition. Gadamer, however, responds by, first, pointing out the artificiality with which Habermas needs to construct the faculty of reflection.

What is the relation of historical [*wirkungsgeschichtliche*] reflection to the tradition of which it becomes aware? My thesis, which I think is the necessary conclusion of recognition of our historical determinedness and our finitude as taught to us by hermeneutics, is [that we need] to see through this opposition between living, 'spontaneous' tradition and reflective appropriation, and see that it is a dogmatic one.<sup>679</sup>

Also misleading, according to Gadamer, is Habermas' drawing an opposition between the realm of cultural tradition and the other 'determinants' of social reality. Yes, his own *Truth and Method* might itself have been rather silent about these matters, but all things considered it would be an absurdity if, among the prejudices upon which hermeneutic philosophy urges us to reflect, it would not involve those related to labor or power as well.<sup>680</sup> It is not

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<sup>673</sup> See also the thoughtful and balanced discussion on criticism and tradition in Gadamer's thought by Warnke (2012, 14-20).

<sup>674</sup> Cf. also Collin (2015, 56).

<sup>675</sup> Habermas (1971a, 49).

<sup>676</sup> Habermas (1971a, 54-55).

<sup>677</sup> Habermas (1971a, 52).

<sup>678</sup> Habermas (1971a, 55).

<sup>679</sup> Gadamer (1971a, 68).

<sup>680</sup> Gadamer (1971a, 70-71).

the case, then, that it ‘absolutizes’ cultural tradition; it only wants to understand anything that can be understood. This is the sense in which language is the key: “Being that can be understood is language.”<sup>681</sup>

Reflection, Gadamer says, does not necessarily dissolve the structures behind authority, since these structures can also be consciously accepted after reflection. Gadamer recognizes that tradition is not self-legitimizing, and that reflection can challenge it; but not all at once. Whoever thinks that this is possible, “I confront with the finitude of human existence and the essential particularity of reflection.”<sup>682</sup> Reflection, for Gadamer, brings ‘before us’ what would otherwise have happened behind our backs, and in this way it indeed allows us to judge our own pre-understandings – but it cannot bring everything before us.

Habermas’ counter-reply outlines a distinction between communication and pseudo-communication, the latter of which is systematically distorted, and he reiterates the point that the background consensus of tradition can be a result of pseudo-communication.<sup>683</sup> More and more, science gets to play a role in transcending the circumstances of the dialogue; and this leads Gadamer to remind Habermas that, as far as he is concerned, philosophical hermeneutics has little to do with method. It does not try to elevate understanding to a discipline; *Verstehen* is the completed form of human society, the ‘Gesprächsgemeinschaft.’ It is interesting that he here explicitly includes science within this *Gesprächsgemeinschaft*.<sup>684</sup>

At this point, anyway, it becomes clear that asserting the impossibility of transcending ‘tradition’ does not imply reifying or absolutizing this tradition, for the very reason that identifying the unifying structure of this tradition and conceptualizing (let alone stating prescriptively) the essence of its hold over us would imply having understood it in its entirety already.

This also means that Eagleton has overestimated the amount of complacency that necessarily follows from recognizing that the weight of the past is bigger than we are:<sup>685</sup> yes, it is *hubris* to think that *if only* we find the right methodical way to look at the tradition we are in, then we can transcend all distorting contingencies. (In the case of history of science, this is not only hubris but also paradoxical: if method transcends tradition, why does history of science matter?) But on the other hand, any aspect of the tradition can be criticized, and there is nothing we are not *allowed* to do with it.

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<sup>681</sup> Gadamer (1971a, 71).

<sup>682</sup> Gadamer (1971a, 74).

<sup>683</sup> Habermas (1971b, 154, 158).

<sup>684</sup> Gadamer (1971b, 289-292).

<sup>685</sup> Cf. Bruns (1992, 195-212).

#### 8.6.4 Neo-Whiggism

My hypothetical critic also accused me of indirect Whiggism:

You could have spared yourself the complicated 6-step account just now, by admitting in step 1 that you do in fact mean that we hold our current opinions because they are the best – and that in any controversy that influenced the current situation, the best party won. Yes, you admit that things could have gone differently, but this denial of inevitabilism will not make your position any less Whiggish. If our current views about what nature is like decide what we believe nature contributed to the resolution of a controversy at a crucial point, this contribution will, unsurprisingly, turn out to be in favor of the ‘side’ that won. A synoptic explanation of any scientific development in your approach will always be: “because this was right/true/etc.” This is simply a very careful step back to the good old triumphalist historiography.

The point here is that the presentism I allow myself with regard to current scientific beliefs will tend to some sort of inevitabilism in the end. If this presentism can, as was suggested in chapter 3, be countered only by demonstrating that causal anachronisms occur, our current scientific knowledge is at a relative advantage, since it is allowed to inform our causal beliefs.

So, even by provisionally and corrigibly assuming the truth, validity or applicability of our own scientific categories and beliefs, an insurmountable asymmetry would arise in our view of a past controversy. There may be controversies or other kinds of uncertain episodes in past science, of which the following seems to be the case:

- 1) It was historically possible for them to be resolved in a different way.
- 2) If they had been resolved in a different way, we would have believed something different about nature now.
- 3) If we had believed something different about nature now, nature would have played a different role in our current explanations of the resolutions of past controversies.
- 4) Therefore, by letting ‘nature as we now believe it is’ play its role in our explanation of the resolutions of past controversies, the actual history in which we come to believe that nature is as we now believe it is has an ‘unfair’ advantage over other possible histories. This imbalance remains true even if we recognize that we hold our own beliefs only contingently and fallibly.

I recognize that points 1 to 3 are true. I believe they fail to support an argument against my thesis, because it does not follow from these points that our actual history has an advantage over other possible histories, and even if it did, this would not undermine my thesis. It would not, for the following three reasons:

- A) Singling out our beliefs about nature as making our accounts of past science imbalanced is arbitrary: we have beliefs about how society and culture function as well, and they co-determine what we believe about the ways in which past

scientific controversies could be resolved. If we believe that the history of science is causally integrated with that of societies and cultures, we ought to believe that in many cases, a historically possible alternative to our scientific beliefs would not leave our beliefs about society and culture undisturbed either. We could as well say that in letting 'social interactions as we now believe they work' play their role in our explanation of the resolutions of past controversies, we give other possible histories an unfair disadvantage in our accounts.

- B) The only thing that distinguishes 'nature' clearly from any other factors here is that it happens to be what we now consider the object of research of scientists. But the demarcation between the natural and the social may itself be a historical contingency; at least, what scientists are and what their research object is, is itself constructed differently throughout history. It is not clear why there would be a necessary link between 'being an entity that current scientists investigate' and 'being an entity the including of which in a historical account tilts the history towards its present state'. For instance, the behavior of air-pumps would presently fall primarily under the competence of scientists, but that does not mean that beliefs about the behavior of air-pumps in the 17<sup>th</sup> century tilt our historical account more towards the present state – or even towards present beliefs about the behavior of air-pumps! – than do current beliefs about social power in the 17<sup>th</sup> century. (Ironically, this is especially the case if we believe that social factors are causally more important in the resolution of scientific controversies than input from nature.)
- C) The argument that under the current approach our actual history attains an 'unfair' advantage over other possible histories suggests that this advantage could have been avoided by another approach. However, we do not have an independent overview of the outcomes of other possible histories; there is no way in which we can know precisely what we would have believed if other parties to a controversy had won. It is impossible for us to average out the different beliefs that we would have held as the result of the different histories that are possible subsequent to the episode we are studying.

Behind the accusation of Whiggism lies a premise similar to the one we saw Ashplant and Wilson assume in chapter 3 in their argument against Butterfield: that presentist categories tend to survive the confrontation with historical sources.

However, it is essential to the perspective outlined in this chapter that the confrontation with historical sources can in principle modify our categories and our beliefs. Within one historical study, this effect may be small, since one study will usually have limited weight relative to that of the entirety of the traditions upon which it reflects; but it is very well conceivable that the collective result of history of science modifies our understanding of important scientific categories, and thereby indirectly our understanding of the world.

In any case, as said, there is no good reason why our understanding of how nature would have behaved will lead to us overestimating the chances of precisely that side in a controversy whose opinions about nature best match how we think it would have behaved. Drawing that conclusion, that applying our own categories and beliefs about nature to past controversies inevitably leads us to side with the party who resembles us most, would be misleading in two ways.

- D) First, it would be failing to take into account the historical discontinuity between our categories and those in the past: it is not a good summary, when the vacuists win a controversy in the 17<sup>th</sup> century, to say that the party ‘resembling’ us the most wins that controversy. The scientific tradition has undergone too many transformations after the vacuists-plenists controversy simply to say we agree with Boyle’s ‘vacuism’ – or with the whole of his experimental philosophy, for that matter.
- E) Second, even if we ‘side’ with a particular party in the sense that in some relevant aspects we believe it to be relatively close to our own perspective, this does not mean that we should be less able to explain why in a particular context such a perspective would be relatively weak. It is perfectly well possible for convinced atheists not to remain puzzled by the question of why atheism is a minority position throughout most of history. Similarly, it is conceivable that we agree more with Galileo than with the Pope about the relative accuracy of the Ptolemaic and the Copernican systems, but that we can still fully understand why in a particular controversy Galileo’s arguments would fail against those of the Pope – fail, not only because of factors ‘extrinsic’ to the debate (such as the coercive powers of the Church), but also because of the intellectual context; because what was at stake was not just the question of heliocentricity or geocentricity, but the question of how to do natural philosophy, and in connection with this, the question of how to do theology, and ethics – at stake were, loosely speaking, different paradigms, neither of which we completely identify with. Once we realize this, the fact that we happen to believe, with Galileo, that it is more accurate to say that the earth goes round the sun than that the sun goes round the earth does not significantly distort our view of the controversy.

#### 8.6.5 Criticism and Relevance: Some Historiographical Examples

Finally, my hypothetical opponent said that:

your history of science is also impotent and irrelevant: it can never find anything that significantly changes how we look at past or current science, because it is decided in advance that any finding will conform roughly to the existing interpretative structure, *and* that history of science, in your view, is simply the handmaiden of science. This severely limits not just the plausibility that you will see something new, but also the force that any historical finding can exert upon our notion of natural science: just like medieval philosophy would never draw

conclusions that challenged Christian theology, and *if* it would do that, theology would still trump philosophy, so your history of science will never find anything that challenges science, and *if* it would, then the ‘authority of the scientific tradition’ still trumps this finding

I have emphasized in the previous section that it is essential to my argument that the discipline of history *can* modify our beliefs and categories; but I have also said that probably, the weight of an individual historical study will be less than that of the entire scientific tradition. This makes the objection understandable that science always seems to trump history. Would our study of a specific episode in the history of science ever lead us to revise our scientific theories? If not, it seems that scientific knowledge can inform our historical accounts, but historical knowledge cannot inform our scientific accounts; that would imply a hierarchical ordering of knowledge.

My answer is that in principle it is possible for historical knowledge to modify scientific knowledge or the status of this scientific knowledge. The likelihood that a piece of information about the past will modify a piece of information in science is not determined by an *a priori* hierarchy between history and science or an inevitably one-way direction of influence, but by the relative weight of the considerations already in place for believing what we do about history and science.

In fact, if there is one ‘normative’ take-away from this thesis, it is that good historiography provides an interesting new voice in a dialogue, a perspective that modifies traditional thought about science in history. Thus, good historiography is itself contextually and historically, rather than normatively and transcendentally defined.

This converges with Jutta Schickore’s analysis of the relation between history and philosophy of science, which she sees as:

neither a bottom-up generalization from historical data nor a top-down ‘test’ of preconceived philosophical frameworks. Rather, it is interpretive and hermeneutic in the sense that one approaches a portion of science that one deems interesting with a preliminary set of tools one deems appropriate, drawn from one’s background knowledge, and see how far it takes one.<sup>686</sup>

One example is her dealing with the work of Francesco Redi, for the interpretation of whose text she initially suspects a notion of testing through replication may be useful. Through confrontation with his texts, she moves away from her initial notion of replication and rephrases her account in terms of repetition; her analytical framework itself, she notes, has changed as a result of her historical analysis, which thereby sheds light on aspects of the usage and meaning of current concepts.<sup>687</sup>

Two examples from recent historiography may help to illustrate the hermeneutical nature of historiography with specific reference to the relation between science and the

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<sup>686</sup> Schickore (2011, 515). On the question of the relation between historical case studies and philosophical claims, see also Kinzel (2015, esp. 53).

<sup>687</sup> Schickore (2011, 521).

world. My first example concerns the historiography of alchemy. Alchemy used to be regarded as an irrational, occult activity that did not fit with the enlightened view of rational science. (We saw an interesting exception to this mainstream view when we discussed Hessen's paper in chapter 4.) From the 1970s onwards, however, it was increasingly recognized that early modern science and alchemy were much more intimately connected than this rationalistic view held possible: the scientific revolution was now seen as 'Janus-faced', containing not just elements pointing towards modern science, but also much of the old and irrational. Even Newton's *Principia*, it was argued, was made possible by concepts that its author owed to his immersion in alchemical thinking. Science and the occult were inextricably connected here.<sup>688</sup>

More recently, especially Lawrence Principe and William Newman have argued that in fact, alchemy itself was *not* the mystical, esoteric, occult business that both previous interpretations had made of it. They argue that it was much more an experimental practice than it was previously taken to be, and needs to be interpreted as an attempt to understand and manipulate the workings of matter – establishing much more continuity between alchemy and modern chemistry than previous interpretations did.<sup>689</sup> Part of their argument is based on textual analysis – for instance, the fact that throughout the seventeenth century, 'alchemy' and 'chemistry' were used interchangeably.<sup>690</sup> They also interpret sources for alchemical practice, such as the notebooks of George Starkey, as evidence that alchemists actually tried their theories "in the fire".<sup>691</sup> Thus, they argue that the historical status of alchemy, and its historical relation to modern chemistry, cannot be understood properly if we do not take into account that alchemy was a practice where "results from the fire impact upon conjectural processes or interpretations".<sup>692</sup>

Principe's and Newman's interpretation of Starkey's ways of relating theory and practice, text and fire, has been modified itself by trying alchemy in the fire. Many alchemical (Principe and Newman prefer 'chymical' to re-establish synonymy between alchemy and chemistry) texts are highly poetical and cryptic, containing entities like a "Fiery Dragon, which hides the Magical Chalybs in his own belly".<sup>693</sup> Principe and Newman show that these riddles are not necessarily pure fantasy; often, they are intended as *Decknamen* for real substances that could be decoded by genuine adepts. Principe and Newman decode some of the riddles themselves, even succeeding in recreating a "Philosophical Tree" that is described by one Philaletes and thereby reinterpreting this image as not a mere literary trope or a manifestation of a collective unconscious (as Jungian interpreters of alchemy suggest), but as rather the result of reproducible alchemical

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<sup>688</sup> See e.g. Dobbs (1975, 6-20, 210-213); Figala (2002, 370-371); Westfall (1975, 215-226); Westfall (1984, 325-331).

<sup>689</sup> Principe and Newman (2001); Newman and Principe (2002, 358-360)

<sup>690</sup> Newman (2006); Newman and Principe (1998, 32-65).

<sup>691</sup> Newman and Principe (2002, e.g. 117).

<sup>692</sup> Newman and Principe (2002, 177).

<sup>693</sup> Newman and Principe (2002, 184).

observation. Principe concludes from this that the same images which previously supported the idea that alchemy was not experimental “may actually be (at least in some cases) not only artifacts of, but arguments in favor of the reality and reproducibility of experimental programs carried out by Stone-seeking alchemists.”<sup>694</sup>

There is an obvious interaction here between what we see happening in laboratories, what we believe Starkey could (under certain circumstances) have seen happening in his 17<sup>th</sup>-century laboratory, and how we believe we are to interpret Starkey’s notebooks. Nor is this an instance of autonomously changing scientific opinion dictating a new interpretation in history of science. Not just because reconstructions of past experiments are only a part of the story, but also because these reconstructions are themselves guided not exclusively by present-day chemistry, but by a combination of that and an interpretation of the language of 17<sup>th</sup>-century notebooks.

It is not the case that at time t1, our chemistry says that the transmutation of elements is impossible and therefore all alchemy, connected as we believe it to be to the idea of the possibility of the transmutation of elements, must be wrong and its results illusory, and that at time t2, chemistry for independent reasons changes its views on the transmutation of elements and the historiography of alchemy changes with it to rehabilitate early modern alchemy. The interactions are far more subtle and not unidirectional. Rather, at time t1 we believe that the things that alchemists talk about can bear no reference to experimental practice because it seems impossible to identify them with material things in the world as we know it by our own chemistry; at time t2 a tension arises between the belief that alchemy was not an experimental practice and the seemingly experimental passages in alchemical notebooks; at time t3 it turns out that it is possible to identify entities in alchemical texts with modern chemical entities, and therefore it turns out that we can understand the beliefs of early modern alchemists in relation to their experimental practices.

It does not follow that at any stage we need to drop – methodologically or really – our own disbelief in the possibility of the transmutation of elements, let alone the whole of our modern chemical knowledge. Our new understanding of early modern alchemists – as experimenting with, and writing about material entities that we think really existed in our shared world – is made possible only by a tension within our previous understanding of early modern alchemists, in which we believed they lived in a world that contained the same chemical entities as ours, but in which they did not talk about these entities, instead discussing hallucinatory entities such as Fiery Dragons. The historical self-image of chemistry, as an experimental activity that must be historically discontinuous with the irrational and unreal claims of alchemy, is thus modified by historical research that applies modern chemical knowledge and practice to the understanding of alchemical activity, interpreting it as taking place in and being about the same world that modern chemistry is about.

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<sup>694</sup> Principe (2000, 67-70 [quote on page 70]).

A different example of the way the understanding of past beliefs about the world is connected to our understanding of the world and feeds back into our own perspective upon this understanding can be found in Daryn Lehoux's *What Did The Romans Know?* Discussing various themes regarding nature-knowledge in the Roman Republic and (especially) the imperial period, Lehoux also turns his attention to the claim that rubbing a magnet with garlic cancels its attractive power, which can be found for instance in Pliny.

Lehoux asks "not just [...] why Pliny believed such silly things, but simultaneously why we think these beliefs are silly".<sup>695</sup> The answer to the first question involves understanding how in Roman thought there is a larger class of cases in which sympathetic and antipathetic substances influence each other. This category system was so strong, Lehoux suggests, and the relation between garlic and magnets fitted so perfectly in it, that it could function as an empirical fact. Lehoux's point is that even though experience may be seen as the final arbiter of belief, it often happens that "inference and testimony [...] bleed over into the category of experience".<sup>696</sup> It is not just dogmatism or even theory-ladenness that Lehoux is getting at; it is our "sloppiness with the very category of the empirical".<sup>697</sup>

For Lehoux confronts us with the possibility that this sloppiness is ours as well. We too, after all, know very well what happens when we rub garlic on a magnet – nothing – without ever trying it: our knowledge about the world and our corresponding categories and classifications are such that it is immediately obvious that garlic and magnetism have nothing to do with each other. But it was just as obvious to the Romans that they did, and just as reasonable that they did not feel the need to test this – or rather felt that this belief was already being corroborated by experience all the time. Hence their confident formulations: "None should be ignorant [...] that because of antipathy garlic rubbed on the magnet impedes it in its natural action."<sup>698</sup> The crux of Lehoux's argument is how much he shows our epistemic situation to be like that of the Romans: we know just as well as they did what happens to the magnet, and our knowledge bears the same relation to our experience as theirs. Only, we know what happens to be the opposite.

The point is that to the extent that Lehoux's study of Roman science has helped us to become aware of aspects of our own beliefs about the world, it can have done so only because in this study we presumed both that the fact that garlic does not demagnetize magnets is true about Roman Antiquity as well – that is, we assume that the Romans were wrong in this belief – and that Roman nature-knowledge was *about* the world. Our understanding of why they could hold this belief involves our own familiarity with phenomena in nature that enable us to make sense of the ancient classificatory and causal system where like and unlike things influence each other in certain ways. Thus, we can understand the belief that garlic demagnetizes as both wrong and understandable as part of

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<sup>695</sup> Lehoux (2012, 134).

<sup>696</sup> Lehoux (2012, 145).

<sup>697</sup> Lehoux (2012, 150).

<sup>698</sup> Lehoux (2012, 138).

a system of knowledge that is about the world. The self-reflective step characteristic of this hermeneutic perspective is the realization that what we have discovered about the relation between Roman science and the world may be the case for our own relation to the world as well.

History *can* challenge our beliefs or the status of those beliefs, but it can do so only because we bring those beliefs to it, not because we leave them at the door when we start doing history. Lehoux sums it up in a way that converges perfectly with this thesis:

All the theories we have been discussing in this book are theories about something, the world, that persists and whose observable behavior in the here and now is indispensable to our understanding of what ancient science *is*.<sup>699</sup>

It is because we understand Roman science as being, like our science, about our world, that we can see its relation to this world as shedding light upon the relation between our own science and the world.

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<sup>699</sup> Lehoux (2012, 232).