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Nature and History

Towards a Hermeneutic Philosophy of Historiography of Science

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Contents

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I dedicate this thesis to my grandparents, who were still alive when I started it, making me sorely aware of the irreversibility of historical time.

Chapter 1: Introduction

1.1 The World in History of Science

This thesis is about the role of the world in history of science: the question what role the things scientists study play in our historical accounts of the development of science. As such, it is about a foundational question in history of science, belonging to philosophy of history of science. As I will try to show in this introductory chapter, this question touches upon many other issues. Generally speaking, different answers to questions about the role of the world are connected to different views about what the function and scope of history of science is, and how it should relate to its subject matter – the history of science – and to the science whose history it studies.

The question of the role of the world brings into focus heated controversies in history of science and related fields. According to some scholars science is, in the end, all about the world, and in order to understand why current theories about the solar system are what they are, what we need to understand is the solar system itself. According to others, this approach would lead inexorably towards Whiggism and 'scientists' history', robbing historiography of its autonomy and critical potential and turning it into the handmaid of science. A third perspective tells us that it is unhelpful to try to put a conceptual fence between scientists and the world that they study, and that without such a fence, the question of the role of 'the world' in history of science turns out to be meaningless.

These possible perspectives, and others, will be dealt with extensively in later chapters, and I will build on them to formulate my own position in the final chapter of this thesis. In the remainder of this introduction, I will try to sketch out some of the motifs with which I believe controversies about the explanatory role of the world in history of science may be fruitfully connected. These are, first, the question to which extent science can be historicized at all; second, the metaphor of history of science as 'bridging' some kind of gap – a gap between science and the humanities, or between nature and culture, or between science and tradition; and third, the problem of the autonomy and authority of history and science.

1.2 History of Science and the Sciences

One important tension that haunts the relation between history of science and science is that between the apparent history-transcending nature of scientific results and the historicity of scientific activity.[1] One way to resolve this tension is to declare only the latter to be amenable to historical research, and say with Alistair Crombie that "the results of scientific activity may be to a large extent impersonal and timeless, but the activity that produces

[1] Cf. Chin (2014, 316) on the relation between 'subliming' and 'subverting' perspectives and their different stances with regard to the contingency and historicity of science.

them is an activity of particular men sharing in the conditions of life, opportunities, and much of the intellectual outlook of a given society and period".[2]

This strategy, distinguishing science as an activity from scientific results, is most pronounced in Helge Kragh's distinction between two senses of science: S1 and S2. S1 he defines as:

> a collection of empirical and formal statements about nature, the theories and data that, at a given moment in time, comprise accepted scientific knowledge. [...] Since S1 is not really conceived as human behavior, it is not the kind of science that would be likely to appeal to the historian.[3]

It seems here that Kragh considers the finished product of science to be outside historical interest, since it is not really part of human behavior. This is confirmed when Kragh goes on to define S2:

> the science (S2) that is historically relevant consists of the activities or behavior of the scientists, including factors of importance to this, in so far as these activities have been connected with scientific endeavours. Thus S2 is science as human behavior whether or not this behavior leads to true, objective knowledge about nature. S2 encompasses S1 as the result of the process but the process itself is not reflected in S1.[4]

This is a somewhat confusing image: historians of science are interested in S2, which encompasses S1, but S2 also becomes invisible in S1. The distinction Kragh makes may be likened to that between theology and Church history; Church history does not coincide with abstract thought about the divine, but may encompass it. But would we say that in that case, this history is not reflected in the resulting theology?

Kragh wants to do justice to the intuition that there is something to science that is not a straightforward result of historical human behavior, but he also wants to talk historically about science. This makes the split between S1 and S2 understandable. However, such a split strongly suggests that insights in the historical aspects of science may turn out to fail to touch at all upon its non-historical aspects. Some authors have been skeptical as to whether science has anything at all to learn from an understanding of its own history – "does one make better cabinets for knowing the history of carpentry?"[5] This rhetorical question, to be sure, could turn out to have an affirmative answer for many reasons,[6] but if

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[2] Crombie and Hoskin (1963, 757).
[3] Kragh (1987, 22).
[4] Kragh (1987, 23).
[5] Turner (1990, 23-24).
[6] Guerlac (1959, 238-239); in general, see Maienschein (2000) and Maienschein and Laubichler (2008) for arguments upholding that awareness of history can improve scientific practice.

we hold on to the supposition that there is an essentially a-historical aspect to science, the scope of history of science will always turn out to be limited.

It seems that the scope of historiography is unnecessarily restricted by an insistence that historians are not interested in any aspect of science that is commonly supposed to be grounded in something other than history. In practice, as Lorraine Daston and Peter Galison have noted, history of science in the past decades refuses above all "to exempt anything, no matter how seemingly self-evident, from historical scrutiny".[7] We need to make sense of what this means, but at least it should not be the case, as Larry Laudan complained in 1990, that historians "[attend] to virtually everything about […] scientists *except their ideas about the natural world*."[8]

Laudan perhaps confused a refusal to ascribe an important role to the world in historical accounts with a refusal to deal seriously with scientists' beliefs – as we will see in chapter 5, the 'Strong Programme' in the Sociology of Scientific Knowledge (SSK) has provided some arguments why in explaining scientific belief formation, we should avoid reference to the external world. But we can only agree with him that there is no reason why scientific beliefs about the natural world should not be of historical interest – after all, these beliefs are themselves historically important. Therefore, we should let go of the split between S1 and S2.

This leaves open the question whether history in fact matters to scientific beliefs, and what this means. Is science not "an on-going present which swallows its own past"?[9] In chapter 2, we will consider this question in terms of the contingentism-inevitabilism debate, and in chapter 4 we will consider reasons for believing that in the end, the past is not reflected in scientific results. The tension between 'being about nature' and 'being historical' will not be resolved quickly, but our attitude towards it ought to be that an affirmation of the historicity of science does not amount to undermining the scientific enterprise.[10] As Ian Hacking said (paraphrasing Nietzsche): "no longer shall we […] show our respect for science by dehistoricizing it."[11]

[7] Daston and Galison (2012, 30).
[8] Laudan (1990, 51).
[9] Alder (2006, 298).
[10] See also Kuukkanen (2011, 597-598) on the distinction between scientific knowledge as being about every time and place on the one hand, and being true in any time and place on the other.
[11] Hacking (1983, 16).

1.3 A Bridge between Two Cultures?

Some authors have ascribed to history of science the task of bridging the gap between the sciences and the humanities.[12] This is plausible enough, since as a branch of history it belongs to the humanities, and it seeks to understand the sciences. However, the question of what exactly the divide between the sciences and the humanities entails is a complex one.[13]

The distinction is often interpreted through the 'two cultures' framework sketched by C.P. Snow in his 1959 Rede lecture, which banks on supposed difference in the attitudes of 'literary intellectuals' and 'scientists' towards progress. According to Snow, scientists "have the future in their bones";[14] their knowledge and mentality is suited to finding technological solutions to problems of overpopulation and poverty. But within the context of history of science, we find an altogether different formulation of the distinction by George Sarton. Sarton laments the "difference of opinion, of outlook, between men of letters, historians, philosophers, the so-called humanists, on the one side, and scientists on the other."[15] The task of the historian of science, according to Sarton, is to reconcile the sciences with the humanities.[16]

Contrary to Snow's, Sarton's aim is anti-technocratic and anti-utilitarian, and he specifically chastises humanists for their impoverished utilitarian view of science.[17] The progress of science as a revolutionary force in history suggests that there is an antithesis between science and tradition, which may lead us to forget that many aspects of science have built on historical developments. The historicity and humanity of science are connected; declaring the past dead means dehumanizing the scientific enterprise.[18] History of science is to save the 'humanity' of science by reminding us of its historicity.

Sarton compares the writing of the history of science to the writing of a biography of a great man, the main point of which would be "to explain the development of his genius, the gradual accomplishment of his special mission."[19] The history of mankind is a history of its creation of beauty, justice, and truth – things which need no definition because we will recognize them when we see them – and this creation takes place in history. Science is special in this regard because it is the only tradition that is clearly cumulative.[20] However, its progress consists not in a transcending of its human origins in a movement towards objectivity, but rather in an unfolding in history of the relation between man and nature. "Nothing could be more foolish than to oppose the study of nature to the study of man."[21]

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[12] E.g. Butterfield (1957, vii).
[13] See also Bouterse and Karstens (2015).
[14] Snow (1959, 10).
[15] Sarton (1931, 69).
[16] Sarton (1953, 14).
[17] Sarton (1931, 29); cf. also Sarton (1952b, xi-xii).
[18] Sarton (1952a, 8).
[19] Sarton (1931, 21).
[20] Sarton (1931, 24-31).
[21] Sarton (1931, 39).

Sarton declares science to be both human and about nature at the same time. "Each scientific result is a fruit of humanity, a proof of its virtue. [...] Each time that we understand the world a little better, we are also able to appreciate more keenly our relationship to it."[22] He also shows how taking seriously the science we have at one point in history does not require us to say that from that point, the past has disappeared:

> Far from there being any conflict between science and tradition, one might claim that tradition is the very life of science. […] Science is not simply the top of the tree; it is the whole tree growing upward, downward and in every direction; the living tree, alive not only in its periphery but in its whole being.[23]

The complexity of this tree can be understood only if we study its growth from its beginnings.[24] Sarton gives an example of how to historicize science (even critically)[25] without giving up its relation to nature.

However, this particular bridging of the gap as Sarton perceives it puts a lot of pressure upon terms such as 'reason' or 'genius', which suggest that the values for which science stands are themselves transcendental or at least universally (and a-temporally) human. This is confirmed when Sarton says that "the views of Einstein were accepted because their truth was proved; those of Velikovsky were rejected because they were unproved or rather unprovable. That is all there is to it."[26] Of course, we believe with Sarton that Einstein got it by and large right, and that Velikovksy got his attempt to re-write both history and astronomy tremendously wrong; but what does it entail to say that this is all there was to it, and to see this as a model of how battles between reason and unreason play out in history?[27] Do we not need to explain what it means to 'prove' something in a particular historical context, and to trace how that particular context has come about? Surely, the simple fact that we know that Velikovsky got it wrong does not suffice to explain why others believed he got it wrong, or – to complicate matters even more – it does not all by itself explain why *we* believe he got it wrong?

Even if we, like the young doctor in Molière's exam, upheld that we believe things because they have a 'quality of truth' to them, it seems that such a view would fall short of genuinely historicizing science. If we can safely inject history-transcending concepts such as genius, reason and truth into history of science because these are rooted in our humanity, then what meaning is there in the claim that we need to understand the scientific tradition – the whole tree – in order to understand its most recent time-slice?

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22 Sarton (1931, 68)
23 Sarton (1952a, 12).
24 Sarton (1953, 6).
25 Sarton (1952a, 15).
26 Sarton (1953, 37).
27 See Gordin (2012) for a contextualization of the Velikovsky affair, which turned out to have a longer tail than Sarton (who played a small part in it himself) knew at the time.

1.4 Science and Its Past

Another way of fleshing out the bridging metaphor can be found in the thought of Reijer Hooykaas: in a 1982 article, he remarks that science and the arts have alienated themselves from each other more and more, and proposes that history of science has a particular cultural mission here.

The humanities are concerned with the thought and actions of people, and the sciences with nature as distinct from human culture; but in history of science, we see that the human element does not get eliminated – which is why it can be the object of a humanistic discipline. After all, there are no natural scientists "in a chemically pure condition": that scientists live in a particular society and family, and have particular character traits and beliefs, influences their thought, however hard they try to eliminate these conditions and be objective.[28] "History shows that the whole person is involved in the natural sciences as well as in the humanities.[29]

This is what enables the bridging work of history of science for Hooykaas: in showing how science is the result of the human spirit, it 'demythologizes' science, showing how it is not infallible but genuinely falls within history; but Hooykaas also draws the very reasonable conclusion that *we* ourselves are genuinely in history, and that there is no privileged point of view from which we can bestow praise or blame. He therefore admonishes historians of science not to divide our ancestors in black and white sheep – an admonition that stands opposite to Sarton's division between reason and unreason.[30]

Hooykaas has a clear view of what the divide between the sciences and the humanities that needs to be bridged is, and what it is not. It is not simply the difference between nature and history, since nature itself is historical in the sense that it is unique and non-repeatable, and there are clear regularities in human history as well.[31] Nonetheless, he says that we need to be careful when we apply naturalistic language – for instance, that of Darwinistic evolution – to the humanities, for it is only in human history that we have a sympathetic relation to our research object.[32] History of science precisely establishes this relation for science; and in doing so, it reminds the scientist to see his work as a part of human culture, and to see himself as the inheritor of a long tradition. "However much natural science strives after objectification and dehumanization, its history is, after all, part of a history of *human* thought and action."[33]

The perspective proposed in this thesis will turn out to resemble rather closely these formulations by Hooykaas. It is useful to think of history of science as a bridge, not between the unique and historical on the one hand and the universal and regular on the

[28] Hooykaas (1982, 154).
[29] Hooykaas (1982, 169 [my translation]). See also Hooykaas (1963, 7-8, 16-17).
[30] Hooykaas (1982, 163-164).
[31] Hooykaas (1966).
[32] Hooykaas (1982, 163).
[33] Hooykaas (1957, 409 [my translation]).

other (for such a bridge is neither possible nor necessary), and not primarily as a bridge between nature and human culture (for the object of history of science is an aspect of human culture as much as any subfield of history, which cannot be understood without its taking place in a natural environment); but as a mediator between current science and the history of which it is a part.

What could this mean for the mutual relations between science and history? The history of history of science in the past half century has to a large extent been one of conscious emancipation from the grip scientists were perceived to hold upon the agenda of the discipline. If in 1963, Bernard Cohen could, while denying that history of science was a bridge between the sciences and the arts,[34] confidently say that "obviously, scientific training is a necessary condition for studying the History of Science, as history is",[35] Paul Forman argued in 1991 that historians of science ought to be radically independent of the science they studied. Their discipline needed to be genuinely intellectually autonomous in order to avoid 'whiggery'.[36] It needed to operate under the notion of a fundamental difference between "'us' as historians and 'them' as scientists".[37]

This also brings us back to the role of nature in history: if we do not want the discipline to be at risk of losing authority over its final explanations to scientists – the 'them' it is supposed to study critically – perhaps we had better assume that the reality scientists try to talk about is not crucial to the course of science. As Forman puts it:

> Only by thoroughly historicizing scientific knowledge - explaining possession of specific pieces or structures of it, not by appealing to a transcendent reality (whose mode of action in this world is no more than metaphorical), but by reference to mundane factors and human actors - can historians of science move away from whiggery and toward intellectual independence.[38]

If we follow scientists in the idea that their mental products acquire some sort of history-transcendence, this prevents us from "adopting the critical attitude essential for an independent apprehension of the reality that science was and is", Forman claims.[39]

It is one aim of this thesis to analyze this intuition – that for history of science, the desired intellectual autonomy, the possibility for a critical study of science, and a downplaying of the role of the 'reality' on which scientists themselves claim their intellectual products are based, come as a package. I think they do not, and I find Forman's plea for radical independence to be fundamentally wrong, but without thereby arguing in favor of the kind of history of science that he claims to oppose – the objections brought in

[34] Cohen (1963, 771-773).
[35] Cohen (1963, 775).
[36] Forman (1991, 87).
[37] Forman (1991, 71).
[38] Forman (1991, 78).
[39] Forman (1991, 85).

against Sarton's perspective on the history of science in the previous section are by and large the same as Forman's.

History of science is indeed a subfield of history, not of science; but in order to be able to feed back into our ideas of what science is, it must take into account these same ideas. This is the core of the perspective upon history of science that this thesis will argue for, which will be approached from the perspective of the role of nature in history of science.

1.5 Outline of this Thesis

This main question, about the explanatory role of nature in history of science, can be approached from two angles: on the one hand, it is a question about the objective role of nature in deciding the course of history of science; on the other, it is a question about whose nature we are actually discussing in this context, and about what the complications are in employing our own beliefs about nature in a historiography that is supposed to shed light precisely on why we have come to believe what we do. This thesis tries to deal with these two questions at once, and culminates in a statement about the role of nature in history of science that covers both aspects. Nonetheless, some chapters belong more or less clearly to one or the other side of the question.

Chapter 2 provides an analysis of a pair of concepts – contingentism and inevitabilism – that is relevant to understanding what is at stake in the causal question about the role of nature. We will define inevitabilism as the claim that the content of science is insensitive to those aspects of historical reality that could have been different.

Chapter 3 analyzes problems connected to 'Whig history' or presentism, and brings us into the debate on the legitimate and illegitimate uses of our own conceptions and beliefs about science and nature. The main problem turns out to be what we will call 'causal anachronism', a species of historical error consisting in the claim that something was the case that in fact could not have been the case. This is what is at stake in debates about presentism, but I will argue that there is no anti-presentist methodological shortcut which can help us to avoid errors of anachronism.

Chapter 4 investigates the relation between inevitabilism and the role of nature in history of science, by looking at possible non-contingent factors that drive the history of science. We conclude that nature itself cannot render inevitable the historical development of science, and that though it can conceivably do so in combination with a non-contingent, history-transcending rationality, such a 'normative inevitability thesis' does not support a causal inevitability thesis as defined in chapter 2 – though our canons of rationality can serve as a hermeneutic point of departure through which we understand past intellectual efforts. We will also see that what at first sight look like alternatives to nature-based inevitabilism – the idealism of Alexandre Koyré and the Marxist social inevitabilism of Boris Hessen and John Desmond Bernal – in fact rely on the idea that the structure of the world determines the content of science, and that according to these authors too, what science looks like in the end can be understood from what nature looks like.

At the other extreme we find the position that nature should not at all figure in our explanations of science, which we will discuss in chapter 5. We will discuss five arguments for this position: the argument from underdetermination; the argument that nature is a common factor that 'drops out'; the argument that society rather than nature provides us with our categories; the argument that historians should deal only with their own areas of competence; and finally, the constructivist argument that nature is in fact the result of social constructions, not its cause. All these arguments will be found wanting, though the question of competence can be turned into a question of circularity which we do not fully resolve in chapter 5.

Chapters 4 and 5 work with the assumption of a distinction between society and nature. In chapter 6, we discuss Bruno Latour's attempt to provide a vocabulary which overcomes the divide between these two. Latour's Actor Network Theory tries to undermine the idea that historical entities need to be related in some way to one unified and stable reality. Since not just concepts, but also their corresponding realities are historically constructed, using current ideas about nature to describe the past may be causally anachronistic in the sense defined in chapter 3. However, the problems with Latour's ontology regarding non-arbitrary addition of new actors to networks, the demarcation of networks, and the tracing of actor's ontologies, suggest that his arguments do not force us to throw away our inherited ontologies completely. Though dislodging the distinction between nature and society, Latour does not demonstrate that the question of the relation between science and nature ceases to be meaningful: a gap can remain between how we see the world and how we believe the actors we follow saw it.

In chapter 7, we look at naturalistic ways of dealing with the relation between science and its objects. Under a naturalistic perspective, nature figures in history of science through causally influencing science. Invisible hand mechanisms can potentially explain both why science is successful and why it does not need to transcend naturalistic explanation. We will focus on David Hull's evolutionary account of science. This turns out to have several virtues, notably its proposal to see science not as a kind, but as a historical individual: a lineage. This lineage evolves because of selection pressures, and it is these selection pressures that form the causal link between nature and science. However, we will also add a few caveats (consistent with some tendencies in Hull's work but not with all), namely that not only the selection of scientific theories, but also the ways in which they mutate, are dependent on cultural and social contexts; this means that in order to explain the content of scientific theories, knowledge of nature is never enough, but detailed understanding of the scientists in their historical context is needed.

This opens up the question of understanding, to which we turn in chapter 8. While chapter 7 dealt mainly with the causal aspect of our problem, chapter 8 also deals with its hermeneutic side – the question of the role of our inherited categories in our dealings with the history from which we inherited them. We can look at science as a tradition that develops in a path-dependent way and accumulates elements from both nature and society or culture, in such a way that the causal influences of both cannot be disentangled in the result. This makes the role of the world in the development of science itself historically conditioned: it is dependent on the shape of the scientific culture of the age, which in turn is not an independent variable, but a result of a historical development in which the external world has continuously played its – ever history-dependent – part. The world is what the scientific tradition at any point interacts with, but this scientific tradition itself has been formed path-dependently by continuous interaction with the world.

This causal thesis has repercussions for the hermeneutic position of history of science and the way in which our beliefs about nature can figure in our historical explanations. When attempting to understand earlier stages of the scientific tradition, our point of entry is a stage in history that has absorbed a lot of previous interactions with nature, on which not just our particular scientific beliefs but also the very demarcation of scientific beliefs from other beliefs causally depend. Making our historiography independent of current scientific categories and beliefs would require reversing in thought the historical process that has led to these categories and beliefs, and even as an ideal this rests on a misjudgment of what historical understanding is. We will end by discussing (and rejecting) four objections to this hermeneuticist perspective on history of science.

Chapter 2: Contingentism and Inevitabilism in History of Science

2.1 The Question

A major question that, in one form or another, occupies scholars of scientific change is whether the actual history they study is the only possible one, or whether different histories were possible.[40] This question is often seen through the prism of the debate about contingency and inevitability in the history of science.

My aim in this chapter is to reflect upon the meaning of this question specifically within the context of a philosophy of historiography of science, which means that somewhat different considerations come into play than discussions of this question within the context of philosophy of science. Notably, one traditional problem immediately retreats from focus, namely the question whether an alternative development of science could have led to something as epistemically *successful* as actual science – which in Ian Hacking's classical formulation of the contingentism-inevitabilism polarity and most authoritative later discussions is the main question.[41]

My premise is that historians of science are interested not primarily in judging epistemic success, but in explaining why science developed as it did. This does not mean that questions about objectivity or justification can be avoided in this explanation, but it means that they are not what is at stake at the outset. Our question is in what sense it is possible to say that science could have developed differently, not in what sense it is possible to say that a different science could have been as successful as actual science. This may seem like dodging precisely the philosophically interesting questions – everybody can easily imagine much less sophisticated alternatives to the current state of knowledge to have occurred, for example if external factors had made sure that human civilization never left the bronze age.[42] However, I want to show that this question can be understood in such a way that we can align different historiographical approaches to different answers to it.

In particular, I will argue that inevitabilist positions have a strong affinity with the idea that the content of science can be explained only by reference to the world that science itself seeks to describe and explain.

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[40] E.g. Hull (1988, 2).

[41] Hacking (2000a, 58-61); Soler (2008a; 2008b, 230-231).

[42] Martin (2013, 925); Fuller (2011, 568).

2.2 Contingency and Indeterminacy

As a first step, we need to realize that what is at stake in the contingentism-inevitabilism debate cannot be captured in terms of the distinction between indeterminism and determinism, and is in fact almost completely independent of it.[43] One can be an inevitabilist while adhering to some kind of indeterminism, or a contingentist while holding fast to the regulative ideal of determinism.[44]

Of course, this means that contingentism is not by definition identical to indeterminism or inevitabilism to determinism.[45] By determinism I mean the belief that any state of reality is compatible with only one state of reality at a later time.[46] Indeterminism is a denial of this; the belief that at least some states of reality are compatible with multiple states at some later time. Graphically represented:[47]

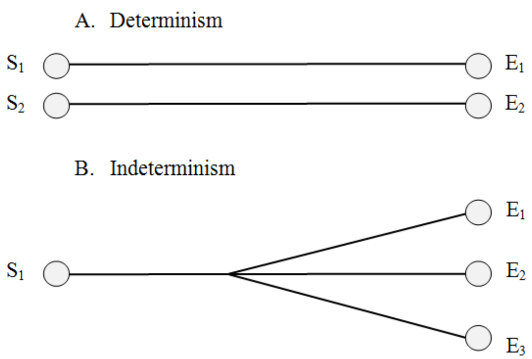

Figure 1: Determinism versus indeterminism

In the deterministic set of worlds A, being on a point on a timeline means inevitably to be on a specific other point later; different outcomes require different conditions. In the indeterministic world B, different outcomes are compatible with the same starting point.

Now, for our current purposes it is crucial that we may be interested in other aspects of possible timelines than their determinacy. Notably, we may be interested in

[43] Contrary to Martin (2013, 926), who sees indeterminism as a strong version of what he calls unpredictability contingency.
[44] Cf. Adcock (2007); see also Nagel (1960) on determinism as a regulative principle for science, including history, and Loewer (2008, esp. 331-334) for a critical evaluation of the idea that determinism has become obsolete because of quantum mechanics.
[45] Ben-Menahem 2009.
46 Cf. Dennett (2003, 25). This definition is less strict than that of Earman (1986, 12-14), who says that those worlds are deterministic which are identical at any time, are identical at all times.
[47] In the timelines used as illustration here, the horizontal dimension represents time; the vertical dimension indicates likeness – that is to say, points that coincide in time differ more when they are further away from each other. This particular illustration is practically identical to that in Beatty (2006, 340).

whether possible timelines converge or diverge from each other. Figure 2 represents two qualitatively different but both deterministic sets of timelines:

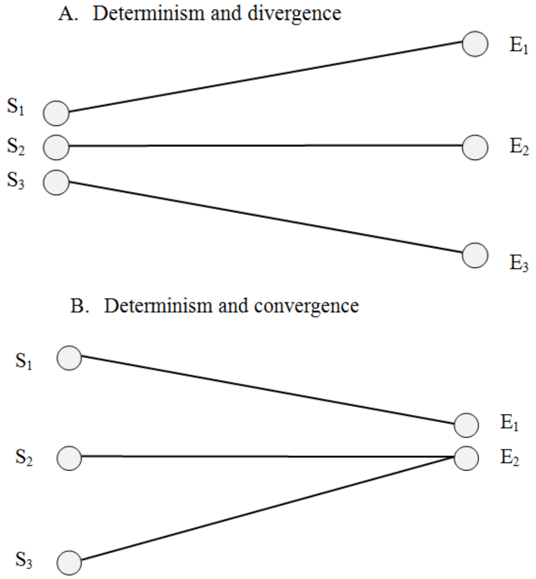

Figure 2: Divergence and convergence in deterministic sets of worlds

In the set of possible timelines A, end-states are more sensitive to the initial conditions than in the set of possible timelines B. Even if from the perspective of one timeline, things are as inevitable in A as in B, doing counterfactual history in set A will lead to different conclusions than in set B: in A, after all, a slight difference in initial conditions would have led to a comparatively large difference in end-states, and in B it is the other way round. (I consider leading to identical situations to be an option as well.)[48]

In one indeterministic world, possible timelines are bound to diverge at some point – because they are identical in the beginning and non-identical later – but it is conceivable both that they will converge again, and that they will diverge further from each other (figure 3):

[48] Ben-Menahem (2009) defines contingency in this way, in terms of sensitivity to initial conditions.

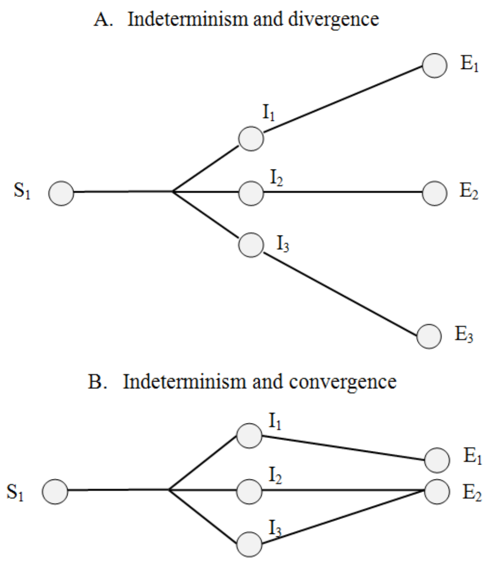

Figure 3: indeterministic divergence and convergence

When we ask the question whether we were bound to end up where now are, we are not asking whether we live in an indeterministic world, but whether the causal processes in our world work in such a way that all or most of the different path that were possible converge towards where we actually ended up.

2.3 Contingency as Path-Dependent Historical Possibility

Next, we need to see that we can meaningfully talk about historical possibility not only in indeterministic worlds, but in deterministic worlds as well. In a deterministic world, after all, there is objectively no possibility: even if the Laplacian demon could give a true answer to the question what would have been different if a certain aspect of the initial situation had been different, he cannot convince himself that things actually could have been different. Historians usually want their counterfactuals to depend not on hypothetical miracles but on what I will call 'historical possibilities'.[49] It may seem that these exist only in indeterministic worlds.

However, it is important here that we differ from the Laplacian demon in that our knowledge of causally relevant factors in history is always finite, and all our claims concerning them are inherently dependent on abstraction and selection. Many of our knowledge claims are compatible with a multitude of states of reality, and span a range of relatively close possible worlds.[50] For instance, our knowledge of the macroscopic fact that there was an assassination attempt on an Austro-Hungarian archduke in 1914 does not fix

[49] Cf. Reiss (2009, 718-722); for history of science, French (2008, e.g. 575).
[50] Cf. also Berry (2009).

precisely which world we live in, and it is therefore itself consistent with multiple different outcomes – importantly, both survival and death of the victim.

It may be that deterministic worlds that were close to each other at this point (to the point that they are hardly distinguishable) have diverged further apart from each other depending on the outcome of this assassination attempt.[51] That is, we can believe both that we live in a deterministic world and that it is meaningful to state that Franz Ferdinand could have survived and the First World War might have been avoided.[52] We can equally well believe that the First World War was both determined *and* inevitable in this stronger sense – it also happened in close worlds in which Franz Ferdinand did not die.

I define contingentism and inevitabilism as theses about the extent to which historically possible alternative paths in history diverge or converge. 'Historical possibility' here means that the occurrence of an alternative is not forbidden by what is implied by our historical descriptions – thus, for instance, survival of Franz Ferdinand on 28 June 1914 is not forbidden by the fact that he was the victim of an assassination attempt on that day. On the other hand, the fact that Franz Ferdinand did die forbids that he could lead the Austrian-Hungarian armies in the First World War. Whatever is forbidden by some historical knowledge in combination with background knowledge is an anachronism with respect to that knowledge (see section 3.3), what is not forbidden by this knowledge is historically possible.

Historical possibility and the corresponding notion of anachronism are distinguished from other kinds of possibility by the fact that its boundaries are dependent on historical time and place. Caesar both crossing the Rubicon at a given moment in 49BC and not crossing the Rubicon then is impossible, but it is a logical impossibility. Caesar flying over the Rubicon by his own strength is a biological or physical impossibility – that is, it is forbidden by our knowledge of the physical environment and the human body, not by our knowledge of the specific context of ancient Rome. Caesar wearing a watch is logically and physically possible: there is nothing we know about watches that implies that they could not function in 49BC, or be worn by Caesar. However, it is forbidden by our knowledge of a specific time and context, namely that of ancient Rome.

The convergence and divergence of historically possible paths can be related to the notion of path dependence, a term used in economics to denote processes whose dynamics do not guarantee convergence to a "unique, globally stable equilibrium configuration";[53] processes, that is, to which "history matters".[54]

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[51] Loewer (2008, 334-336)
[52] Cf. Dennett (1984) on the difference between deterministic and fatalistic worlds; cf. also Taylor and Dennett (2002) and Dennett (2003, 63-95, which also contains another assassination-related example).
[53] David (2007, 97).
[54] David (2007, 92). See Vergne (2010) on the relation between path dependence and randomness.

There are some connotations to the usage of this term in economic discourse that I should want to avoid. First, its usual relation to lack of change.[55] I assume that science is subject to historical processes of change in any historically possible scenario. Second, its association with notions of efficiency; mechanisms of path dependence, such as increasing returns or lock-in, explain why sub-optimal institutions survive.[56] Setting aside the question whether this is useful even in economics,[57] it is hard to translate this aspect of path dependence to history of science, not just in practice but in principle: it presupposes that we can assess the relative efficiency of our existing science and a possible alternative. Whereas we can, in principle, assess this difference when it comes to technologies actually in use and their conceivable alternatives, it seems paradoxical to do so for our systems of knowledge. For instance, even if we believe that it was possible for phlogiston theory to have developed into a (in some sense that would need to be specified) more efficient chemistry than Lavoisier's chemistry, it is paradoxical to agree upon the superiority of the alternative without adopting it.[58]

Taking this into account, the notion of path dependence is very useful: it awakens us to the possibility that the likelihood of something happening is influenced by what happens before.[59] It seems to me that a general contingency thesis with respect to the history of science is well described as the belief that the likelihood of particular later stages in the history of science depends to a large extent upon things that happened before that could well have gone otherwise, so that the content of science in the year 2065 is much more fixed now (in 2015) than it was in 1915, for instance, since a lot of alternative diverging paths that were still possible in 1915 have not been taken. As John Beatty and Isabel Carrera write, "when a particular future depends on a particular past that was not bound to happen, but did, history matters."[60]

The inevitabilist, on the other hand, believes that there were no or relatively few historically possible alternatives in 1915 that would not have converged to roughly our current state by now – either because very few alternatives were historically possible, or because the dynamics of science leads these alternatives to converge in the end.

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[55] Boas (2007, 35-37); Crouch and Farrell (2004, 5-6).
[56] Page (2006, 90); Boas (2007, 35-37); Crouch and Farrell (2004, 5-6).
[57] For an insightful criticism of this (esp. concerning the relationship drawn between path dependence and market failure by Liebowitz and Margolis (1995), who on the basis of this relationship claim that there can be no path dependence in economics) see David (2000, 8-12). Cf. Boas (2007, 38n8) on path dependence in political science.
[58] Chang (2012, 42-50, 62-65). Chang claims that phlogiston theory has been prematurely abandoned and its survival would have 'accelerated' developments in chemistry and physics (65). Consistent with what I am claiming here, Chang goes on to argue that phlogiston theory is and ought to be a part of modern chemistry as well. Cf. also Stanford (2006, 3-26) on unconceived alternatives.
[59] Crouch and Farrell (2004, 12).
[60] Beatty and Carrera (2011, 495).

2.4 Contingency, Historical Explanation, and the World

This distinction between contingentism and inevitabilism leads us to a hypothesis concerning the different status they will ascribe to the external world. If the inevitabilist maintains that historical developments do not make a difference to the content of science, she will most likely claim that this content is in the end decided by the world outside history. The contingentist, on the other hand, will usually say that the makeup of nature does not fix the eventual content of science, but that additional *explanantes* are needed, contingent upon histories that could have been otherwise.

This means that there is no necessary trade-off between contingency and explicability, as there is between indeterminacy and explicability. Contingentism in history of science is rather a statement about the insufficiency of *ahistorical* explanations: saying that the development of the concept of quarks is contingent means that an ahistorical explanation, such as the actual existence of quarks in nature, does not suffice, for there is a lot of relevant historical knowledge consistent with the existence of quarks under which it is possible that science would not come to contain quark physics.[61] The inevitabilist would make the opposing claim that all or almost all worlds that contain quarks and modern physics will eventually contain a quark concept. Importantly, neither the inevitabilist nor the contingentist needs to believe that the concept of quark is not determined; they disagree only about what it is determined by.

I will illustrate this interpretation of the contingency-inevitability polarity by a few historiographical examples. When Bruno Latour seeks to understand how Pasteur and the Pasteurians were accepted, he says that "the first rule of method common to history and the sociology of science is to convince ourselves that this was not necessary."[62] He elaborates:

> it might have been said – it ought to have been said – that this handful of scientists was precisely no more than a handful. It might – and ought – to have been said that they were 'only theoreticians shut away in their laboratories, without contact with the outside world.' This was not said. Why?[63]

Latour says here that it was not inevitable that Pasteurianism was accepted; it could have been ignored. He goes on to provide an extensive account of the groups that Pasteur was able to enlist thanks to his various movements, and of his ability thereby to involve larger movements in his own. Latour's denial of inevitability does not amount to a denial of explicability; he seems to claim not that Pasteur's success was not *determined*, but rather that

[61] There are, of course, other points of departure conceivable than a belief in quarks: a belief in the non-existence of quarks for example, agnosticism about quarks, or the opinion that the question of the existence or non-existence of quarks is meaningless. The point here is that *even* if one assents to the existence of quarks, these quarks do not necessarily and sufficiently explain the existence of belief in quarks.

[62] Latour (1993b, 61).

[63] Latour (1993b, 61).

it was not *inevitably* linked to the content of Pasteurianism. It was possible for the history of science to look differently even after Pasteur's doctrines were conceived of, and recognizing this is important because it creates room for all the other factors involved in the success of Pasteurianism.

Jim Endersby, in a study of the Victorian Darwinian Joseph Hooker, says that:

> once we examine the details of Hooker's career and compare them with those of his contemporaries, it becomes clear that there was nothing inevitable about the changes he participated in. […] I shall also show that there was nothing predictable about Hooker's embrace of Darwinism, which was supposedly the common, secularizing ideology of the scientific professionalizers. Indeed, I shall argue that Hooker's acceptance of Darwinism was more complex and ambiguous than has hitherto been recognized.[64]

Here, too, the denial of inevitability is not a denial of determinacy, but a rhetorical move to make room for additional *explanantes*, by saying that straightforward, law-like relations between phenomena (for instance, between scientific professionalizers and Darwinism) actually disappear in the complexity of the historical narrative.[65]

I will zoom in closer on my last example: Steven Shapin and Simon Schaffer's declaration in *Leviathan and the air-pump* that:

> we want to show that there was nothing self-evident or inevitable about the series of historical judgments which yielded a natural philosophical consensus in favour of the experimental programme[66]

Shapin and Schaffer, too, continue with a book-long account that in the end leaves the reader with the satisfied feeling that the triumph of experimental science actually falls comfortably within the limited range of outcomes consistent with the social and political context of Restoration England. They certainly do not want to claim that this constellation randomly favored Boyle's natural philosophy rather than Hobbes, and they sum up their beliefs in quite deterministic language in the final chapter: "he who has the most, and the most powerful, allies wins."[67]

The point is not that Boyle's triumph is inexplicable, but that there is no ahistorical entity or fact such as the possibility of a vacuum which by itself determined the outcome of the debate. The allies that Boyle had, and his assumed rightness, must themselves be considered to be historical products.[68] Boyle's victory is determined – thus inevitable – in a world in which we take into account all the forces that were in play in Restoration England,

[64] Endersby (2008, 5-6).
[65] Cf. also Kracauer (1969, 27-44).
[66] Shapin and Schaffer (1985, 13).
[67] Shapin and Schaffer (1985, 243).
[68] Shapin and Schaffer (1985, 14).

but this polity could have been different; and the fact that the fate of Boyle's science depended on that polity and not (just) on nature means that it is right to say that it was not historically inevitable.

I hope to have done justice to what Shapin and Schaffer are saying while rephrasing their thesis to fit my definition of contingentism and inevitabilism in the history of science. It seems to me that they are committed to the statement that things could have been different now, in the sense that a chain of historically possible events exists (starting from the 17th century) which leads to a significantly different science which looks less like Boyle's. Inevitabilists are committed to the inverse statement, that there is no chain of historically possible events that would have led to a significantly different science in the end. The crucial semantic issue in each individual case is what counts as 'historically possible' (and as 'significantly different'); the crucial substantive issue is whether anything that could be agreed to be historically possible would indeed have led to a significantly different science.

We have not established that inevitabilism can be grounded only by the external world (we will discuss this further in chapter 4), but controversy about historical possibility and the divergence and convergence of possible histories will inescapably involve questions about the causal importance of different factors in history. Contingency claims are made with this rhetorical goal in mind: to show the insufficiency of other explanations, and replace these by a superior one.[69] Thus, the causal role of the world in history of science is of direct relevance for the contingency-inevitability polarity.

2.4 The Special Position of History of Science

Historians usually work with macroscopic, culture-laden entities and facts – such as Austro-Hungarian archdukes and assassination attempts – whose interrelations display a large measure of subjective indeterminacy. Though they can usually readily admit the influence of non-cultural 'natural' factors, there are reasons why this may be more complicated in the case of history of science.

Often, descriptions of historical events in terms other than those of mainstream general history do not compete with this history for explanatory relevance: for instance, we can zoom in on the medical details of the assault on Franz Ferdinand without thereby jeopardizing the possibility to attribute his death to social, political or ideological causes. In this case, medical and political-historical explanations do not compete. In other cases, they may compete; for example, Jared Diamond's claim that (among other things) diseases to which European conquerors were but American indigenous peoples were not immune explain Western dominance in the modern era is made in explicit competition with cultural explanations of Western dominance.[70]

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[69] On this, see Henry (2008) and Sankey (2008) – Sankey believes that inevitabilism and realism are strongly connected.
[70] Diamond (2005, 405-425).

In addition to this, the historiography of science has a specific problem that other branches of history do not, because it studies specific cultural entities, which are supposed to have a necessary relation to certain natural entities. Usually, historical contingentism as defined above is not in opposition to scientific claims that there are known necessary relations between the entities that populate our universe. However, a tension arises if we claim that our knowledge of those necessary relations is contingent. After all, the following three claims cannot all be true:

[1] Scientific theories are historical entities

[2] Historical entities are historically contingent

[3] Scientific theories have a uniform and necessary relation to the non-historically contingent things they describe.

One way to solve this is to deny [1]: to deny that scientific theories are genuinely historical entities in the sense implied by [2]. This is the position that Steven Weinberg takes, when he says that the laws of nature as known by science are:

> culture-free and they are permanent [...] in their final form, in which cultural influences are refined away. I will even use the dangerous words 'nothing but': aside from inessentials like the mathematical notation we use, the laws of physics as we understand them now are nothing but a description of reality.[71]

Based on his view that science reflects the world, Weinberg's view of physics in particular is highly inevitabilist: he believes that physics

> is moving toward a fixed point […] a theory that, when finally reached, will be a permanent part of our knowledge of the world. Then our work as elementary particle physicists is done, and will become nothing but history.[72]

This confirms that a plausible argument for the inevitabilist is to say that there is a necessary relation between a feature of nature and a feature of some cultural products (namely finished science), while the contingentist will have to maintain that this relation is path-dependent.

I want to emphasize that I consider the inevitabilist position to be a logically valid one. It is not clear to me, however, how scientific theories can pass from a culture-laden to a culture-free state; how cultural influences are 'refined away' (we will discuss Weinberg further in section 3.2). Contrary to Weinberg, I see no reason not to regard scientific theories as cultural products, but their being cultural does not itself preclude their being determined partly (and even significantly) by nature.

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71 Weinberg (1996, 136).

72 Weinberg (1996, 137).

There is not even necessarily a question of truth here: a definite truth-relation is only one of the ways in which the world may determine the shape of cultural products, and moreover, there may be multiple non-contradicting and true theories about this world. Inversely, natural phenomena may be necessary causes of some untrue beliefs. Most importantly, it is unclear where 'truth' enters the chain of causality leading to scientific theories, except as a principle or value in the minds of the scientists.

I repeat that historiographically speaking, the interesting question is not whether scientific theories are true or justified or whether different ones could have been as true or justified. Rather, it is about the ways in which nature plays a *causal* role in determining the cultural products that are scientific theories. Can these products be simply reduced to nature (which would imply commitment to [3] and to inevitabilism), are they determined by something other than nature (which means rejecting [3] and creating space for contingentism), or is there some kind of complex interplay, and if so, is there any identifiable pattern in the way nature plays its part in this interplay? Can the world 'resist' certain scientific theories under certain circumstances, and how?[73]

Conceptualizing the ways in which the world or nature may co-determine some cultural products is a legitimate question for history of science. This, however, requires the conceptual space that this chapter has sought to make. Importantly, it requires the recognition that contingentism – the legitimate default position for historians, since it maximizes the importance of historical knowledge – does not imply that nature cannot be a necessary cause of a certain scientific theory.

[73] Trizio (2008, 253-256) observes that there are several histories of geographical discoveries conceivable but that given the actual distribution of land over the globe, the results of these histories still (in a certain sense inevitably) tend to converge. He asks the question whether the same would hold for high-level hypotheses and theories in physics, and if not, what the differences are that point to contingentism in those areas – for example, that geographical discoverers did not create a new ontology but only added individual entities.

2.6 Conclusions

The two main points argued in this chapter are:

1) Controversies between contingentism and inevitabilism in history of science are best understood as different views on the extent to which historically possible alternative paths tend to converge towards similar or identical later states. Contingentists believe that science is relatively path dependent, whereas inevitabilists believe it is not.
2) Inevitabilism has an affinity with the view that the content of science is eventually explained only by what the world is like.

Chapter 4 continues the second point. The relation between contingentism and the role the world can play in our historical explanations is much more complicated, since it involves both the causal question what precisely the role of the world is, and the question whether our beliefs about nature can shed light at all upon possible histories that do not contain those beliefs.

Chapter 3: Whig History and Anachronism in History of Science

3.1 Whose Nature?

We are dealing in this thesis with the question of what role nature can play in history of science. Inescapably, then, we have to deal with the question *whose* nature we are talking about. Is it our own ideas about nature that inform our explanations and interpretations of past science?

For if that is the case, are we not assuming the superiority of our own ideas about nature and making this superiority an essential interpretive device in our historical interpretation? In general, what legitimate and illegitimate uses can we make of our own concepts and beliefs? Can we say that Aristotle was a biologist, or that Galileo was a scientist? Can we talk about electrons in a historical account of the discovery of electrons *before* the time they were discovered? Or is this anachronistic, presentist, or Whiggish, and is it not inextricably connected to the kind of scientists' history that historiography of science needs to leave behind?[74]

The avoidance of Whiggism seems to be solidly ingrained in the ethos of the historian of science, but precisely this term is, as Peter Dear and Sheila Jasanoff have noticed, rather "loosely defined".[75] When historians accuse each other of Whiggism (which does not happen often in journal articles, and when it does, usually in reviews),[76] the term is used in a slightly different way each time: for denoting the tendency of interpreting the earlier works of scientists "through the lens" of their later works;[77] as a qualification for a history in which "everything is seen as contributing to the great march forward" and where deviations from that path are treated as "mere digressions or […] reinterpreted from today's perspective";[78]

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[74] On the supposed connection between Whig history and scientists' history, see Forman (1991, 78) and the quotes in section 1.4. That the historical relations between innovations in history of science and the contributions of scientists to the discipline are rather less straightforward is made clear by Mayer (2000; 2004).

[75] Dear and Jasanoff (2010, 771). Dear and Jasanoff associate the fear of Whiggishness with the fear of anachronism.

[76] Alvargonzález (2013, 86) says that the Whig label "brands a deep stigma", but without providing examples of this stigma. It is in my interest as well to uphold that Whiggism and its connotations play an important role in policing the boundaries of historiography of science (and I do believe that this is the case), but it is also worth pointing out that its workings are not always easy to trace in print. Rickles (2011), nevertheless, is an example where the accusation of Whiggism figures in a (not unjustified) strong denunciation of the reviewed work (409-410), together with accusations of factual error and plagiarism.

[77] White (2005, 129).

[78] French (2006, 191).

as a history where projects fail owing to "ignorance and lack of understanding";[79] and as "manifest destiny history"[80] – the last case, interestingly, used in a review by Ian Hacking to make the case that an author who calls his history "Whig" actually is not one. What further complicates matters is that the label of 'Whig' historian is sometimes worn with pride, as a way of dismantling what is perceived as too crude a weapon with which to attack one's opponents.[81]

In this chapter, we will unpack some of the intuitions and theses associated with the notion of Whig history, starting with a short discussion of its classical formulation by Herbert Butterfield (section 3.2), and evaluating later treatments by theorists in history of science and continuing to analyze the issues of anachronism, progress and presentism separately. It will be argued that the issue of anachronism is a real one (section 3.3); that the judgment that science exhibits cumulative progress, which has been used by some authors to justify presentism in history of science, does not in fact support this aim (section 3.4); but that the problem of anachronism itself does not justify a general anti-presentism (section 3.5).

The chapter culminates in a proposal to think of our historical categories as themselves developing path-dependently in dialogue between our pre-existing beliefs and our sources.

3.2 Whig History According to Butterfield

Herbert Butterfield's *The Whig Interpretation of History* is a sustained plea to look at the past with the eyes of the past, rather than subordinating it to present perspectives and judgments.[82] The text is essayistic and its claims are argued for in a loose manner, and any selection of the main feature of Whig history as defined by Butterfield can itself only be an interpretation – or, in Butterfield's view, a 'Whiggish' abridgment. Nonetheless, we will try to identify some recurring themes.

What Whig history is, and does wrong, is that it studies the past with direct reference to the present,[83] while "real historical understanding is not achieved by the subordination of the past to the present, but rather by making the past our present and attempting to see life with the eyes of another century than our own."[84] This we will define as the problem of *presentism* here: the, in Butterfield's view, mistaken belief that our present beliefs and categories can genuinely enlighten the past, when actually they ought to be left at the door when we start doing historiography. Related to the vice of presentism is the

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[79] Barnes (2006, 384). Barnes consciously uses the label of Whiggism in a slightly unconventional context, since in the book under review, it is precisely the scientific experts whose project fails because of a lack of understanding.
[80] Hacking (2004, 463).
[81] Mayr (1990, 301); Bod (2010, 479); Alvargonzález (2013).
[82] Butterfield ([1931] 1959, 14).
[83] Butterfield ([1931] 1959, 11-13).
[84] Butterfield ([1931] 1959, 14).

abstraction of things from their historical context;[85] this abstraction entails selection and abridgment, which implies a failure to do justice to the complexity and unpredictability of the past.[86] "All history must tend to become more whig in proportion as it becomes more abridged."[87]

Whig history also defines and judges events and persons in reference to their relation to *progress*. This is connected to the mistake of presentism – it is the failure to see that in past conflicts, all parties are alien to us, and that the quarrels of 16th-century Protestants and Catholics are "as unrelated to ourselves as the factions of Blues and Greens in ancient Constantinople."[88] But it also rests on a mistaken idea that value judgments can be part of history at all.[89]

By making distinctions in the past that make sense only from a present-day perspective, and especially by attempting to reduce what happens in the past to 'deeper' causes,[90] we try to add to the locality and concreteness of the past, and these additions can only lead to error. For instance, "the Whig historian is apt to imagine the British constitution as coming down to us safely at last, in spite of so many vicissitudes; when in reality it is the result of those very vicissitudes of which he seems to complain."[91] Here the point seems to be a kind of *anachronism*: the Whig historian thinks he can see the outlines of the British constitution when it is in fact not there. The error of anachronism is closely related to the error of presentism: by imagining that the present-day British constitution has a history-transcending status that allows it to cast light upon a 16th century in which in fact it did not exist, the historian makes an error.

Modern categories are of no use in understanding. If we are to understand history, we are to leave the present behind and immerse ourselves completely in the complexity and strangeness of the past. Anything short of this will lead to historical errors and undue claims of progress. In the following sections, we will deal with the issues of anachronism, progress, and presentism and selectivity separately, and see that their relations are not as tight as Butterfield suggests.

3.3 Causal and Conceptual Anachronism

Our first problem is that of anachronism. Anachronism can denote a kind of historical error that is stronger than a simple factual mistake: saying that a proposition is anachronistic amounts to saying that it not only *was* not the case, but that it *could* not have been the case at the time. Einstein not just *was* not a falsificationist; he *could*n't have been, since

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[85] Butterfield ([1931] 1959, 30).
[86] Butterfield ([1931] 1959, 20-24).
[87] Butterfield ([1931] 1959, 7).
[88] Butterfield ([1931] 1959, 38).
[89] Butterfield ([1931] 1959, 117).
[90] Butterfield ([1931] 1959, 57-58).
[91] Butterfield ([1931] 1959, 41).

falsificationism had not yet been formulated.[92] Our conception of what is historically possible identifies which claims constitute anachronisms in this sense; in fact, debates about this kind of anachronism can be seen as debates about historical possibility (see also section 2.3).

In practice, the term 'anachronism' is also used for something different, namely the application of our own beliefs and concepts to times and places in which those beliefs or concepts were unavailable.[93] This has in itself nothing to do with the identification of entities or processes that were impossible at the time, but is rather a historiographical counterpart to the anthropological distinction between 'etic' and 'emic' descriptions.[94]

For the sake of clarity, then, we need to distinguish between these two senses in which the term is used, which I will here call *causal* and *conceptual* anachronisms. A causal anachronism is, as we defined above, the belief that something was the case that was actually historically impossible; a conceptual anachronism is the application of concepts or beliefs to times in which they did not exist.[95] (Our definition of conceptual anachronism is not the same as that of presentism; the belief that present categories and beliefs can help us to understand the past, which may rely on historical continuities between past and present beliefs.) I will take for granted that a causal anachronism is always worth avoiding. The question is whether, and under which circumstances, we should avoid conceptual anachronism. Here it is worthwhile to revisit some arguments put forward by Quentin Skinner in his 'Meaning and Understanding in the History of Ideas.'[96]

Skinner attacks the "anachronistic mythologies"[97] he identifies in history of ideas in his time: these involve the idea that there are perennial problems that both we and earlier authors are occupied with, and an insistence that the views of the authors we interpret must have remained stable over time, rather than being linked with concrete and time-bound contexts.[98] Both mythologies lead to an anachronism that has to do with the usage of

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[92] The example comes from Newall (2009, 268-269), who deals with anachronism as a 'logical fallacy'.
[93] Cf. Spelda (2012, 93).
[94] Cf. Jardine (2004).
[95] The term 'conceptual anachronism' can be found in Poe (1996, 352). In Poe's classification, it is one of three species of anachronism, and it means "the propensity thoughtlessly to use concepts from our time to describe another" or "a corruption of the use of modern concepts in historical narratives". Poe's other two species are 'determinism', by which he means something like inevitabilism or fatalism (which for our current purposes we keep distinct from anachronism); and 'partisanship', which is "the habit of making moral judgment where none should be made". It will be clear that, when I use the term 'conceptual anachronism', I do not employ either Poe's classification of anachronism or his definition of this type and its reliance on the psychological state of the historian; I mean all applications of our concepts to times and places at which those concepts were unavailable. A distinction that resembles the current one more closely can be found in Jardine (2000); Jardine (2003) also uses the term 'conceptual anachronism'.
[96] Skinner (1969).
[97] Skinner (1969, 40).
[98] See also the criticism by Burns (2011) that Skinner overlooks the possibility of historical continuities between earlier and later terms and concepts.

concepts. This leads to historical error when, for instance, a historian of the English Revolution interprets the Levellers' concern with the extension of the right to vote as an argument for democracy, and applies his own paradigm of a democracy – a liberal democracy including general (male) suffrage and "some anachronistic concept of 'the welfare state'" – to the beliefs of the Levellers.[99]

We can see that this would indeed be wrong, but what precisely goes wrong, and what does it have to do with conceptual anachronism? When are anachronistic descriptions misleading? Skinner's criterion is rather interesting: his point turns out to be that an account of "an agent's behavior" cannot survive the criticism that it is "dependent on the use of criteria of description and classification not available to the agent himself."[100] This, in fact, goes beyond an indictment of anachronism, to a point where all sociological or psychological explanations become illegitimate. Skinner's point seems to be more about agency or action, which is (again, according to him) by definition about more or less conscious intention and which for that reason needs to be understandable in terms available to the consciousness of the agent.[101] When Skinner satirizes that a "fourteenth-century anti-papalist pamphleteer can scarcely have been *intending* to contribute to an eighteenth-century French constitutionalist debate",[102] his primary enemy is not anachronism, but an improper view of what it means to understand someone's actions at all.[103]

In keeping with the spirit of his intentions, we ought to be careful not to read Skinner as trying to answer our problems – his problem is, in the end, not primarily that of anachronism but of the possibility of treating doctrines as "self-sufficient object[s] of inquiry and understanding".[104] This means that if we abstract from his arguments about agency, Skinner actually delivers rather little in the way of arguments against conceptual anachronism as such.

But we do see how such an argument might get off the ground: by showing that there is, not just a psychological, but a stronger relation between conceptual anachronisms and causal anachronisms, such that the use of a conceptual anachronism will always amount to a causal anachronism (which it does under Skinner's assumption about historical

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[99] Skinner (1969, 27).
[100] Skinner (1969, 29).
[101] This distinguishes Skinner's view on linguistic conventions from that of Pocock (1985), in whose view language goes further in determining the intentions and the boundaries of the actors' possibility to act. See also Bevir (2009), who describes how in practice, Cambridge contextualism has let go of Skinner's and Pocock's methodological prescriptions in favor of a 'broad historicist sensibility' (222).
[102] Skinner (1969, 29).
[103] See also McIntyre (2008, 154-155), and Martinich's (2009) painstaking but ultimately unconvincing distinction between four kinds of meaning in Skinner's theory of interpretation: in particular, Martinich's claim that historians are interested primarily in 'significance' rather than communicative meaning, and the claim that Skinner conflates these two, are respectively doubtful and belied by Skinner (1969, 23), though indeed Skinner's talks in a rather eclectic way about meaning. See also Skodo (2009, 311-313).
[104] Skinner (1969, 31). See also the critical discussion by Lamb (2009).

understanding). This seems to be the case everywhere where the possibility of a certain practice or action is dependent on the availability of a certain concept.

Ian Hacking has made this case in detail for concepts in psychiatry, such as child abuse, attention deficit hyperactivity disorder (ADHD) or multiple personalities.[105] His position, which he has dubbed 'dynamic nominalism' to signify that the categories created by people are not fixed and to distance himself from an anti-realist nominalism, is that there are kinds that come into being together with the concepts that denote them.

> The claim […] is not that there was a kind of person who came increasingly to be recognized by bureaucrats or by students of human nature, but rather that a kind of person came into being at the same time as the kind itself was being invented. My claim about making up people is that in a few interesting respects multiple personalities (and much else) are more like gloves than like horses. The category and the people emerged hand in hand.[106]

It is not just a matter of semantics; not just that under our descriptions, someone in the 19th century is a child abuser while under 19th-century descriptions he is not. The point is that even under our own concepts, it is not clear that someone could be a child abuser in a society or culture that lacked the corresponding concept.[107]

Hacking's claim is not logical, but causal; his point about ADHD, for instance, is that it is an 'interactive kind'. The existence of the category in a society influences the people that fall under this category, possibly because of their awareness of this category but also because of institutions whose existence depends on the category and which are influencing the behavior of the people denoted by the category.[108] The phenomena that ADHD refers to could not have taken their precise shape without the category of ADHD. Whether this is the case for a specific category depends on what it denotes; Hacking does not say that all categories are interactive kinds.

The two kinds of anachronism approach each other more when the phenomenon a concept refers to has specific causal relations to the existence of that same concept in society. Whether this is the case depends on what we mean by our concepts and on our causal beliefs. For instance, if homosexuality necessarily (by definition or with regard to the conditions for its existence) involves the existence of a specific social role for the homosexual, there is a mistake in calling classical Greek pederasty homosexual: it would suggest that ancient Greece had this social role, and mistakenly identify ancient Greeks as homosexuals. By contrast, if the causal explanation of homosexuality is just about genes, identifying ancient Greeks as homosexuals may be conceptually anachronistic but, properly understood, causally impeccable.

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105 Hacking (2000b, 125-162; 2002, 51-72, 64-69, 99-114).
106 Hacking (2002, 106-107).
107 Cf. Gustafson (2010, 311-316).
108 Hacking (2000b, 100-124).

Making the case that some proposition is causally anachronistic depends in each case again on our present beliefs about which things are interactive kinds. We see an example of this when Andrew Cunningham, in line with his thesis on the 'modern origin of science',[109] argues that it was impossible for pre-modern thinkers to be scientists. Science, he argues, is an intentional, game-like activity that someone cannot take part in without knowing it. Pre-modern thinkers knew themselves to be doing natural philosophy, which was an activity directed not primarily at knowing nature but rather at knowing God in nature. Calling Aristotle or 17th-century natural philosophers scientists misidentifies what they were doing, since they *could not* have been scientists in a period where that concept was not available. [110]

Cunningham phrases his argument in anti-presentist terms – he says that our misjudgment of past natural philosophy flows from an inability to "get out of the present" that the historian ought to overcome in some way.[111] I believe this misconstrues the problem, and this is illustrated by Cunningham's ensuing debate with Peter Dear. Dear accuses Cunningham of essentialism, since natural philosophy was not necessarily defined by its link to God, while 19th-century science could still, albeit in different senses, be about God.[112] In his response, Cunningham effectively bites the bullet, saying that as far as he is concerned, natural philosophy and science have essential characteristics without which they cease to be natural philosophy and science, respectively.[113]

If anything, this shows the extent to which Cunningham's own thesis depends on the validity and applicability of his present distinctions. The 'essences' he consciously provides are helpful in identifying whether someone in the past was a philosopher or a scientist or neither, and in spelling out Cunningham's thesis that the proposition that there were scientists before the 19th century constitutes what we here call a causal anachronism; but this distinction is itself something in the present.

It turns out that what is at stake in a controversy like this is not the question of which side is more 'presentist' and therefore more in the wrong, but rather the combined semantic and substantive issues of what we mean by science, and of what we believe people in the past did or did not do, and could or could not have done. The problem of when conceptual anachronisms constitute causal anachronisms is real, but it is hardly soluble in general terms, any more than the problem of 'avoiding historical error' is. If we take Whiggish history to mean a consciously liberal attitude towards causal anachronism, it does not exist.

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109 Cunningham and Williams (1993).
110 Cunningham (1988, esp. 373-386).
111 Cunningham (1988, 367).
112 Dear (2001).
113 Cunningham (2001).

3.4 Progress and Scientific Exceptionalism

In the light of Butterfield's remarks against Whig history, it seems ironical that when Butterfield himself turns to history of science ("in order to try to set that subject on its feet"),[114] he seems to commit a lot of Whiggish sins. He identifies the scientific revolution as "the real origin both of the modern world and of the modern mentality"[115] and tells a lot of smaller origin stories (with 'steps towards' certain outcomes) within this framework.[116] He looks at the history of science on a large scale: a period of five centuries, to which he ascribes a high measure of unity and continuity.[117] He explicitly judges scientific theories in relation to the current state of science, and even sees it as an important task of history of science to draw attention to "the intellectual obstruction which, at a given moment, is checking the progress of thought – the hurdle which it was then particularly necessary for the mind to surmount".[118]

The irony has been noted by others,[119] and been used to discredit Butterfield's argument against Whig history specifically for history of science: didn't this prove that it was impossible for the historian *not* to believe in the progress of science? This is the conclusion that Rupert Hall draws: compared to other branches of history, the historian of science distinguishes himself by actually knowing the right answer to the problems that past scientists were breaking their heads over. "Rightness and wrongness over matters like the velocity of light, the oxides of nitrogen or the charge on an electron have *in the long run* nothing to do with the theories or even the frailty, error, or inconsistency of the original investigator. [...] Thus, it seems to me, the Whiggish idea of progress has inevitably to be built in the history of science."[120] Ernst Mayr gives a similar reason for why the label of Whig history was inapplicable to history of science: change in science is different from change in politics, because of its more obviously cumulative character.[121] More recently, the point has been made by David Alvargonzález that history of science may be 'essentially' Whiggish because of the progressive nature of science.[122]

I believe this line of answer to Butterfield fails, for several reasons. First, it can be undermined by the contention that scientific knowledge is not, in fact, progressive –

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114 Butterfield in a letter to the historian R.F. Treharne, 21 July 1947, as quoted by Bentley (2012, 188). Bentley explains that Butterfield intended to save history of science from the whiggish perspectives of scientists (189).

115 Butterfield ([1949] 1957, 8); cf. also Butterfield ([1931] 1959).

116 E.g. Butterfield ([1949] 1957, 13, 56, 57, 221).

117 Butterfield ([1949] 1957, 7, 203).

118 Butterfield ([1949] 1957, 204). Cf. also e.g. Butterfield ([1949] 1957, 15, 42, 54-55).

119 E.g. Hall(1983, 58); Wilson and Ashplant (1988, 3-4); Ashplant and Wilson (1988, 253), Henry (2002, 4); Carr (1961, 35-36).

120 Hall (1983, 56-57).

121 Mayr (1990, 302).

122 Alvargonzález (2013, esp. 90-94). Alvargonzález is more careful about attributing progress to the social sciences, and says that this also poses a difficulty for the discussion of Whiggism in their history (94).

drawing us into a debate that belongs primarily to philosophy of science, rather than (philosophy of) historiography of science. In order to substantiate our claims about scientific progress, for instance, we have to decide what is of primary importance when we want to measure whether science has progressed, and we have to decide when progress counts as cumulative.

Second, it is very well possible that science in general manifests progress according to some measure, but that this progress is a contingent rather than a necessary fact about science. If things could have gone otherwise, our present-day beliefs about science do not have a status that significantly differs from our beliefs about other things, which may, after all, also have progressed on some scale. Thus, the question of the legitimacy of presentism here becomes connected to the question of the inevitability or contingency of scientific beliefs. Hall means to say that unlike in other areas, in science we would 'in the long run' always have ended up giving the same answers we do now. But this is not just belief in progress in the actual history of science; it is scientific inevitabilism as defined in the previous chapter. There is indeed a plausible connection between conceptual presentism and inevitabilism, though it is based on considerations not concerning the avoidance of anachronism but rather concerning the avoidance of circularity, as we will see in section 5.3.4.

This route also brings us into a minefield of demarcation issues. For each new interpretation or explanation of an episode in the history of science, we would have to establish first that it is genuinely *science*, in the sense of: part of a necessarily progressive inquiry series. This fits ill with the fact that historians generally try to historicize and contextualize not just scientific theories, but the very boundaries between science and non-science.

Third, as an answer to Butterfield, the thesis that science is necessarily progressive, even if demonstrably true, misses the point. When Butterfield forbids us to talk about progress in political history, he does not forbid us to say that we would rather live in his 20th-century Britain than in the 16th century, or even to be confident that on some scale there has been evident progress; his point is that saying this now does not add anything to our understanding of what happened in the past. The progress in question is not a 16th-century actor's category; it is something *we* say, and something we say only as a result of history. In no way can such a statement be regarded as doing justice to the past on its own terms. When we say "progress" where the historical actors didn't, we are, according to Butterfield, doing something other than history.

> The truth is that [...] historical explaining does not condemn; neither does it excuse; it does not even touch the realm in which words like these have meaning or relevance; it is compounded of observations made upon the events of the concrete world; it is neither more nor less than the process of seeing things in their context.[123]

[123] Butterfield ([1931] 1959, 117).

It is important that though Butterfield clearly treated the history of science as one of progress, he tried to shake off presentism and anachronism in history of science as well as in any other field of history.[124]

Our response to Butterfield's radical historicism, then, can never be that history of science is special because it turns out to manifest progress, no matter how subtle and nuanced our conception of this progress may be.[125] If we disagree with Butterfield's point that judgments about progress need to be avoided in historiography because they are conceptually anachronistic and *all* conceptual anachronism needs to be avoided, then our disagreement stretches to political history as well as to history of science.

3.5 Selection and Presentism

The critics of Butterfield mentioned in the previous section seem to be on strong ground not on the issue of progress, but on the issue of selectivity. Perhaps Butterfield's insistence, stated emphatically in *The whig interpretation* but also in that apparently Whiggish *Origins of modern science*, that we should never abridge because "all history must tend to become more whig in proportion as it becomes more abridged",[126] that we should look through a microscope,[127] is itself a plea for the unattainable. As Hall comments, "I am not confident that the 'concrete facts' seen through the microscope assemble themselves a-theoretically into 'explanations', whether one examines cells or the French Revolution."[128] This argument for the inescapability of selection and abstraction can be turned into an argument against Butterfield's anti-presentism: it does not really make sense to publish a book as its own translation, David Hull says,[129] and similarly present-day concerns can be used responsibly when we want to make sense of the past for the present.

Maybe Butterfield's intuition that some history-writing gets the relation between the present and the past wrong is correct, but his diagnosis of why this is the case is not. This is what that A. Wilson and T.G. Ashplant argue in a two-part article on Whig history. They follow Hall (as I do) in his criticism that selection is inevitable and should be non-arbitrary,[130] and go on to reformulate where, according to them, the problem of Whiggism actually begins: for Butterfield, the Whig fallacy is the principle of "direct reference to the present", that is, "with one eye on the present";[131] another way of interpreting it, which Butterfield's choice of words sometimes suggests and which Wilson and Ashplant explicitly

124 E.g. Butterfield (1950, 56-57).
125 See e.g. Arvagonzález (2013, 90-93).
126 Butterfield ([1931] 1959, 7).
127 Butterfield ([1949] 1957, 8).
128 Hall (1983, 51). Cf. Watson (1986, 21-22).
129 Hull (1979, 7-8).
130 Wilson and Ashplant (1988, 6-9).
131 Wilson and Ashplant (1988, 10).

embrace, is the problem that historians are "*with both eyes in the present*",[132] that is, "constrained by the perceptual and conceptual categories of the present, bound within the framework of the present, deploying a perceptual 'set' derived from the present."[133] This predicament can lead to misunderstandings that do not disappear simply because of a closer look at the sources: "present-centred categories can well survive the experience of research, for that research can be subordinated to those categories."[134]

Wilson and Ashplant are thinking of cases in which an explanatory *asymmetry* is made between beliefs in the past that resemble modern beliefs and therefore require no explanation, and beliefs in the past that do not resemble modern beliefs and therefore do require explanation. For example, assuming a present-day contrast between science and superstition, reason and magic and reading such a contrast into the past will lead to an unbalanced view, in which astrology and belief in ghosts in the 17th century require more explanation than rationalism or skepticism.[135] That this attitude tends towards a mistaken view of history is evident from the fact that the history of science has precisely turned out to undermine a dichotomy between science and magic in the 17th century, Wilson and Ashplant say. "An adequate understanding of the thinking of seventeenth-century men and women requires that we go beyond our own initial present-centredness."[136] We are on the terrain of historiographical virtues and vices again, and presentism tends to lead us astray.

This is a convincing example, but let us proceed carefully. What this example shows is how, from the perspective of a present-day scholarly consensus about the relation between science and magic in the 17th century, other scholarly beliefs about this relation look like a conceptual and possibly a causal anachronism. Now, the point is not to argue against the judgment that these earlier beliefs constituted an anachronism; it is rather that this judgment is based on semantic and causal beliefs – beliefs about what science and magic mean and about how the things they refer to actually related in the 17th century.

Saying that distinguishing between science and magic is an instance of anachronism only establishes disagreement about the meaning of concepts and the modal structure of history; it does not establish that this disagreement follows from the fact that one side is 'present-centered'. The fact that Joseph Agassi, in a 1963 invective against presentism in history of science, chastised other historians precisely for *failing* to apply a distinction between science and magic, illustrates this.[137]

How could scholars become aware that their categories did not match those of the sources? One possible answer is that the scholar always ought to acquire his categories from

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[132] Wilson and Ashplant (1988, 10).
[133] Wilson and Ashplant (1988, 10).
[134] Ashplant and Wilson (1988, 261). But cf. also Abadía (2009, esp. 65-69).
[135] Ashplant and Wilson (1988, 257-260). The historical work here criticized by Ashplant and Wilson is Keith Thomas' *Religion and the decline of magic*. Here Thomas Hobbes is one of the thinkers whose ideas resemble present-day opinions closely enough not to require explanation (258).
[136] Ashplant and Wilson (1988, 260).
[137] Agassi (1963, 11).

the sources. This approaches Butterfield's solution of shunning all abridgment. After all, it is when we fill in the gaps in our source material with our own beliefs and according to our own categories that we start committing conceptual anachronisms and thereby (according to Butterfield) historical errors. Wilson and Ashplant are right that this is a misdiagnosis: present-centered categories are unavoidable and can survive research, and therefore empirical research will not simply and autonomously erase them.[138]

Another possible answer is that the historian ought to have been aware all along that categories like science and magic are not cultural universals. When someone says that Aristotle was a 'biologist', the problem is not just that she has not studied the sources closely enough, but rather that she forgets that the notion of biologist is embedded to such an extent in specific and historically contingent institutions and practices that it is highly unlikely that the term could be applied to classical Greece at all without being severely misleading.[139] There is a gap between our category system and that in which the historical evidence was produced, and Wilson and Ashplant say that the historian needs to be "first aware of that gap"[140] – though even then, present-centeredness is inherent in historical research, which is therefore inherently problematical.[141]

I believe this is too pessimistic. A more dialectical relationship between categories and sources is at least possible. It is conceivable that we approach 17th-century sources with the assumption of a clear distinction between science and magic, but that what we find in the sources does, if not unequivocally falsify the applicability of this distinction, at least contradict some of the expectations that accompany it: the expectation, for instance, that science and magic will be practiced by different persons in the 17th century, or be connected to different social roles. If we find that enough of our implicit expectations are contradicted, we can proceed to revise some of our assumptions. One of the ways in which we can do that may turn out to be letting go of the opposition between science and magic.

In this particular case, our knowledge that the distinction between science and magic is both a conceptual and a causal anachronism (since science and magic are interactive kinds and since they are not so clearly distinct in the 17th century) has been made possible by historical study of the 17th century. Far from providing, as Peter Dear has called it, an illustration "of the fallacies that can result from […] hermeneutic circularity",[142] it is better to say that Wilson and Ashplant's narrative puts hermeneutic circularity in a positive light, where from the dialogue between our original categories and the historical sources there follows a change not just in our view of the sources, but also in our own categories. We *do* get beyond our initial present-centeredness, but we do so only because it is challenged by

[138] Ashplant and Wilson (1988, 266-267).
[139] Jardine (2000, 259-265).
[140] Wilson and Ashplant (1988, 13).
[141] Wilson and Ashplant (1988, 16).
[142] Dear (2012b, 51).

historical research – research to which we bring our present beliefs and categories, in the knowledge that they are revisable and that history bears on them.

This attitude differs from Butterfield's empiricism or historicism and from presentism, and may be aptly called hermeneutic: it recognizes that present and past 'horizons' differ, but assumes that the past is not completely alien and that we can build on continuities between it and the present in order to bridge some of the gaps between it and ourselves.

3.6 Avoiding Anachronism in a Changing Present

From the preceding, we can draw some general conclusions about Whiggism in history of science, bringing together the separate strands of anachronism, progress, and selectivity and presentism.

Butterfield is mistaken in his suggestion that all selection and abstraction proceeding from a present-day perspective are necessarily wrong, but the reason is not, as has often been claimed, that in history of science the present-day perspective is especially privileged thanks to scientific progress, and therefore better equipped for looking back than present-day perspectives in other fields. Nor is it the case, as Wilson and Ashplant suggest, that bringing our present-day categories to historical research is always a hindrance to understanding, and one that the historical sources cannot modify. The confrontation with historical sources can modify our categories, and those categories are what we understand history with.

There are instances in which our categories are conceptually anachronistic, which can become instances of historical error when the categories in question are interactive kinds in Ian Hacking's sense. In those cases, our conceptual anachronisms may spill over into causal anachronisms – leading us to believe that there were scientists in a period where there could not have been, for instance. But recognizing this causal anachronism (if it is one) results from insight into the extent to which 'scientist' is an interactive kind combined with familiarity with the sources; not from a general insight in the wrongness of presentism. Dear's and Cunningham's disagreement about the usage of the term 'scientist' underlines this.

Other examples abound. When Thomas Kuhn advises that in so far as possible, the "historian should set aside the science that he knows" and should learn it from the sources,[143] or when Collins and Pinch claim that "we shall not understand the Pasteur-Pouchet debate as it was lived out unless we cut off our backward seeing faculty",[144] these

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143 Kuhn (1968, 76). Interestingly, Kuhn also says in the same paragraph (77) that "the historian should pay particular attention to his subject's apparent errors, not for their own sake but because they reveal far more of the mind at work than do the passages in which a scientist seems to record a result or an argument that modern science still retains" – a present-centeredness that immediately contradicts the idea of setting aside the science that the historian knows.

144 Collins (1993, 85).

remarks stem from aspects of a historiographical ethos that they share with Butterfield and Cunningham, which exaggerates both the dangers of presentism and the promises of empiricism. In fact, presentism does not automatically lead to causal anachronism, and it is not necessarily based on the assumption of inevitable progress.

Our own categories and beliefs will always be something of the present, and though historians rightly avoid causal anachronism, the identification of causal anachronism depends on those categories and beliefs. But our present changes, and its beliefs and categories may be modified as a result of historical knowledge. This is as it should be; after all, we cannot be expected to know of any phenomenon *a priori* whether or not it could be culturally universal – it is precisely because of historical (or, for that matter, anthropological) knowledge that we can assess the range of diversity between human cultures; it is precisely because we have been confronted with knowledge about past societies that we have come to believe some practices to be contingent that we might otherwise have considered natural and inevitable. If historiography plays this role for science, this is only for the better.

3.7 Conclusions

What do these considerations imply for the role our beliefs about nature can play in historiography of science, as far as the problems of Whiggism in the sense of presentism, anachronism, and triumphalism (in the sense of belief in inevitable progress) are concerned? We can draw the following conclusions:

1) The question we need to ask when we involve natural entities in historical accounts, is whether the involvement of these entities constitutes a causal anachronism. We have seen that this may, generally speaking, be the case under the assumption that natural entities, too, can be interactive kinds in Hacking's sense of the word; that their existence goes hand in hand with the availability of a corresponding concept. In chapter 6, we will see that Bruno Latour holds this position, but unless it turns out that this case can indeed be made in general, there is no reason not to involve natural entities in historical accounts.
2) That this is legitimate does *not* depend on scientific exceptionalism: it is not because science manifests progress that presentism with regard to natural entities does not constitute a causal anachronism. It is simply because what constitutes such an anachronism is identified by our present causal beliefs. Thus, there is no anti-presentist default position to which history of science forms the exception.

Chapter 4: Roads to the Inevitable? Nature, Thought, and Society

4.1 Candidates for the Great Explainer

In chapter 2, we hypothesized that one plausible way to support the idea that it was historically impossible for science in the end to become something other than it has actually become, is to claim that the history of science is guided by factors outside of historical contingency.

We also saw that one plausible candidate for this guide is nature: *ex hypothesi*, nature is what exists independent of historical human action, and science seems to be highly sensitive to its structure. It may be that science tracks something stable and historically inevitable, and that it thereby participates in this inevitability; that, as Steven Weinberg has claimed, it "is the way it is not so much because of various adventitious historic acts of invention, but because of the way nature is."[145] Indeed, attempts to undermine inevitabilism often turn on discrediting the idea that the development of scientific disciplines is rendered inevitable by nature.[146]

However, there is no obvious reason why nature would be the only entity that could play this role. For instance, it seems that from a Marxist perspective it is possible to say that society has an inevitable final state from which a corresponding final state of science follows, so that inevitabilism about science is grounded in inevitabilism about the resolution to social dialectical processes. Yet another possibility is that there is an inevitable logic to the development of scientific concepts and ideas.

This chapter consists of three parts. In the first, we proceed from the possibility that science is determined by nature alone. I will argue that this possibility cannot work, and that its apparent plausibility always relies on an implicit normative notion of universal rationality. This is not a reason for immediate rejection of this possibility, and we will continue this part by discussing the uses and abuses of rationality in history of science, by discussing Max Weber and Robert Merton. Second, from this discussion we move into the more idealist pole of the spectrum of possibilities sketched above; into the possibility that science is determined by an internal and necessary logic of the history of ideas. As possible representatives of this idea, we will discuss the work of Arthur Lovejoy, the *Journal of the history of ideas* in its early decades, and Alexandre Koyré. Finally, we will consider a Marxist view of the history of science, through Boris Hessen and John Desmond Bernal, in order to see in what sense this perspective manages to connect inevitable aspects of science both to society and to nature.

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145 Weinberg (2015, xi). Cf. also Boon (2015) on the support of inevitabilism by metaphysical realism (though she does not endorse such a realism herself).

146 This is true for two of the three arguments put forward by Kidd (2013, esp. 317-320).

Together with the question of what, according to these authors, is the 'great decider' in science – what determines the contents of its beliefs about nature – and how it relates to nature, we will also take an interest in their perspectives on the role of our own understandings of nature and science in our interpretations of past science. That is, in their views on the hermeneutic status of our own beliefs.

4.2 Nature-Based Inevitabilism

4.2.1 Irresistible Nature: Steven Weinberg and the Teaching Machine

Allan Franklin has tried to refute contingentism by connecting it to a "lack of belief in the efficacy of nature, as revealed by experiment, to decide scientific issues."[147] He argues that in fact, there are plenty of examples from the history of science – the rejection of the principle of parity conservation, and the solution of controversy around the existence of 17-keV neutrinos. Here, Franklin argues, experiment did decide the issue, and that this is an argument against contingentism.[148] Conversely, Harry Collins argues that in scientific controversies, nature usually speaks very little: referring to the supernova that supposedly led to the first measuring of gravitational waves, he says that "nature would have spoken for only 0.00000000013% of this half century."[149]

Not only in theoretical debates, but also in historiographical practice, the boundaries historians draw between the contingent and the inevitable often seem to fall together with what in science genuinely reflects the world and what does not. Peter Bowler has written on conceivable alternatives to Darwinism, asking the question what would have happened had Darwin not survived his journey on the Beagle – a historical possibility. Bowler's position is to a large extent contingentist – the rise of Darwinism in the nineteenth century was not made inevitable by the way the world is, but the personality of Darwin was a necessary cause of its development and triumph, absence of which could have led to a wholly different theoretical development.[150] However, Bowler has also explicitly stated that he believes that, though a lot would have been different if Darwin were deleted from history, the theory of evolution by natural selection would eventually be discovered "because it does reflect an aspect of how nature actually works."[151]

What would it entail to say that nature on its own forces us to draw specific conclusions; that, as it has recently become more fashionable to say about material objects such as "gunpowder, dyestuffs, metals, clays and ceramics" *et cetera*, "all of them spoke irresistibly, and not only by interpretation and representation"?[152] What does it entail to

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[147] Franklin (2008, 243).
[148] Franklin (2008, 244-251).
[149] Collins (2004, 15).
[150] Bowler (2008; 2013).
[151] Bowler (2015, 22), in response to Richards (2015, 17) and Love (2015, 10) on his underlying commitments to inevitability.
[152] Klein (2010a, 9), though see also ibid., 19.

believe that nature 'irresistibly' forces our beliefs upon us, or that evidence is "overwhelming"?[153]

Certainly, something other than that it logically forces these beliefs upon us. When we say that the evidence against geocentrism is overwhelming, we do not say that a crafty sophist could not still find logical space for maintaining it; it is that the existence of such a logical space cannot move us to consider the possibility of geocentrism as a 'live option'.

If we want to say that the content of science is determined in this sense by nature alone, the negative, falsificationist way in which nature can speak irresistibly – by loudly shouting 'No' to the general beliefs that imply what turn out to be false predictions – does not suffice; we need a way for nature positively to force us to believe something. But is it at all possible for it to do this purely on its own? If that is the case, then how do we account for historical change in our beliefs? At the very least, the existence of different scientific beliefs in history means that our temporal and spatial relation to nature matters. The inevitabilist thesis is, obviously, one that spans many generations – it says that in the end, a scientific tradition will always arrive at the same point, not that nature forces everyone at all times to believe the same. But what mechanisms does nature have that force science to converge to the same point over time?

Steven Weinberg in his 2015 book *To Explain the World* provides a 'nature-based' inevitabilism along with a mechanism. According to Weinberg, successful scientific theories such as Isaac Newton's theory of universal gravitation are what they are, in the end, because of the way the world is. This means that progress is made not by philosophically inspired methodological improvements such as that by Descartes – Descartes, after all, was wrong about many things,[154] and this goes to show that the human mind has nothing to add to science. Rather, we learn everything related to science by confrontation with the world, which "acts on us like a teaching machine" and teaches us not just what nature, but what genuine science is.[155] Science *itself* is discovered, not constructed; it fits the world it seeks to explain much like agriculture fits the biological realities which it exploits to gain food.[156] That science matches the world so directly is also reason for Weinberg to think that presentism is legitimate in history of science: though he tries to avoid anachronisms with regard to what the Presocratic philosophers could have thought, for instance,[157] science, as a history-transcending entity that is grounded in nothing but the world, is never an anachronism.

How does this teaching work? The world, Weinberg says, gives someone who finds a good explanation an intense sense of joy and satisfaction.[158] Weinberg quotes Ptolemy on the sense of joy he felt when describing the movement of the stars: "my feet no

153 Brown (1989, 92-93).
154 Weinberg (2015, 204).
155 Weinberg (2015, 255).
156 Weinberg (2015, xi). Cf. Hacking (1996).
157 Weinberg (2015, 204-206).
158 Weinberg (2015, 254).

longer touch the Earth, but, side by side with Zeus himself, I take my fill of ambrosia, the food of the gods."[159] He says that Copernicus must have been even happier when he could discard all the fine-tuning that Ptolemy's system required by assuming the earth to move, and that Kepler must have enjoyed very much "replacing the Copernican mess with motion on ellipses".[160]

This pleasure then serves as the basis for theory selection:

> [Newton's model] provided universal principles that allowed the successful calculation of a great deal that had previously seemed mysterious. In this way, it provided an irresistible model for what a physical theory should be, and could be.
> This is an example of a kind of Darwinian selection operating in the history of science. We get intense pleasure when something has been successfully explained, as when Newton explained Kepler's laws of planetary motion along with much else. The scientific theories and methods that survive are those that provide such pleasure, whether or not they fit any preexisting model of how science ought to be done.[161]

This is an interesting but ultimately unconvincing mechanism. First, there is a tension between the metaphor of Darwinian selection and inevitabilism itself: though Darwinian processes of mutation and selection may explain the functionality of science in causal terms, it is not clear that they can support a thesis of inevitabilism – much like Darwinistic processes in the biological world, while they may explain the evolution of humankind, do not thereby state (let alone explain) that this evolution was inevitable.

Second, Weinberg's sole feedback mechanism, according to which the world rewards success with pleasure, does not hold up. Crucially, and expectedly, delight in discovery can be misleading, as Weinberg himself at one point suggests: Ptolemy's joy in astronomy was "flawed – it always is". This is inconsistent with the idea that pleasure is the *signum veritatis* which Weinberg wants it to be, unless not only we, but all relevant historical actors have in the end been able to distinguish between genuine and illusory pleasure. Weinberg says that Ptolemaic epicycles are something to be "repelled" by, and seems to expect that Copernicus and Kepler must have experienced ever greater pleasure in doing away first with these epicycles and then with "the Copernican mess";[162] but as a mechanism for the development of ever-better theories, this is rather unbelievable. The implication would be that the world reserves an as yet unknown degree of delight for the discoverer of the Grand Unifying Theory. This begs the question why the world would, through natural processes, bring forth creatures with aesthetic sensibilities that are so well attuned in advance to the deep structure of the universe.

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159 Weinberg (2015, 100, 255).
160 Weinberg (2015, 254).
161 Weinberg (2015, 248).
162 Weinberg (2015, 255).

In fact, aesthetic sensibilities may differ over time – for instance, a model containing a proliferation of circles rather than a small number of ellipses may lead to different degrees of pleasure in different times. At the very least, Weinberg's model would need to integrate in his model historical differences in the (aesthetic) evaluation of scientific theories: why did a different theory make Copernicus happy than did Ptolemy?

Weinberg does not, in fact, provide a mechanism that can sustain a solely nature-based inevitabilism. We will discuss a more plausible Darwinian evolutionary model for the history of science in chapter 6.

The point to take away from the fact that a 'nature-based' inevitabilism that takes into account *only* the autonomous working of nature upon humans fails, is not a logical point about underdetermination (on which see section 5.3); it is rather that evidence does not in fact overwhelm different people in the same way. We need to specify to whom the evidence is overwhelming in what way. However, the nature-based inevitabilist may grant this, but add that it is a trivial exercise to identify which characteristics someone needs to have, in order for a specific overwhelming influence of the evidence to apply – namely that she is rational.

This is, I think, the most plausible case for nature-based inevitabilism: that nature completely determines scientific development, if science is rational. Friedman aptly summarizes this within the context of an analysis of the competition between rationalistic philosophies of science and SSK: "if there *were* 'super-cultural' norms of rational argument and evidence, so the argument goes, then scientific theories would be determined one way or another by reality, experience, and reason".[163]

4.2.2 Nature and Rationality I: Max Weber and the Hermeneutic Function of Rationality

One way to counter this view is by skepticism about the existence or accessibility of super-cultural norms. We will deal with this and related problem in the next chapters. But within the context of philosophy of historiography of science, we also need to ask another question, namely what the existence of (and our access to) super-cultural norms, transcending historical contingency, would mean for the extent to which science is amenable to historical explanation. Can we talk about rationality 'determining' the relation of scientific theories to nature without declaring essential parts of science to be outside the reach of historical study (similar to the perspectives we discussed in section 1.2)?

As a window upon this problem, we will revisit and reinterpret some texts by Max Weber. The judgment of rationality plays a major role in Weber's work, and although his project was much broader than 'merely' the understanding of Western science, his remarks on related subjects are suggestive of his solution to the problem of how to study institutions embodying rationality historically.

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[163] Friedman (1998, 244). A good example of a rationalist embracing this thesis, and the corresponding refusal to let sociology touch science at all, is Jarvie (1984).

Weber applies a rigorous distinction between judgments of facts and judgments of value, between 'is' and 'ought'.[164] However, this distinction does not exhaust his opinions on the relation between value judgments and science, since judgments of fact require a judgment of the *validity* of a certain argument. While practical value judgments are inescapably subjective and may differ from person to person, a "methodically correct scientific argument" must be recognized as valid as well by a Chinese reader who does not share the ethical opinions of the Western scholar.[165]

The key word is 'must' (*muβ*): whence the *duty* of the Chinese reader to accept certain kinds of reasoning as valid? I interpret Weber as claiming that judgments of rationality have, in the end, objective validity. While people can legitimately differ about subjective value judgments, they can be objectively right or wrong in judgments of rationality, because, in the latter case, something in the world makes them right or wrong.

Science and values touch each other in another way: Weber clears away with some impatience the objection that his ideal of value-free science means that science cannot study the subjective judgments of other people.[166] The very possibility of sociology depends on this. The scholar simply needs to separate his observations of empirical fact from his own practical judgments. The study of an empirical consensus is completely distinct from the attribution of validity to this consensus, and therefore a 'realistic' science about ethical things does not produce an 'ethics' in the sense of a body of claims about what ought to be the case. "No more so than" – and here Weber employs an analogy that is of interest to our present purpose;

> No more than a 'realistic' account of the astronomical conceptions of, say, the Chinese – one, that is, which shows from which practical motives and how they practiced astronomy, and to which results they came and why they came to these results – can ever aim to prove the validity of this Chinese astronomy.[167]

In this passage, Weber quite naturally treats the case of the history of Chinese astronomy in relation to the validity of Chinese astronomy as analogous to the case of a science of ethics in relation to the desirability of a certain system of ethics. That Weber considers this analogy to be unproblematic in spite of the fact that the first involves something objectively valid and the second something inescapably subjective, will turn out to be part of his solution to the problem of the possibility of a history of science.

After these negative views – empirical science does not teach us about the validity of anything – Weber rehearses his positive views of research in the humanities:

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164 Weber (1904, 186); Weber (1919a).
165 Weber (1904, 155).
166 Weber (1917-1918, 462).
167 Weber (1917-1918, 464 [my translation]).

> Empirical-psychological and historical research into a specific value perspective with respect to its individual, social, and historical determinedness leads one only ever to explaining it though understanding. That is not nothing.[168]

The notion of explaining through understanding – 'verstehend Erklären' – illustrates that there is no chasm in Weber's thought between the scientific ideal of explanation and the humanistic ideal of understanding.[169] The disciplines that study aspects of human history have to do explanatory work just like any other discipline, but they have to do so in part through understanding. For human actions have inner motives that are understandable in the sense that we can 're-experience' them – though Weber formulates his remarks with care and with the necessary scare-quotes. There is no immediate access to past experiences, and there is a crucial step of 'Wertbeziehung': articulating one's own experience by connecting it to values, through which the object of study can be constructed.[170]

This 'construction' of the object of study is a necessary step because an objective scientific analysis of anything – including human culture – can never be disconnected from the perspective through which it is analyzed. Any discipline that seeks to account for why reality is as it is immediately hits upon the problem of human finitude against the apparent infinitude of the world: life hands us a seemingly endless multitude of events, and any object will keep presenting itself to us as infinite. We have to identify meaningful wholes in reality, which we do through *Wertbeziehung*, of which the purest instantiation is the ideal type.[171]

It is worth pointing out that, in spite of the differences between the neo-Kantian tradition that Weber has inherited here and the hermeneutical perspectives that will inform the last chapter of this thesis, there are striking similarities between Weber's views on historical understanding and that of Hans-Georg Gadamer (which we will discuss in section 8.2.2). Both see understanding as something that can be achieved only indirectly, mediated by historically relative languages or concepts, and connect this view to the historically conditioned and finite nature of our understanding – which is no less finite when it comes to understanding of historical human culture.[172] (In this sense Gadamer and Weber are closer to each other than to, for instance, Wilhelm Dilthey.)

Weber treats one kind of interpretive knowledge as a special case: 'rational' interpretation through the categories of 'goal' and 'instrument'.[173] This is a sensitive point in our discussion of his thought, since he seems to flirt here with the idea that rational and

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[168] Weber (1917-1918, 465).
[169] Weber (1903-1906, 45-47).
[170] Weber (1903-1906, 122-124). On this, see also Bouterse (2014).
[171] Weber (1904, 189-202).
[172] Cf. also Kedar (2007) and his interpretation of Weberian ideal types as in line with philosophical hermeneutics, because of the mediating position of these ideal types in a dialogue between the interpreter and the interpreted phenomenon.
[173] Weber (1903-1906, 128-129).

irrational action differ objectively in such a way that they require different treatment, an idea that has become discredited in more recent debates about the study of science.

This issue hinges on the following question: if rational action can indeed be understood better than irrational action, is this because it is *valid* or because it *resembles* our own attitudes more? To clarify: if we fancy ourselves to know what the objectively rational course of action in a certain situation is, the ideal type of rational action is both something we consider to be objectively valid and something close to what we would do if we could freely act in the same situation. The question is which of these two attributes is relevant to our causal explanations. Is rational action, according to Weber, explained by its own validity, or is it 'merely' more readily understandable to us for pragmatic reasons?

I believe the latter option is the case. In his essay on value judgments and science, Weber confirms that the validity of judgments of rationality is different from the validity of value judgments, but also deals with "the place of the rational within empirical disciplines".[174] He says that "when the normatively valid becomes an object of *empirical* investigation, it loses – as object – its normative character: it is treated as 'being', not as 'valid'."[175]

Weber's example will clarify his point. When the work of an accountant becomes of scientific interest, our own familiarity with the multiplication table figures in two rather different ways. On the one hand, its normative validity is "of course absolutely supposed" in our own accounting work. On the other hand, when we want to say whether the accountant has used it 'rightly', its status is immediately different: we treat it

> as a factual rule of behavior, that one habitually uses as a result of education […] That it is normatively 'valid', i.e. that it is 'right', is in this case, where its usage is the 'object', beside the discussion and logically completely indifferent.[176]

That this is, like the validity of Chinese astronomy, a matter of objective rather than subjective validity does not make a difference to Weber. The empirical fact that a mathematical rule is applied is completely independent of the fact that the normative validity of mathematical rules is "the a priori of all and every empirical science".[177]

In the study of mental phenomena, this distinction can be lost sight of, precisely because 'right' thinking is more readily understandable to us than 'wrong' thinking.[178] But that a thinker solves a problem in a way that to us seems self-evidently 'right' should not lead us to believe that the normatively valid functions in our explanations *as* right; it functions rather as "an instrument of 'understanding', in precisely the same way in which purely *psychological* 'empathy' (*Einfühlen*) provides this understanding with respect to

[174] Weber (1917-1918, 492).
[175] Weber (1917-1918, 493).
[176] Weber (1917-1918, 493).
[177] Weber (1917-1918, 494).
[178] Weber (1917-1918, 494).

logically irrational relations of feelings and affects".[179] Our access to the normatively rational is part of our causal historical explanation of past behavior no more than our access to emotions is; it only provides us with a way of constructing the object of our research.

Weber shows us that it is possible to believe in the universal validity of certain kinds of reasoning, while this validity still disappears from historical explanations. Even the strongest belief that our own ways of reasoning are the only valid ones, in combination with the observation that a past scientist or school obeys our sacred standards to the last detail, does not take away the slightest bit from our duty to explain causally – by citing the individual influences that work on this individual scientist or school (i.e. by referring to context) – what this scientist or school does.[180] The corollary of this is that relativizing our own ways of reasoning or, in general, our own way of doing science, does not equip us any better for a contextual study of past science.[181]

In the next section, we will employ this interpretation of Weber's thought to assess the promises of nature-and-rationality-based inevitabilism in the views of Robert Merton.

4.2.3 Nature and Rationality II: Robert Merton and the Normative Structure of Science

In accounts of the development of science studies, Merton often figures as a representative of a traditional or 'received' view that is challenged for its reliance on internal-external distinctions,[182] its normative demarcation of science from non-science and its corresponding monolithic and uncritical view of science,[183] as well as its 'traditional' opinion that in science "competing claims to validity are settled by the universalistic facts of nature which are consonant with one and not with another theory".[184]

Merton's methodological papers tend to complicate these judgments. In fact, Merton is rather suspicious of attempts to regard the development of science and

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[179] Weber (1917-1918, 495).

[180] Cf. the controversy between Dray (1963) and Hempel (1963) on the question whether the rationality of an action explains that action – Hempel, of course, taking the position that it did not (that is, not under the conditions stipulated by his covering law model), unless the rationality of the agent was mentioned separately in the explanation (i.e. something like: it was rational to do X in situation C, agent A was in situation C and was rational; agent A did X). Cf. also Gutting (1984, 97-99) for a similar point about the Strong Program, and Laudan (1977, 165-167) on the role of normative evaluations of rationality in historical explanation.

[181] This means, for instance, that in the controversy between Roll-Hansen (1980) and Barnes (1980) concerning the dispute between biometricians and Mendelians, we could agree with Roll-Hansen (1980, 514) that "the dispute was solved in full accordance with the rationalist view of science", while also agreeing with Barnes (1980, esp. 689-694) that re-evaluating the *explanandum* as rational or irrational does not alter the explanation. See Friedman (1998, esp. 242-245) on the question under which conditions SSK can be considered to be in competition with rationalistic philosophical approaches to science.

[182] Shapin (1988, 594-597); Shapin (1992, 336-337).

[183] Taylor (1996, 58-60); Hands (1998, 702-707).

[184] Leonard (2002).

technology as autonomous;[185] science can mimic the caste structures of its cultural context, and even suppress, as a result of societal influences, the universalism that normatively defines it.[186] Moreover, he regards his own delineation of science from non-science not as favoring externalist abstinence with regard to science itself,[187] but as bringing sociological understanding closer to the core of science by subjecting the "social and cultural structure of science itself" to sociological scrutiny, thus overcoming some of the restrictions of other, externalist approaches.[188]

The rise of science, Merton says, was itself dependent on certain cultural and material conditions, such as Puritan ethics and the perceived economic and military uses of science. Nonetheless, other conditions could have done the same, and – similar to Weber's thought on the relation between Protestant ethics and capitalism – after science became sufficiently 'autonomous', it relied less on these external sources of legitimacy.[189] In the end, it is true that Merton identifies science not by its status in society, or the things it studies, or as a tradition, but as an ideal-type, defined by its values and embodied in practice by the mechanisms which can plausibly be regarded as realizing these values.

What does this mean for the role of the world in history of science, and the way in which this relates to sociological study? The most explicit claim about this in Merton's work, about competing claims in science being settled by "the universalistic facts of nature", stands alone and has a complex history. As Cole shows, Merton added this remark as a note to his famous paper on the normative structure of science, then adapted it for the collection of his methodological papers to say: "sooner or later, competing claims to validity are settled by universalistic criteria."[190] On the one hand, this editing history suggests that Merton consciously takes position as an inevitabilist; on the other hand, the role of nature in the decision of scientific controversy remains somewhat ambiguous.

Here our considerations concerning the relation between ideal types and explanation in Weber's thought may plausibly fill in the gaps. The identification of science by its 'normative structure' means that we, hermeneutically speaking, have access to what science is, independent of the question of its causal integration within society – we recognize it because it resembles what we regard as science; or, to be precise, because it functions to realize the same ideals that we regard as being constitutive of science. If this reading is plausible, the thesis that competing claims are in the end settled by "the universalistic facts of nature" or "universalistic criteria" is the claim that a fully spelled-out ideal-type of science will be so clear and distinct that it contains norms that settle any debate once all the evidence is available.

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185 Merton (1945, 39); Merton (1970, ix-x); Merton (1941).
186 Merton (1942, 270-273).
187 As e.g. Callon and Latour (1992, 356-357) assume.
188 Merton (1977, 20-22); Merton (1945); Merton and Barber (1963).
189 Merton (1970, xxii). See also Merton (1938).
190 Merton (1942, 271n6); Cole (1992, 4).

One objection to this is that the problem of underdetermination (see section 5.3) renders this impossible in principle; and even if Merton thinks it possible, he has certainly not spelled out a model that provides such specific norms. But more important to our current concerns is that even if the ideal-type of science contains a set of algorithms that makes theory choice trivial, the point remains that whenever scientists or scientific communities in practice follow this algorithm, their actions still require a full causal explanation.

This means that scientific norms in Mertonian sociology come with the same limitations as rationality in Weberian sociology. They may, in a much richer model than either Merton or Weber actually provides, plausibly be regarded as rendering, in conjunction with input from the world, the output of science inevitable; but this becomes the near truism of saying that the more the conjunction of evidence and processing of the evidence of others approaches our own, the more inevitable it is that their results will resemble ours as well.[191] What the inevitabilist gains from this is the possibility to say that rationality, or the normative structure of science, has multiple ways of being instantiated in a society. But its instantiation still needs to be causally explained, and there is no general reason why the factors contributing to this instantiation could not, historically speaking, have been different.

There is, to put it in the most succinct terms, no route from 'normative inevitabilism' to 'causal inevitabilism'. A defense of inevitabilism saying that input from nature plus some model of rationality renders scientific content inevitable only moves the contingency from this content to the instantiation of this model of rationality. This is no futile exercise, for the ways in which input from the world is processed in science may be more readily hermeneutically accessible to us than the results of this processing; but it does not support a thesis of historical inevitability, since the more demanding the notion of scientific rationality becomes, the less historically inevitable science is – and the more scattered it will be.

4.3 The Logic of Ideas

4.3.1 The Early Decades of the Journal of the History of Ideas

An ahistorical rationality cannot be employed to support inevitabilism. But a more 'historicized' way of talking about reason in history is also conceivable. What if there is an inevitable logic to the development of scientific ideas, but the right or scientific way of thinking itself gets realized only over time? In that case, after all, there may still be an inevitable end-point to the development of science, but the history of this development may turn out to matter in some real way.

Where could we find representatives of such an 'idealist inevitabilism'? We will look in two places. In the next section (4.3.2), we will focus on the work of Alexandre Koyré.

[191] Hacking (2000a, 64-66).

In the current section, we focus on the *Journal of the History of Ideas* in the 1940s and 1950s, which in those decades was an important locus of activity in history of science.[192] Out of around 430 regular articles, about 60 were devoted to subjects closely related to the history of science, and the intersection of history of ideas and history of science makes these volumes an interesting source for studying a possible affinity between idealism and inevitabilism.

The founding editor of the journal, Arthur Lovejoy, uses his opening article to maintain, among other points, that history of ideas can survive the recognition of human irrationality. Indeed, he says, it "would be a misconception to suppose that the intellectual historian is concerned solely with the history of intellection."[193] But people are not therefore driven only by non-rational or social forces, which Lovejoy more or less equates; rather, there are "two types of factor" at work in the history of thought, whose respective influence can be measured.[194]

Lovejoy manifests confidence here in the possibility of abstracting intellectual factors from the flow of history;[195] but this falls short of an idealist reductionism. He explicitly states that he does not believe in the "working of an immanent dialectic whereby ideas are progressively clarified and problems consecutively get themselves solved";[196] the intrusion of psychological or sociological factors makes sure that the history of philosophy is not one "in which objective truth progressively unfolds itself in a rational order."[197] Somewhat mysteriously, however, and without explanation, Lovejoy exempts "the domain of strictly experimental science" from his anti-inevitabilist remarks.[198]

The articles in the *Journal* are united primarily in their presupposition that a focus on 'ideas' in the history of science is in some sense helpful; the *Journal* obviously does not force a doctrine with regard to historical causality upon its authors, and many authors feel comfortable zooming in on the social aspects of the history of ideas – emphasizing the importance of scientific societies, of social types corresponding to different attitude towards science, or presenting a "socio-statistical" study of reactions to Darwinism.[199] A study by David Joravsky applies the specific drives towards cultural revolution and collectivized agriculture to explaining the course of Soviet biology before Lysenko.[200] In one article, Edgar Zilsel develops his famous thesis that the development of modern science in the West must be seen as a "sociological process" resulting from the interaction of craftspeople and

192 Grafton (2006, 16-17).
193 Lovejoy (1940, 16-17).
194 Lovejoy (1940, 18).
195 Cf. Kelley (1990, 12-14).
196 Lovejoy (1940, 20).
197 Lovejoy (1940, 21).
198 Lovejoy (1940, 20).
199 Johnson (1940); Houghton (1942); Ellegård (1958, 379-387).
200 Joravsky (1959).

scholars – experiment and theory meet each other when manual labor and scholarly intellect meet.[201]

How do the authors usually talk about the relation between ideas and experiment? Can experiments make a real difference, and how does their role relate to intellectual factors? Thomas S. Hall's 1950 study of 'the scientific origins of the protoplasm problem' starts with describing a 19th-century experiment – Dujardin's crushing the membrane of a single-celled animal – "which was to have far-reaching consequences for the future of biology",[202] but then moves on to trace the idea of a 'protoplasm' in Presocratic thought, rather than regarding it as the result of the aforementioned experiment. Aram Vartanian connects experiments and concepts more elegantly, by showing how the observed features of a polyp by Abraham Trembley shapes a range of 18th-century debates concerning materialism; thought about conceptual matters is rather directly triggered by observational input,[203] and on occasion, the polyp figures almost as a Latourian actant, "favoring" materialist ideas.[204] It does so mainly through altering the shape of the most rational conceptual system – it has materialist "implications".[205] Nonetheless, from the perspective of the pre-existing system, the relevant observations are obviously contingent – this is not an example of an irresistible inner logic in the history of scientific thought.

Stephen Toulmin, on the other hand, attacks the notion of a crucial experiment; this is a self-justifying notion, and a misleading one at that, as he demonstrates through Lavoisier's red calx of mercury experiment. The experiment was originally Priestley's, and far from providing "irresistible proof that the calx is compound not an element",[206] Toulmin maintains that Priestley could consistently deny this conclusion,[207] and that one experiment cannot autonomously be logically crucial, though it may be crucial in a causal sense or given theoretical assumptions. There is a certain priority of concepts here, but on their own, these concepts don't have an irresistible internal logic to them.

We do find a hint of the views we are looking for in E.W. Strong's account of Whewell's controversy with Mill about science, where Whewell defends the thesis that "*there are scientific truths which are seen by intuition, but this intuition is progressive*";[208] this is a defining feature of idealist inevitabilism, but though Strong is explicitly sympathetic towards Whewell in this debate, he believes that Whewell's strongest point against Mill, that conceptions are formed by minds that are historically conditioned, "stands whether one

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201 Zilsel (1941, quote on page 25).
202 Hall (1950, 339).
203 Vartanian (1950, 262).
204 Vartanian (1950, 279, 285).
205 Vartanian (1950, 284).
206 Toulmin (1957, 206).
207 Toulmin (1957, 214).
208 Strong (1955, 221).

accepts or rejects his contention that intelligible relations (*a priori* Ideas) are progressively disclosed."[209] A strong, reductionist idealism is hard to find in the *Journal*.

The evidence with regard to inevitabilism is ambiguous as well, and mostly indirect: Charles Nauert uses teleological language when he says that Agrippa's work approaches the idea of hypothesis and testing – "his mind is tending in this direction".[210] While this can be a merely rhetorical device, Maurice Mandelbaum explicitly talks about scientific problems which "paved the way" for evolutionism, and Darwin's *Origin of Species* presenting a view "toward which [multiple] scientific theories had tended to converge".[211] He also claims elsewhere that "all that prevented Darwin from denying the truth of Theism was the anti-dogmatic cast of his own mind, and his acceptance of the limitations of all human minds, considering their lowly origins",[212] which can be considered to be an affirmation of the internal 'logic' of his ideas but obviously on a very local and contextually defined level.

Alvar Ellegard talks in inevitabilist terms about Darwinism on a larger level, saying that once certain empiricist methodological principles were established, "the Darwinian theory could hardly be resisted" – and that it was impossible to accept Darwinism and leave traditional religion intact.[213] Supposedly inevitable clashes between science and religion are, by the way, not representative of the way of thinking about science and culture in the *Journal*.[214] Particularly interesting in this respect is an article by Norton Garfinkle on the reception of the work of Erasmus Darwin in England, which he argues alters significantly after the French Revolution: the Revolution ends a period of tolerance in Britain, and indirectly leads to a new emphasis on Biblical literalism, and harsh criticism of scientific writings such as Erasmus Darwin's *Zoonomia*, that are perceived as subversive of traditional religion.[215] Like many articles, this relates science directly to social, political and cultural factors that are obviously contingent with respect to what would be its own internal development.

The clearest inevitabilist argument with respect to the content of science is Elizabeth Gasking's 'Why was Mendel's Work Ignored?'. Gasking explains why it was no wonder that most biologists ignored Mendel in an intellectual climate that focused on processes of selection rather than of variation,[216] and why those who did read it were, understandably, unconvinced after Mendel turned his attention to trying to experiment with *Hieracium*. Understandably, for "in retrospect, we can see why the work was so fruitless":

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[209] Strong (1955, 231).
[210] Nauert (1957, 179).
[211] Mandelbaum (1957, 361).
[212] Mandelbaum (1958, 378).
[213] Ellegård (1957, 392).
[214] Strong (1952;) Kocher (1950).
[215] Garfinkle (1955, esp. 380-383).
[216] Gasking (1959, 74).

the *genus* was "variable and atypical in its methods of reproduction"[217] and the experiments on these plants therefore failed to reproduce the elegant fractions of his pea experiments. Mendel "could not recognize the deceptive character of his results, and must have been very disappointed at finding this apparent refutation of his earlier theories."[218]

Gasking's judgment is, in the end, mainly that of prematurity (she does not use the term): "science is organized knowledge, and no piece of work, however complete in itself, is valued until it can be fitted into the general corpus. Given the position in biology when Mendel wrote, it was perhaps inevitable that his discovery should not have been appreciated."[219] Mendel's faith that his time would come was proven right in the end, when work on crytology and biometrics made it possible for Mendel's theories to be understood – though "paradoxically it seems that, had Mendel's work been lost forever, modern genetics would nevertheless be much the same today, and that the rediscovery of his monograph had at most the effect of aiding and speeding up the birth of the subject."[220]

We recognize this as far from a paradoxical claim. Rather, it is a coherent affirmation of nature-based inevitabilism: it is the way inheritance generally works in plants that made Mendel right. We saw this confirmed by the claim that it was the fact that he happened to stumble upon an exceptional and non-representative case that delayed the acceptance of his main findings. We can recognize this fact from a present-day perspective that has inherited a biology that eventually embraced Mendel's findings. That Mendel's laws would eventually be accepted anyway is in harmony with the implied claim that his ideas were premature – the assertion of prematurity, after all, can only be made if the later or final state of the system in which the premature findings is inevitable to such a degree that there is no plausible historical development of science in which the 'premature' findings never find a home.[221]

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217 Gasking (1959, 75-76).

218 Gasking (1959, 76).

219 Gasking (1959, 77). The 'inevitability' here is obviously very local and refers only to the determined nature of Mendel's rejection by the biological community in his own time.

220 Gasking (1959, 79).

221 According to the main theorist of prematurity, Gunther Stent – who uses Mendel as a primary example of the phenomenon – prematurity in scientific discovery can in fact be identified synchronically based on the question of whether its implications can be logically connected to contemporary canonical knowledge (Stent [1972]). Ernest Hook (2002, 9) has more recently maintained that Stent's notion of prematurity is not Whiggish if simple hindsight does not amount to Whiggism, and claimed that the concept remains helpful (9-11). However, Stent's synchronic definition of prematurity is too broad: it would also involve sightings of ghosts and extraterrestrial presence on earth, of which it is also true that they do not presently fit well with accepted scientific knowledge. If the concept of prematurity implicitly only applies to beliefs which are true – so that false beliefs are never 'premature' – it seems to be Whiggish in a stronger sense than just embracing the benefit of hindsight: it would mean that the non-acceptance of true beliefs requires more explanation, and of a different kind, than the non-acceptance of false beliefs. Hook connects prematurity to 'delay' in the recognition of scientific claims, which is clearly diachronic; the point is now that the notion of 'delay' only makes sense under the assumption that recognition was, in the end, inevitable.

Can we draw some general conclusions from all this? One is that though few articles make explicit statements about contingency or inevitability, we have accumulated some indirect evidence that the founder of the *Journal* and many of its contributors thought that the development of science is, on a longer time-scale, not contingent. It is less clear what they consider to be the chief explanation for the degree of inevitability that science has, and on the basis of what we have discussed, it is unlikely that the answers would be strongly idealist. The most clearly inevitabilist articles were inevitabilist about a specific theory based on specific evidence from nature.

Another thing that these articles teach us is that questions of contingency and inevitability can arise on any scale and with regard to many questions: very often, we are not simply contingentists or inevitabilists with regard to science as a whole, but with regard to the question whether, for instance, Mendel's theories would eventually come to be accepted, or whether 19th-century evolutionary theories would inevitably clash with religious beliefs. It is possible to be an inevitabilist about either of these questions while being a contingentist about the other, which makes it problematic to treat the articles to the *Journal* as evidence for the same debate.

This also brings to light another corollary of the way in which we defined contingentism and inevitabilism in chapter 2, namely that the question of the degree of contingency is always posed against implicit or explicit historical background knowledge and, in principle, temporally delineated. It is: *given* that these facts were in place at time t_1, was this aspect of the outcome at t_2 inevitable or does it obtain only under some of the historical paths that were possible from t_1? This question can be posed on any scale, with any level of detail of background knowledge and with respect to any aspect of any outcome.

A global inevitabilist thesis would be that at some time in the past, most or all our current scientific beliefs were already almost inevitable, in the sense that from that point there are very few historical paths leading to 21st-century scientific beliefs being radically different from the actual ones. Most professions of inevitability in the *Journal of the History of Ideas* are locally defined – they suggest that in a certain context, it was inevitable that a certain finding would impact scientific ideas in a certain way.

The question remains meaningful whether we can find a global inevitabilism about the content of science based on idealism, and what that would look like.

4.3.2 Alexandre Koyré's Intellectualist Inevitabilism

The historian of science whose work comes closest to this position is Alexandre Koyré, whose perspective on the history of science has been characterized as idealist,[222] as Platonist,[223] and as Hegelian;[224] but we need to be careful with these characterizations, since,

[222] Hall (1987, 489).
[223] Elkana (1987, 128-139).
[224] Stump (2001).

as Yehuda Elkana has put it, "neither Koyré, nor Plato for that matter, fit the caricature of a historian of disembodied ideas."[225]

One corollary to Koyré's 'Platonic' tendencies is that it is sometimes hard to be certain, not about what is being said – Koyré is never obscure when it comes to the content of thought – but who exactly is saying it and how seriously we are to take it. Koyré writes not without humor and irony, and though he is often quick to point out where his early modern natural philosophers make mistakes, he is also very good in thinking along with them and pushing their thought just a bit further towards what he considers to be its unavoidable implication. So when he says that 'good physics is done *a priori*', what exactly is the status of that remark? Is he Galileo's mouthpiece or is Galileo his?[226]

In general, however, Koyré leaves little to guess about the direction in which his work points; and that is in the direction that scientific accomplishment needs to be understood as an accomplishment of the human mind – and an historical accomplishment at that, since "modern science did not spring perfect and complete, as Athena from the head of Zeus, from the minds of Galileo and Descartes."[227]

The scientific revolution was not just a matter of 'discovery'; the scientists that accomplished it "had, to begin with, to reshape and re-form our intellect itself."[228] For us, standing on the other side of this effort, it is hard to see; Galilean motion seems so 'natural' to us that "we even believe we have derived it from experience and observation", which obviously we have not.[229] We need to overcome the self-evidence of our current notions when doing history; not those notions themselves – Koyré will be the last to forego the privilege of telling past natural philosophers, on the basis of his own understanding, where they erred[230] – but their supposed 'naturalness'. We need to see that our having this understanding is the result of history.[231] History shows us:

> how difficult it was even for such revolutionary minds as Galileo and Newton to free themselves from the conjoint influence of tradition and common sense, to draw – and to accept – the inevitable consequences of their own fundamental concepts.[232]

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225 Elkana (1987, 131).
226 E.g. Koyré (1939, 166); Koyré (1943b, 42).
227 Koyré (1943a, 1).
228 Koyré (1943a, 3).
229 Koyré (1943a, 20).
230 E.g. Koyré (1961, 40); Koyré (1957, 54).
231 Koyré (1961, 18).
232 Koyré (1955, 329). Cf. ibid., 343. Pointing out the errors in natural philosophical works is not an exercise in pedantry; it is an essential ingredient to the unfolding of modern science. As Mario Biagioli (1987, 179-180) has said, "it is a history-dependent and man-made error that, according to Koyré, is dialectically synthesized during the *cheminement* of science". It is in this context, too, that we can understand Stump's (2001, 245, 258) thesis that Koyré should be classified as 'Hegelian'.

Modern science is not natural, but it is inevitable. The key to interpreting Koyré is seeing that he opposes reason to nature; the rise of modern science is the victory of thought over experience and imagination. Thought could not have reached any other conclusions than it did, but it is not natural to think, or to let one's opinions be guided by thought alone. Koyré shows how the revolutionary scientists of the early modern period gradually succeeded in doing this.

To flesh out this interpretation, we look at what Koyré has to say about the role of observation in science. He distinguishes between observation and experimentation, because "*simple* observation, the observation of common sense" was only an obstacle to modern physics.[233] Experiment – "the methodical interrogation of nature"[234] – did play a positive role, but this implies a *language* in which nature can be interrogated, and this language "could not be determined by the experience which its use was to make possible. It had to come from other sources."[235]

Even when conceding a positive role for experiment, Koyré is always happier when he can diminish its extent. When he notes that "none of the numerical data invoked by Galileo [in the *Discorsi*] relates to measurements actually made", he makes sure that the reader understands the compliment: "I do not reproach him on this account; on the contrary, I should like to claim for him the glory and merit of having known how to dispense with experiments."[236] Nonetheless, he goes on to list the essential functions that experiment *does* fulfill.

Only experiment, he says, can provide numerical data that complete our knowledge of nature. Only experiment can select the right means to a certain purpose. Only experiment can ensure that "matters take place in tangible reality, *in hoc vero aere*, very nearly as they do in the Archimedean world of reified geometry on which our deductions are grounded", though in the case of fundamental laws of nature pure reason suffices in principle.[237] The function to which he devotes the most attention is:

> pedagogic […] it was experiment that pointed out the inadequacy of Aristotelian doctrine with respect to reality, and which, as much as its inherent contradictions, convinced Simplicio that it was wrong. […] No doubt, the arguments and 'experiments' adduced by Galileo are sufficient for enlightened minds free from prejudice, as represented by Sagredo. But what about the others? For them, something more is required, namely, a real experiment.[238]

[233] Koyré (1939, 2). Cf. Koyré (1943b, 18).
[234] Koyré (1943b, 18).
[235] Koyré (1943b, 19).
[236] Koyré (1960, 75).
[237] Koyré (1960, 76).
[238] Koyré (1960, 76).

This may be an instance of Platonic irony, and it is ambiguous to which extent this is Koyré talking or his Galileo; but the gist is that experiment does not get to the core of science, and that it serves mainly as a teaching tool for the mind.

In fact, Galileo's Salviati is "so good a midwife of the brain" that he can even teach Simplicio without experiment, like Socrates did the slave boy in Plato's *Meno*, for "men are already in possession of the true principles of the nature of the physical world even in advance of any experiment. Men know the truth, though they may not be aware of this."[239] Koyré's commitment to this Galilean view remains unwavering when he discusses other heroes of the scientific revolution. Copernicus, too, meditates and makes calculations rather than observations; his superiority is mathematical, though of course "the Sun, Moon and planets are real objects".[240]

The ability of human reason to access scientific truths without experience means that the world needs to be of such a structure that thinking about it can give us gradually more insight into it. We can see this in recent discussion in philosophy of science concerning the meaning of thought experiments; James Robert Brown has asserted the existence of 'Platonic thought experiments', which generate a theory which is better than its predecessor without being based on new evidence or logically derived from old evidence. Galileo's thought experiment concerning the rate of fall of bodies of different weights meets these criteria.[241] Thought experiments enable us to grasp relations between universals, of which natural laws are instances.[242] This means, Brown maintains, that thought experiments can and do transcend empirical knowledge by giving us *a priori* knowledge of nature – of the real phenomena, rather than the appearances.[243]

According to James W. McAllister, thought experiments are valuable only under the assumption that reality consists of universal and stable phenomena – an assumption to which Galileo did and Aristotelians did not adhere.[244] Brown's view is an instance of such a metaphysics, which is more explicit about it than Koyré but which matches his interpretation of Galileo's thought experiments. Brown too prefers to see Galileo as a rationalist rather than the brilliant experimenter that he is according to Stillman Drake.[245]

Koyré himself has dealt with "the use and abuse of imaginary experiment" in an article on Galileo's treatise 'De motu gravium', where he tries to "describe and justify Galileo's use of the method of imaginary experiment concurrently with, and even in

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239 Koyré (1939, 167).
240 Koyré (1961, 24).
241 Brown (1991, 99-101).
242 Brown (1991, 118).
243 Brown (2004) in controversy with Norton (2004). Cf. Brown (2013) 62-65.
244 McAllister (2004, 1164-1165). Cf. also McAllister (2005, 35-56).
245 Brown (1991, 122). My argument does not deal with the plausibility of either interpretation, but see the critical discussion by Hall (1987, 485-496): Hall's point that "the true course of the evolution of major innovations is not to be deduced solely from their author's narrative in print" (488) seems well taken.

preference to, real experiment",[246] adding in a footnote that thought experiment "plays a part intermediate between pure thought and tangible experiment".[247] It could go wrong, however, and sometimes it did, even in the case of Galileo; but that was only because of its inherent risk to "go to the extreme in putting ideas into concrete form".[248] To Koyré's taste, even thought experiments can be too close to experience! In that case they cease to teach us something about the real world; Koyré gives an example where Galileo's Salviati makes a mistaken prediction about what happens when water and wine react, a mistake that he diagnoses as resulting from Galileo's taking the liberty to *imagine* on the basis of confused experience.[249] Scientific genius, we know by now, consists precisely in replacing imagining with reasoning, for "it is […], pure unadulterated thought, and not experience or sense-perception, as until then, that gives the basis for the 'new science' of Galileo Galilei."[250] Here too he concludes: "Good physics is made *a priori*. As I have already said, it must at all costs avoid the temptation and fault of extreme concretism, and must not allow imagination to take the place of theory."[251]

Where can we place Koyré on the spectrum of contingentism and inevitabilism, and how does this relate to his views about how the world constrains scientific theory development? In Koyré's metaphysics, access to real nature is realized progressively, in a historical process in which the mind errs a lot – immersed as it is in the world of becoming and experience. The development of science is inevitable, but the world does not exercise a causal influence upon our beliefs; rather, it possesses a structure that we progressively manage to grasp rationally. *If* we think, we can come to but one conclusion.

There is a tension between the fact that Copernicus, Galileo, or Newton could not have come up with different systems that were equally true – for their systems are no speculations about possible worlds, but hook on to the one real world – and their creative genius; the "power and boldness" of Copernicus' mind, for instance.[252] But the aim of history of science is not just to affirm the teleology of history on the macro-scale, but also to understand its contingencies on the micro-scale. We are in a complicated hermeneutical position in this respect: on the one hand, we have better knowledge now of the same universal relations that our forefathers were looking for, and this knowledge genuinely

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[246] Koyré (1960, 82).

[247] Koyré (1960, 82n1). This confirms Floris Cohen's (1994, 81) reading of Koyré that "a certain balance between the mathematically abstract and the world of experience is indeed necessary", where most of the time it seems to me that this balance tilts rather heavily towards the abstract and away from experience.

[248] Koyré (1960, 82).

[249] Koyré (1960, 84). See also McAllister (2013, 11-29) about the relation between thought experiment and imagination. Galileo, according to McAllister, has little room for the imagination – but this is also because his thought experiments "did not extend observation, as imagination is supposed to do, but confirmed it" (21).

[250] Koyré (1943a, 13).

[251] Koyré (1960, 88).

[252] Koyré (1961, 54).

enlightens their endeavours precisely because they were looking for those relations. On the other hand, the self-evidence with which *we* hold these notions obscures the enormity of their task, and thus our understanding of the historicity of the human achievement.

For the goal is ever and always to understand; to understand why the geniuses and less-than-geniuses of the scientific revolution thought and imagined as they did.[253] The depth of the *docta ignorantia* of Cusanus; the vitalistic and magical optimism of Bruno – these persons, in Koyré's historiography, are in no sense passive tools of some inner logic, and in this respect he does not fit the caricature of a historian of disembodied ideas.[254] But he is consistent to the borders of caricature in the extent to which he discards every hint that what his subjects think and believe can be systematically understood by reference to anything else than the workings of their own mind and its (admittedly fallible) pondering of reality.[255]

In *The Astronomical Revolution*, Koyré characteristically uses one of the heroes himself as his mouthpiece, in this case Copernicus:

> Were we to persist in trying to make him understand that […] we are trying to explain his work and genius by his 'nationality' and his 'race', he would probably become annoyed […] he would say that all historical, Marxist or sociological conceptions by which man, or a work, of genius may be 'explained' by heredity, race, class, position, background and a particular moment in history, could only have been invented and maintained by barbarians completely devoid of philosophical upbringing.[256]

This statement is a point of metaphysics – as we have seen, it fits within Koyré's Platonist-inevitabilist framework where contingencies cannot sustain genuine truths, and necessary steps in the history of thought cannot have trivial material causes[257] – but in its emotional charge and its political-ideological implications, it is also a question of hermeneutics: it is a plea to take seriously the conscious intentions and motives of the actors that we seek to understand, especially if they are of such individuality and genius that not to do so would

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[253] Koyré's individualism in this respect is also a reason to be skeptical of Stefan Amsterdamski's (1987, 113) suggestion that "pour que les idéaux du savoir issus de l'évolution autonome de la pensée puissant determiner le cheminement du savoir scientifique, ils doivent être acceptés et institutionalisés. Cela depend d'une multitude de facteurs historiques et sociologiques, circonstanciels par rapport à cette logique autonome de la pensée". For Koyré, it seems that thought takes place in the minds of individuals, and that there is no good in attempts to socialize it, even in the 'context of justification'.

[254] Koyré (1957).

[255] See e.g. Koyré (1956a).

[256] Koyré (1961, 19-20).

[257] In this sense, it is the same point that Koyré also makes when he says that "many of the external influences which historians have called turning-points in the history of science are completely illusory. The appearance of the cannon did not cause the emergence of the new dynamics […]. However, though the series of events constituting the evolution of mathematics, astronomy and physics cannot be explained in isolation – it is always vain to 'explain' an invention or a discovery – they can at least be made intelligible." (Koyré' [1964, 11]). On the lack of a role for contingent knowledge in Koyré, cf. Hall (1987, 495).

be a barbaric injustice to them. "The social structure of England in the seventeenth century cannot explain Newton, any more than the Russia of Nicholas I can throw light on the work of Lobachevsky, or the Germany of Wilhelm II enable us to understand Einstein. To look for explanations along these lines is an entirely futile enterprise."[258] Futile, and an affront.[259] In the next section, we will have a look at some of the barbarians who had the audacity to try this.

4.4 The Final Stage of Society

4.4.1 Some Preliminary Considerations on Marxism

Finally, we will discuss some Marxist perspectives on the determining factors in the history of science. More than most philosophies of history, classical Marxism is characterized by the assertion of inevitability.

Marxism famously sees as the driving force of history the circumstances and ways of production: there is an internal, progressive dialectic in the collective relations between man and the world. Understandably then, science plays an important role in Marxist thought: it is science through which we come to grips with the world with which we have to work. As such – and we will see this view in practice shortly – science is inextricably intertwined with modes of production, and is therefore of world-historical significance. However, this does not decide the question precisely what its role is: we can imagine science to be part of a superstructure, *following* modes of production, perhaps even directly reflecting the interests of the dominant classes; we can also imagine science, in giving us progressively more access to and insight into the objective natural world outside of class interest and struggle, to exercise an emancipatory force and thereby further the progress of history towards its final state.[260]

The tension between these two interpretations is genuine, but a skilled dialectician will be able to resolve it in advance, by noting that when historical necessity is fulfilled, class interest and objectivity will coincide, and claiming that there is, in the end, no discrepancy between the emancipatory and instrumental faces of science. There is no contradiction between driving social and historical progress and being driven by it; between providing objective access to an outside world and being determined by forces within human society. Sheehan has aptly summarized the meaning of Marxism for science studies in general, in a way that also applies to our current purpose: "Marxism has made the strongest claims of any intellectual tradition before or since about the socio-historical character of science, yet always affirmed its cognitive achievements."[261] The special attractiveness of Marxist theory

258 Koyré (1963, 856).
259 The same idealism resonates in Richard Westfall's ruminations on an "autonomous realm of the spirit", the neglect of which he finds to be a major shortcoming of Marxist histories of science like Bernal's (Ravetz and Westfall [1981, 405]).
260 Cf. Cohen (1994, 218-219).
261 Sheehan (2007, 197).

is that it seems to do both these things, not by designing a compromise between them, but by doing both completely and at once.

Marxism and history of science intersected with world history in a poignant way in July 1931, when a Soviet delegation under the leadership of the revolutionary theorist Nikolai Bukharin – by then expelled from the highest party positions but still a figure of authority – joined the 'second international congress of the history of science and technology' in London.[262] In his entourage, Bukharin brought the physicist Boris Hessen, who was to deliver one of the most famous papers in the history of science studies.

Bukharin's own paper deals directly with the relation between history and the external world in science, approaching from a Marxist perspective the idea that "'I' have been 'given' only 'my' own 'sensations'".[263] In every individual experience, he says, "there are included beforehand society, external nature and history – i.e., social history."[264] Moreover, the external world relates to society not as simply 'given', but as "the object of active influence on the part of social, historically developing man."[265] Science is simply "the continuation of practice […] by other means."[266]

According to Bukharin, this does not conflict with the idea that in this active relation with the external world, a picture of the world is attained that is "much more adequate to reality than all its predecessors, and therefore so fruitful for practice".[267] He supplements his "practical criterion of truth" with a correspondentist one;[268] science is historical, but it also becomes more adequate to how nature objectively is. This is consistent with other writings of Bukharin: elsewhere, he explains that the all-pervasive state power of the proletariat does not mean that objective laws disappear under revolutionary voluntarism. Nature "always remains a realm of necessity."[269] Scientific Marxism recognizes this necessity; "science itself would be objectless if there were not objective laws".[270]

This also suggests how the relativity of cognitive activity to class structures – easy to uphold when it concerns bourgeois science – behaves under pressure: is science still class-relative under proletarian rule? The answer can be: yes, being does not stop determining consciousness. Or: no, because in this case the *objective* laws are visible, underneath which there is nothing left to discover. But then, knowledge of these objective laws becomes an

262 Bukharin (1931a).
263 Bukharin (1931b, 11).
264 Bukharin (1931b, 12).
265 Bukharin (1931b, 16).
266 Bukharin (1931b, 20).
267 Bukharin (1931b, 21).
268 Bukharin (1931b, 15, 18).
269 Bukharin (1935, 83).
270 Bukharin (1935, 84). Bukharin's emphasis on liberty and humanistic values as a necessary counterpart to 'laws of necessity' can also be read as an implicit criticism on the exploitative, despotic policies of Stalinism – as can his statements on the role of science and technology in general (Cohen [1973, esp. 352-374]). Note that 'objective' law can refer to historical development as well as to natural necessity.

instrument of freedom only under the right social circumstances – suggesting a synthesis of these two answers. Access to the objective state of affairs and the liberating status of science go hand in hand.

In a more radical development of Marxist holism, we might expect nature 'itself' – *objective* nature – to develop historically through the same dialectical mechanisms as society does; however, we do not see Bukharin claim this. In general, we will see that even though Marxist theoreticians are generally prepared to say that nature does not present itself to us in any other way than in relation to production, they are not prepared to identify the way in which it presents itself with the way it objectively is. This is understandable, when we realize that doing so would mean that the findings of bourgeois science would *not* be biased, but would rather correspond to the objective world of bourgeois society. Better to say that the validity of science depends still on the world, and that the dialectical historical process simply actualizes the one right perspective upon this world (namely by the class that is destined to inherit it).

4.4.2 Boris Hessen and the Inevitability of Newtonianism

The Hessen paper about the 'social and economic roots of Newton's *Principia*' starts by presenting the perspective Hessen wants to discredit: "just think", he quotes Whitehead about Galileo and Newton, "what the course of human history would have been if these two men had not appeared in the world."[271] Hessen says this quote shows a failure to get behind ideology, and a misleading individualism, which a Marxist perspective removes. Admittedly, Hessen does not underline his opposition to the contingency implied by Whitehead's words, though he does emphasize the determined nature of the Marxist dialectical process.[272]

All ideas can be understood in relation to material productive forces. Only the proletariat, which in the end will rule, "is free from a limited understanding of the historical process and produces a true, genuine history of nature and society".[273] This does not mean that its science becomes disconnected from its material bases – on the contrary, class societies are misguided about nature and history precisely because their "dominant ideas are separated from the relations of production."[274] Science remains material in nature and integrated in society, even and especially in the inevitable classless society.

But how does this integration work? Hessen's paper provides a consistent picture. He talks about the specific technical problems that arise with the disintegration of the feudal economy and the development of merchant capital, overseas trading and large-scale

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[271] Hessen (1931, 151). Freudenthal and McLaughlin (2009, 96n4) provide Whitehead's original quote: "Think for a moment of the possible course of history supposing that the life's work of these two men were absent." When quoting Hessen, I follow Freudenthal's and McLaughlin's edition, in the original page numbering (which they also provide).
[272] Hessen (1931, 152).
[273] Hessen (1931, 154).
[274] Hessen (1931, 154).

industry: for instance, questions of how to increase the tonnage and stability of ships, and how to determine their position at sea. After presenting these problems, Hessen says: "let us consider what physical premises are necessary in order to solve these technical problems."[275] His discussion clearly and consistently implies that there are optimal technical solutions to the problems that arise from the circumstances of production in a given society; but what these solutions are is decided by nature. We understand the interest of early modern science in problems concerning the free fall of bodies, hydro- and aerostatics and celestial mechanics because the actual solutions to the economic problems of the age are to be found there: "the physical problems presented by the development of transport, industry and mining […] are all *purely mechanical problems*."[276] It does not suffice to say that they are *interpreted* as such by the dominant classes; that would leave a gap in the explanation.[277] Rather, Hessen considers the capitalists of the age to be instrumentally rational given the real nature of their problems.

In order to understand their work, we do not need the scientists we study to discern, let alone identify explicitly, the motives for their own scientific endeavours. Relating objective class interests to what nature objectively looks like is enough. That "it would be futile to seek in [the *Principia*] an exposition by Newton himself of the connection between the problems that he sets and solves and the technical demands from which they arose" does not mean that this connection does not exist.[278] It means only that we need to supplement our interpretation of the *Principia* with what we know of this material basis, to see how "the 'terrestrial core' of the *Principia* consists precisely of the technical problems that we have analyzed above".[279] It is our own knowledge of the real, mechanical nature of these technical problems that allows us to make this connection.

Not even Newton's alchemy escapes Hessen's search for a 'terrestrial core'; this too was "closely bound up with the production of necessities, and the aura of mystery surrounding the alchemists should not conceal from us the real nature of their research".[280] Here Hessen's Marxist hunches converge in an interesting way with the findings of recent historiography (see section 8.6.4). Something similar is the case for Hessen's judgment of the

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[275] Hessen (1931, 158).
[276] Hessen (1931, 165).
[277] Freudenthal and McLaughlin (2009, 6) suggest that the fact that an economic need is conceptualized *as* a technological problem is not considered self-evident by Hessen (which is relevant to their claims on the relation between technology and science), but support this claim by means of a quote from Merton. It is closer to the truth to say, with Cohen (1994) 331, that Hessen projects his awareness of the physical background of these technical problems back onto the early modern period.
[278] Hessen (1931, 171). Cf. Freudenthal (2005); Freudenthal and McLaughlin (2009) in their claim that Hessen's argument is not vulnerable to the criticism that the motives of scientists were not utilitarian. Freudenthal and McLaughlin connect this to a new interpretation of Hessen's thesis. The point, they say, is not that science studies particular problems in order to improve technology, but that science develops by studying technology. I suspect their reading fits more comfortably with Grossman's thesis than with Hessen's, who does consider science to develop not primarily on the basis of existing technology, but in accordance with the interests of the ruling class (and within boundaries set by those interests).
[279] Hessen (1931, 171).
[280] Hessen (1931, 172).

relation between Newton's scientific work and his religious beliefs – Hessen finds that "Newton's theological views were by no means a mere appendage to his system."[281] They were rather, in reinforcing the bourgeois view that matter could never move itself (depriving matter of "that inalienable property without which the structure and origin of the world cannot be explained by material causes")[282] a scientifically relevant brake on the inevitable development of science. And a necessary one at that, since in the bourgeois society that had just arisen in England, the time was not ripe for the principle of the 'self-movement of matter' that would come to characterize modern physics.[283]

For if Newton could not yet arrive at the truth of the law of energy conservation, this was "not because he lacked genius",[284] but because this could happen only when the development of the steam engine raised the problem of translating one kind of motion into another, leading to the discovery that all forces could be transformed into each other and helping physics towards "the inevitable conclusion that the end result was the eternal circulation of moving matter."[285]

In the end, Hessen's paper is not about Newton, but about the inevitable progress of science and its relation to the inevitable progress of society; it simply zooms in on Newton to make the case for a Marxist reading of the history of science in what would likely be the most difficult place to make it. But what, in this revisionist picture, determines the content of scientific theories, and how?

Here we must conclude that for Hessen, nature decides what science must look like in order to respond to societal needs. Though nature is itself a product of its own historical development,[286] the symmetry with society ends here: there is no hint that the way in which nature itself develops responds, dialectically or not, to societal development. It is society and, in its superstructure, science that develop dialectically. Both society and nature are present in nature, evidently; but for Hessen – much like Merton – society provides the questions to which science finds the answers in nature. It cannot find complete and unbiased answers until it attains the perspective of the proletariat, but to the extent that it does find these, it is by seeing nature as it really is. Of course, the mechanism through which it succeeds in doing so is a logic of development within the material relations to nature; but in these relations, nature remains forever the object – it is man who proceeds from being an object to becoming a subject as well.[287]

Hessen's thesis stands out as an early and elegant way to look at science as importantly social in nature. Even if it does not provide a genuinely innovative perspective on the relation between science and nature, it provides an awe-inspiringly innovative

[281] Hessen (1931, 185).
[282] Hessen (1931, 187).
[283] Hessen (1931, 188-189).
[284] Hessen (1931, 203).
[285] Hessen (1931, 202).
[286] Hessen (1931, 203).
[287] Hessen (1931, 211).

perspective on the relations between science and society; even if inextricably entangled with an indigestible Marxist doctrine, it stands as a voice in favor of contextualizing science.

4.4.2 John Desmond Bernal and Objective Science-Not-From-the-Skies

Similar compliments can be given to John Bernal.[288] Many of the remarks he makes in his history of science are an inversion of the intellectualism and universalism of Sarton (see section 1.3): Bernal attacks the idea of a "pure science",[289] and says that it "would be very wide of the mark to assume that mankind has in the past acted as one intellectual unit".[290] Science has rather always been guided by particular economic interests, driven by "the very process by which men made their living – the productive process by which they got food, clothing and shelter."[291] Realizing this, we see that science can be understood without idealism, mysticism, or "'know nothing' words such as 'inspiration' or 'genius'".[292] Bernal repeats Engels' complaint that "it has become the custom in Germany to write the history of the sciences as if they had fallen from the skies".[293]

Bernal's focus on social factors, combined with his Marxist notion of production as the fundamental social activity leads to a view of science in which 'nature' plays a role only by its relevance for human production – it is an object of practice, rather than knowledge. Bernal quotes Marx: "one basis for life and another for science is *a priori* a lie. Nature as it develops through human history – in the genesis of human society – is the real nature (known to) of man."[294]

The point is taken that sustained scientific interest can never be directed towards something abstracted from human productive interests;[295] however, even if nature comes into the picture only as something that pertains to struggles concerning the organization of production, it matters a lot in that role, for science is essentially "ordered technique", and the technical level of production in any period sets a limit to the possible forms of social organization.[296] Science pushes these limits outwards, but what the technical possibilities that science can discover are, is determined by nature. For instance, when discussing 19th-century developments in electricity and magnetism, Bernal says that:

288 For Bernal's own reception of the Hessen paper, see Bernal (1934, 379-380) and esp. Bernal (1931).
289 Bernal (1954, 17).
290 Bernal (1954, 23).
291 Bernal (1952, 6).
292 Bernal (1954, 22).
293 Bernal (1954, 30).
294 Bernal (1952, 27). The original quote reads: "eine *andre* Basis für das Leben, eine andre für die *Wissenschaft*, ist von vornherein eine Lüge. Die in der menschlichen Geschichte – dem Entstehungsakt der menschlichen Gesellschaft werdende Natur – ist die *wirkliche* Natur d[es] Menschen." (Marx [1883-1844] 1982, 272)
295 Bernal (1954, 253-254).
296 Bernal (1954, ix, 24).

> It is interesting to reflect on the sequence of apparently accidental discoveries that led to this stage of knowledge. At first sight it seems to reinforce the idea that science is entirely unpredictable and depends entirely on purely chance discoveries. Actually, now that we know the character of some of the relations between different aspects of Nature, we can see that it must have been extremely difficult in the long run not to have hit upon them in one way, if not in another.[297]

This is a rather pure doctrine of inevitabilism with regard to the content of science, and the inevitability is decreed not by the logic of society, but by what nature is really like.[298]

In the same context, Bernal says that: "it was these physical discoveries that were to give a new impetus to mathematics and to wean it from the now sterile adherence to the Newtonian tradition."[299] Bernal contrasts tradition and innovation, where innovation comes from physical discoveries that are, from the actor's perspective if not in retrospect, 'accidental'. Nature's role in the history of science stands in an important respect *outside* the logic of history, to the extent that history refers to the explanatory importance of what has happened before; the 'weight of the past'.

In Bernal's Marxist perspective, how science functions in society is determined by the logic of this society, but in the end, science involves knowledge about nature that is to a high extent independent, at least in the sense of: independent of knowledge about society. (This is evident from the fact that in Bernal's vision for a better future, this future needs to possess "the greatest knowledge of Nature and society",[300] which are, apparently, two separate things to acquire knowledge of.[301]) This knowledge drives social change without in turn being driven by it – it is all base and little superstructure.[302] So, if we want a complete understanding of science in history, it is not enough to understand past societies; we need to see that certain relations in nature made it "extremely difficult" in the long run not to have hit upon them.

Bernal's model of science in history, though considering science as a genuinely social phenomenon, does not make the move into the sociologism of saying that what is said about nature is a result only of society. In this Bernal differs from the representatives of the Strong Programme in the Sociology of Scientific Knowledge that we will discuss in the next chapter: they explicitly say that what is believed about nature is first and foremost (and perhaps – but this is the point at issue – exclusively) the result of the structure of society. Rather, Bernal, even from his Marxist vantage point, supports and underlines the intuition

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297 Bernal (1954, 437-438).
298 See also Bernal (1953, 135-136) on nineteenth-century developments: "science had in itself, in its laws, in its accumulation of registered facts, an element of inner stability that prevented external economic or social factors from distorting the direction of its progress, although it might speed up or slow down its rate".
299 Bernal (1954, 437).
300 Bernal (1954, 928).
301 But see also Bernal (1947).
302 Cf. Hands (1998, 700), and Cohen (1994, 217-223).

that if we are inevitabilists about science, we need to believe that science has a special relation to nature.

4.5 Conclusions

From our discussions in this chapter we can draw the following conclusions and insights:

1) Historical inevitabilism about the content of science grounded in the way the world is turns out to be hard to maintain without further qualifications. If we add normative accounts of scientific rationality, the 'normative inevitabilism' that may result from this does not amount to historical inevitabilism.
2) We have seen especially in our dealings with the *Journal of the History of Ideas* articles that it is worthwhile to distinguish between global and local inevitabilism and contingentism. Whereas our considerations in the first half of this chapter discredit the idea of a global nature-based inevitabilism with regard to the content of science, it leaves open the possibility that there are contextually defined and temporally limited cases where the cards were dealt in such a way that a particular outcome was more or less inevitable.
3) At the outset of this chapter, we left open the possibility of something other than nature grounding scientific inevitabilism. However, in practice, both the Marxist positions and Koyré's idealism ground their inevitabilism in the end in a special relation between science and the world. The different positions distinguish themselves mainly by their characterization of the mechanisms through which this special relation is reached (different in particular in their relations to society) and the kinds of obstacles that need to be overcome in order to reach it.
4) With regard to historical understanding, most of the authors we dealt with in this chapter are presentists, in the sense that they believe that our own scientific knowledge can shed light upon earlier science. This presentism seems strongly associated with their inevitabilism about the content of science: it is because we know what past scientists were looking for that our knowledge of the final results helps in understanding their efforts.

Chapter 5: Leaving Nature Out

5.1 The 'No Nature'-principle

The main problem in this thesis concerns the role that nature can play in the historical explanation of scientific development, and how this role relates to the contextual cultural and societal factors that historians are comfortable dealing with. In the previous chapter, we looked at different cases that could be made for scientific inevitabilism, where historical contingencies do not make a difference to what the final stage of science can and will be. We saw that such inevitabilism is in practice grounded in the idea that the world on its own decides what science looks like.

In this chapter, we will look at the other extreme: at positions that have in common that they, at least ostensibly, deny nature a role in the explanation of the historical development of science. Even this careful formulation can, without further qualifications, be misleading, for none of the arguments that we will consider in this chapter depends on skepticism about the existence of an external world that does things to our senses. The world is out there, and its existence as such is independent of what we think about it. However, history of science is not about this world; it is about the history of scientific beliefs and related phenomena; and some scholars would argue that if we want to explain scientific thinking about the world, we do best not to refer to this world.

To avoid tiresome repetition, I will abbreviate this to what I dub the 'No Nature' principle, or NN:

NN: The principle that explanations of scientific development should not rely on nature as an *explanans*.

NN can be embraced on different grounds, though the thinkers dealt with in this chapter have in common that they identify legitimate explanatory factors in scientific belief formation as social factors. Apart from these commonalities, there are still interesting differences between the realism of David Bloor, the methodological relativism of Harry Collins, and the constructivism of Karin Knorr-Cetina.

The idea that science should be regarded as a product of society *rather than* as of nature has inspired a lot of interesting historical work in many subfields of history of science. Klaus Danziger's famous history of the origins of psychology proceeds from the idea that psychological knowledge is socially constructed.[303] Andrew Pickering, in his history of high energy physics, argues that "the world of HEP was socially produced",[304] and that since consensus about the reality of particular theoretical constructs in physics is the outcome of a historical process, "recourse to the reality of natural phenomena and theoretical entities is

[303] Danziger (1990, 1-6).
[304] Pickering (1984, 406).

self-defeating".[305] Pickering says that as a historian, he wants to escape from "the retrospective idiom of the scientist" which refers to "the state of nature", and refer rather to the cultural context in which judgments are made.[306]

In the remaining part of this chapter, we will evaluate possible strategies for supporting NN that have been put forward by different thinkers, especially from the Strong Programme in the Sociology of Scientific Knowledge (SSK). These arguments are:

A1: Nature underdetermines the content of scientific theories.
A2: Nature is common to all of us and therefore 'drops out' of explanations.
A3: Neither external reality nor individual minds provide us with the categories we employ to study nature, and therefore real explanatory power resides in society.
A4: If historians of science rely on knowledge of nature in their explanations of knowledge of nature rather than on their autonomous competence and expertise, their explanations are circular.
A5: Nature is the result of social constructions, not their cause.

All of these arguments can be found in the works of more than one author, but I have chosen to use David Bloor's work as a window upon the first three, Harry Collins' for the fourth, and Karin Knorr-Cetina's for the fifth.

5.2 David Bloor's Realism

5.2.1 Introduction

David Bloor has, at first sight, explicitly and consistently embraced NN. He has, for example, said that "there is […] no need to try to explain stability by appealing to truth or reality";[307] that explanations need to be impartial and 'symmetrical' with respect to the truth or falsity of the opinions they seek to explain;[308] and that the electron itself 'drops out' of explanations of belief in the electron.[309]

On the other hand, however, Bloor has always insisted that the role of observation in scientific belief-formation needs to be taken into account,[310] that the study of knowledge ought to have a "plausible and substantial picture of the role of sensory experience",[311] and that it ought to treat this sensory experience as reliable: "materialism and the reliability of sense experience are […] presupposed by the sociology of knowledge and no retreat from these assumptions is permissible.".[312] Where does this sense experience come from? Bloor

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305 Pickering (1984, 7).
306 Pickering (1984, 8).
307 Bloor (1982, 281).
308 Bloor (1976, 2-5).
309 Bloor (1999, 93).
310 Barnes, Henry, and Bloor (1996, 1).
311 Bloor (1976, 28).
312 Bloor (1976, 29).

says that it is in line with his 'Strong Programme' in the sociology of scientific knowledge to assume that "we exist within a common external environment that has a determinate structure".[313]

On this matter, then, Bloor's ideas are rather commonsensical: nature exists, and its presence is felt reliably by our senses. But why, then, is the way nature is not actually a part of the explanation of the way science is?

Bloor has answered this question in multiple ways: first, by insisting that nature as accessed by sense experience underdetermines theories about nature; and second, by suggesting that, precisely because our natural environment is something that we all share, it drops out of the explanation of differences in scientific belief. I will show that the first argument does not support NN, and that the second is simply invalid. A third line of response open to Bloor, however, is to say that our categories are completely social and exhaustively explained by social factors, but that societies themselves are part of natural reality and that "society enables us rather than disables us; that we know reality through it, not in spite of it".[314]

5.2.2 The First Argument: Underdetermination

As said, Bloor claims that it is no problem for him to say that reality comes into the story through sensory experience. "But the important point", he continues, "is that reality, so experienced, *under-determines* what the scientists say or think about it."[315] This is why we should look rather at social factors like tradition, authority, paradigms, and interests.

Underdetermination of theory by data as a general principle is an ambiguous concept. We have seen in the previous chapter that though it is hard to maintain that reality on its own determines what can be said about it, rationalists may maintain that there is a model of rationality that, in conjunction with relevant data, allows us to decide between theories. The case for underdetermination then depends on the permissiveness of this model of rationality. As Larry Laudan has argued, it may be the case that multiple theories are empirically equivalent in the sense that they entail the same data, but it may be possible to formulate a rational ampliative logic that allows us to say which theories are better supported by the data.[316] It is one thing to say that creationism with divinely planted fossils is empirically equivalent to Darwinian evolution, and quite another thing to say that creationism and Darwinism have equal support.[317]

That the case for underdetermination is always relative to a specific model of rationality sits rather uneasy with the famous symmetry thesis, which prescribes (among other things) that the explanation of scientific belief should be independent of the rationality

[313] Bloor (1976, 36).
[314] Bloor (1996, 853). Virtually the same quote appears in Bloor and Edge (2000, 159). See also Bloor (2011, 403).
[315] Bloor (1996, 841).
[316] Laudan and Leplin (1996, 63-68). See also the position of the non-relativist in Laudan (1988).
[317] Cf. Okasha (2000, 290).

of the studied schools, persons, or decisions.[318] However, it may be that the underdetermination argument mainly serves to show the insufficiency of rationalist accounts of scientific development, and can be dispensed with once these have been refuted.

I take it that Bloor wants to argue that no model of rationality succeeds in avoiding the problem of underdetermination; reality in combination with any model of rationality underdetermines what scientists say or think about reality. I also think that this is a strong case: indeed, scientific theory formation is, in practice, not an algorithmic process.[319] Even though I am slightly more hesitant than Bloor in excluding even the conceptual possibility of an ideal model of rationality that completely determines theory choice given certain input, this is, in the current context, a moot point. After all, if we would discover such a model, it would not be of help in our causal explanation of past science (see the argument in sections 4.2.2 and 4.2.3).

What *can* be of explanatory value is human dispositions in the past, some of which may, pragmatically speaking, be called rational; but Bloor is certainly right if he maintains that reality in combination with common dispositions underdetermines theory choice. We should not believe, he argues, that some belief systems depend on a systematic disturbing of our natural reasoning capacities; rather, "the empirical evidence suggests that all institutionalized systems of belief are compatible with plausible models of natural rationality".[320] Or elsewhere: "the historical literature on scientific controversy typically shows neither side compromising on what we may assume to be their natural reasoning propensities."[321] Comparing British and German communities of experts on aerodynamics, Bloor concludes that "British and German experts did not diverge because their basic cognitive faculties differed or because their personalities were different or because one group engaged with the material world while the other turned its back on it."[322]

Indeed. However, it is unclear how these plausible formulations of the underdetermination problem could support NN. After all, the question here is not whether under some relevant model of rationality, the same evidence lends equal support to more than one theory, but the entirely different question whether under some relevant model of rationality, all conceivable alternative evidence would have lent the same support to the same theories.[323] The question is counterfactual: if the actual evidence supports a certain set of theories to a certain extent, but other evidence would not, then the fact that we have *this* evidence matters to the explanation of theory choice even if it does not determine which of these theories is chosen.

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318 Okasha (2000, 293-294). For a similar confusion, see McMullin (1995, 243-245).
319 Cf. Rorty (1979, 322); Laudan (1996, 17-19).
320 Bloor (1984, 86).
321 Bloor (1988, 67).
322 Bloor (2011, 402).
323 See also the distinctions between different underdetermination theses in Laudan (1996, 31-43); Douven (2008, 294-299).

An argument for NN based on the underdetermination thesis would need to maintain that on any model of rationality, it holds for all theories that for any evidence, these theories and at least some of their alternatives are supported equally well. I know of no defenses of this thesis. We do not need to be inevitabilists about the content of science – not even normative inevitabilists in the sense of section 4.2.3 – in order to believe that evidence from nature influences our beliefs about nature.

5.2.3 The Second Argument: Nature Dropping Out

Given his repeated insistence that sociological approaches to science need to be in harmony with the assumption of a common structured external environment that impinges on our senses, Bloor would probably grant this point. The implied suggestion of this insistence, after all, is that this environment cannot be 'thought away' without consequence. Bloor has explicitly stated that non-social nature plays a role in belief formation.[324] Bloor would then go on, however, by saying that this does not undermine NN, because even though non-social nature plays a role in belief formation, this does not mean that it should play a role in the *explanation* of belief formation (which is what NN is about). That it does not, according to Bloor, is precisely for the reason that he is speaking of a *common* external environment.

Bloor has made this point in an argument against Bruno Latour:

> If we believe, as most of us do believe, that Millikan got it basically right, it will follow that we also believe that electrons, as part of the world Millikan described, did play a causal role in making him believe in, and talk about, electrons. But then we have to remember that (on such a scenario) electrons will *also* have played their part in making sure that Millikan's contemporary and opponent, Felix Ehrenhaft, *didn't* believe in electrons. Once we realise this, then there is a sense in which the electron 'itself' drops out of the story because it is a common factor behind two different responses, and it is the cause of the difference that interests us.[325]

The argument is not that nature isn't there or that it is idle, but that the relations between different societies and nature are symmetrical, and that the symmetry-breaking factors reside in these societies, not in nature. The existence of a heliocentric solar system does not explain the triumph of heliocentrism, precisely because this solar system was there when geocentrism reigned as well.

The problem is that Bloor's reasoning fails to take into account those cases in which differences between societies lead to differences in their relation to nature – for this remains possible even if we grant to Bloor that "all cultures are equally near to nature".[326] It may rain for both of us, yet I may get wet and you may not, because you happen to have brought an

[324] Bloor (1999, 87-91).
[325] Bloor (1999, 93).
[326] Bloor (1999, 88).

umbrella. The difference between us is that you have brought an umbrella and I have not, but this does not mean that it wasn't the rain that got me wet.

Nick Tosh and Tim Lewens have independently made this point against Bloor. Tosh gives the example of bacteria which are exposed to heat, some of which have a thick cell wall and some of which a thin one. If some bacteria die and some don't, the crucial variable may turn out to be the thickness of the cell wall, but the heating does not become irrelevant, and the crucial variable is crucial precisely because it determines the nature of interaction with a common external factor.[327]

Lewens uses the example of Jim who meets Bigfoot in a cave and John who does not: Bigfoot does play a role in Jim's belief in Bigfoot, but not in John's lack of belief in Bigfoot. By analogy, Lewens says that:

> in many cases, if we want to explain contrasts in belief, it will be appropriate to look to what parts of the world the different scientists are exposed to, and sometimes it will be appropriate to say that a salient difference is that scientist A is affected by an object that is part of the content of A's belief that P, while scientist B, who believes not-P, is not affected by that object, or has a very different kind of encounter with the object.[328]

Bloor himself has not responded to Tosh and Lewens, but Jeff Kochan has objected that their arguments fail to take account of the contrastive nature of explanation.[329] For instance, that Jim believes that Bigfoot is in the cave rather than his mother is not explained by the simple fact that Bigfoot is in the cave; it is explained by the fact that Bigfoot is in the cave rather than Jim's mother. Similarly, the question is not why Millikan believed in electrons *simpliciter*, but why he believed in electrons rather than in sub-electrons.

The point about the contrastive nature of explanation is well taken. However, it does not support Bloor's argument. If it would, it should be based on an example building on common external factors and contrasting social factors. If I want to explain why *I* got wet and you did not rather than the other way round, the answer cannot be simply: "because it rained". In this case, the answer is: "because I refused to bring an umbrella and you did not." Similarly, if I want to explain why a certain belief was held by one person or society but not another, the answer cannot be only a factor that is common to both. Thus, when I want to

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[327] Tosh (2007, 186-191).

[328] Lewens (2005, 572).

[329] Kochan (2010). Other than this point, Kochan's argument relies on a misrepresentation of the points at issue. Kochan says, for instance, that Tosh understands the status of electrons in a scientifically realist sense whereas Bloor does not, and that Bloor thus treats electrons as a topic for explanation rather than a resource. In fact, Bloor's points do not rely on a denial of scientific realism (see e.g. Bloor [1999, 87]), though it does rely on a denial of what Bloor calls absolutism (see also Bloor [2011, 421-446]); and Tosh's point does not rely on realism: he explicitly says that "a historian might subscribe to a rather full-blooded form of relativism, while cheerfully scavenging the knowledge from modern scientists to explain the events, and the beliefs, of the past" (Tosh [2006, 677]). Lewens suggests and Tosh explicitly argues that scientific knowledge can be both a resource and a topic for enquiry.

explain why Millikan believed in electrons and Ehrenhaft did not rather than the other way round, the answer cannot be: “because there are electrons”.

But as Kochan’s own example suffices to show, we can ask interesting explanatory questions about beliefs that are contrastive but whose foils have nothing to do with social circumstances. If I ask why I got wet rather than not wet, the answer “because it rained rather than not rained” is a valid answer, while this same answer is not a valid answer to the question why you stayed dry rather than getting wet. “Why did Millikan believe in electrons rather than sub-electrons” is precisely such a question, and the answer might well be: “because the universe contains electrons rather than sub-electrons”. There is no reason why either the rain or the electrons would ‘drop out’ of every story, as NN would require.

5.2.4 The Third Argument: The Social Determination of Classification

Bloor’s third argument for NN builds on a Durkheimian view of the relation between society and nature, which can be summarized in the claim that the classification of things reproduces the classification of men.[330]

Emile Durkheim and Marcel Mauss, in their text on ‘primitive forms of classification’, emphasize that our categories are not handed to us by the world itself or by natural mental necessity. Therefore, we must ask ourselves what leads people to arrange their ideas in the way they do.[331] In *The Elementary Forms of the Religious Life*, Durkheim also emphasizes the insufficiency of classical empiricism and Kantian apriorism: “the categories of human thought are never fixed in a definite form; they are ceaselessly made, unmade, and remade”.[332] In line with Durkheim’s conclusions, Bloor wants to demonstrate that if our categories come neither from the world nor from reason, they come from society.

He justifies this by making use of Mary Hesse’s reception of the work of Duhem and Quine. He claims that “knowledge is organic, and the organization of the whole takes precedence over the parts, overseeing their adjustment and correction”, and that “the organization of the classificatory system is not, and cannot be, determined by the way the world is.”[333] The first of these claims corresponds roughly to Quine’s unmasking of the second dogma in his ‘Two Dogmas of Empiricism’, and his insistence that our statements are not confronted with the tribunal of sense experience individually, but as a corporate body.[334]

According to Bloor, types in a classification are related by laws – “fire is hot”, “wood floats”. These laws can be thought of as the co-presence or co-absence of features of the world, but also as conventions which “belong more to the public domain than to the

330 Bloor (1982, 267).
331 Durkheim and Mauss (1903, 8-9).
332 Durkheim ([1912] 1995, 14).
333 Bloor (1982, 269).
334 Quine (1951, 39).

psyche of the individual learner."[335] These laws form networks, and it is these networks that we bring to our experience. For instance, a system according to which animals that suckle their young are mammals, mammals are a sub-group of land-animals, and everything in the sea is a fish, may be confronted with the existence of whales. Now, similarity relations in experience cannot decide for us how to modify our initial network.[336] In principle, "all the elements of [a] network of classification are equally open to negotiation."[337]

We can see the point: we can retain either of our previous laws by abandoning either of the others. An obvious objection is that the confrontation with young-suckling water animals itself is clearly something from the side of nature. It seems both Quine and Bloor have no trouble admitting this, and Quine even says in *Word and Object* that:

> we can investigate the world, and man as a part of it, and thus find out what cues he could have of what goes on around him. Subtracting his cues from his world view, we get man's net contribution as the difference. This difference marks the extent of man's conceptual sovereignty – the domain within which he can revise his theory while saving the data.[338]

In fact, this quote seems in outright contradiction to NN. Bloor, too, says that "the network is not a free-floating system of thought. Classificatory decisions are made with reference to the world and in the light of experience."[339]

However, this relation to the world is not one of correspondence, but of adaptation, which is a looser relation: "we are used to the idea that there is more than one way of being adapted to the world."[340] Importantly, it is a relation that does not imply that belief is fixated by reality alone. Rather, the stability of a system of knowledge comes "entirely from the collective decisions of its creators and users."[341] Bloor follows Mary Douglas in believing that nature is always put to social use in attempts at control: the coherence of networks of laws derives from social interests.[342]

Nature disappears from this picture not because it does not matter, but because its structuring is subsumed completely by the structuring of the social world. Durkheim emphasizes that since society is part of nature, the fact that ideas are constructed out of social elements does not mean "that they are devoid of all objective value", and in fact means rather the opposite.[343] Bloor echoes this view when he says that "it is perfectly possible for systems of knowledge to reflect society and be addressed to the natural world at

[335] Bloor (1982, 272).
[336] Bloor (1982, 273-274).
[337] Bloor (1982, 277).
[338] Quine (1960, 5).
[339] Bloor (1982, 278).
[340] Bloor (1982, 278).
[341] Bloor (1982, 279-280).
[342] See Woolgar (1981) for a criticism of interests as resources for sociological explanations of science.
[343] Durkheim (1912) 1995, 18-19.

the same time."[344] Though strictly speaking, nature enters the picture *somewhere*, the point of NN turns out not to be that nature does not matter, but that our understanding of it does not add any information to our explanations of why societies believe about nature what they do.

Bloor illustrates his view with an example that reminds strongly of Hessen's interpretation of Newtonian mechanism (see section 4.4.2). In Robert Boyle's time, Bloor says, matter represents the people and is therefore seen as inert and incapable of self-organization, whereas the active principle and force that stands above matter represents the Anglican Church.[345] Though discomfortingly simplistic, this example illustrates how Bloor believes interests determine the representation of nature.

Particularly, it illustrates that it is *current* interests that determine this representation. This is to be expected, since if this requirement is relinquished, the current social order does not completely explain current science. But how plausible is this 'synchronic' view of the relation between society and science? Cannot representations of nature manifest some degree of inertia relative to developments in those social interests, or be 'transplanted' in other societies of whose particular interests they are not a direct product?[346] Ernan McMullin has argued against Bloor that "the spiral structure of the DNA molecule is not in any interesting sense a product of mid-twentieth-century British culture: it is a product rather of a centuries-long effort spanning many cultures of the most widely diverse sort".[347]

What is at stake here is not just the competition between social and rationalistic explanation, but also that between synchronic and diachronic explanation. One thing that distinguishes a historical from a sociological view of science is precisely the idea that the science of a certain time cannot be adequately understood if we take into consideration only this particular time and place.

However, Bloor can offer theoretical support for his idea that society must always be directly involved in the determination of scientific practice, and that it is impossible even in theory to escape a synchronic logic of social interests. This support lies in his interpretation of Wittgensteinian meaning finitism.

5.2.5 The Third Argument Continued: Meaning Finitism and Social Pattern Matching

When does it mean to do something *because* a rule demands it? When we extend a series of multiples of 2 ('2, 4, 6, 8 …'), what is it that compels us to fill in the right numbers – or what defines the right numbers anyway? The 'finitism' that Bloor embraces states that we are taught rules by means of a finite number of concrete cases, that our consciousness itself is finite, and that there is no way of overcoming the finite nature of our resulting

[344] Bloor (1982, 293).
[345] Bloor (1982, 288).
[346] See e.g. the arguments concerning the transplantation of Greek natural philosophy in different cultural contexts by Cohen (2010, 53-96).
[347] McMullin (1984, 158).

understanding.[348] This means that what we have 'in mind' cannot suffice to decide what the rule demands in the infinite number of cases to which it could possibly be applied.

The rules 'themselves' do not compel us; "and yet we constantly speak as if we are compelled by some reality outside us." This is indeed the case, but what compels us is society: "we are only compelled by rules in so far as we, collectively, compel one another."[349] This may be puzzling: if nothing can bridge the gap between the finite nature of our rule-following practices and the potentially infinite scope of our rule application, then why would society form the exception?[350]

In fact, Bloor recognizes that society does not overcome meaning finitism, but as a partial answer to this objection may count the idea that correct rule-following is not merely enforced by society, but defined – and that in this regard, society is omnipotent, because it is unthinkable for any other entity to define correct rule-following. Being 'wrong' simply means being deviant.[351]

This adds up to the case that society as a whole is always directly present whenever we follow a rule in science. Two main lines of opposition to Bloor's argument from meaning finitism can be identified: one on the basis of an argument that there is no gap between rules and their application, and one concerning Bloor's account of the relation between nature and social institutions.

As for the first objection, Michael Lynch has argued against Bloor that it does not do to isolate the formulation of the rule from the practice that is formulated by that rule; these things have an 'internal' relation, and there simply is no rule for counting by twos "aside from the organized practices that 'extend' it to new cases".[352] And without such a gap between rule and practice, there is no underdetermination that needs to be filled with social interests or other sociological categories.[353]

In Bloor's eyes, this ignores the point of finitism, which is precisely that:

> each application of a rule is in principle problematic. […] In principle each application of a rule is negotiable, and the negotiation (or lack of it) is intelligible in terms of the dispositions and interests of the rule followers themselves.[354]

It is not entirely clear that we need to go along with Bloor's opinion that new applications are always problematic. From an everyday perspective, for instance, counting is not a matter of problematic negotiation.[355] Moreover, our practice will always correspond to the same

348 Bloor (1997, 9-26).
349 Bloor (1997, 22).
350 Cf. Brown (1989, 54-56).
351 Bloor (1997, 16). Cf. also ibid., 36.
352 Lynch (1992, 227-228).
353 Cf. Kusch (2004a, 571-572), who is critical of this argument.
354 Bloor (1992, 271).
355 Sharrock (2004).

rule if the formulation and the application of the rule are not distinct. Nonetheless, even if we identify rules as practices, this simply shifts the problem to the question of what sustains these practices in our community. In this respect at least, Bloor's attempt to get 'beyond' the rules to the social interests that constitute or sustain them, seems legitimate. We *can* ask the question in virtue of what (kind of) thing rules in science are what they are.[356]

A second objection against Bloor's arguments is based on the role that society plays in his account, and its implausible relation to nature. The previous section should have immunized us to a too simplistic reading of Bloor in which the power of society, because it is complete, is also arbitrary. Bloor is a realist about 'natural kinds', things that "have an existence independent of our regard".[357] Trees, pebbles and molecules are different in this respect from coins and monarchs, which Bloor sees as 'social kinds' in the sense that they exist in virtue of individual dispositions and relations that refer to each other, and that from the perspective of society as a whole are aptly called 'self-referring'.

Now, why would we, in the study of knowledge of natural kinds, pay attention only to its self-referential component and not to its representational aspects?[358] The reason is that only the former can deliver measures of normativity. If we had only individual 'pattern-matching' processes – that applied the label 'dog' to dogs, for example, on the basis of resemblance to internal (psychological) patterns – there would be no meaning to claims that this pattern-matching had been done rightly or wrongly. With only external reality to keep the pattern-matching 'machines' on track, their results will inevitably diverge. (Given Bloor's meaning finitism, we can understand this point.) Only when there are more of these pattern-matching 'machines' interacting can the social fact of consensus arise.[359] It is this social fact that is of interest here.

Stephen Kemp has argued that Bloor's solution is shaky: there are non-social criteria for pattern-matching, while communities can themselves be unstable and thereby fail to guarantee the stability of the pattern-matching process.[360] Bloor agrees that the group as a whole is in the same position as the individual, but that it does provide norms to the individual that the process of pattern-matching does not generate on its own.[361]

This is all very well, but it does not add up to a defense of NN. Bloor could never maintain that social processes *alone* are enough to stabilize beliefs, and in fact, he has dropped enough hints that, in fact, he is not looking to defend this idea. If we want to explain that a bunch of interacting pattern-matching machines call something a 'dog' rather than a 'cat', we will – and this is an important insight for the determined defense of which Bloor deserves credit – we *will* need to refer to social processes and consensus formation,

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[356] Cf. also Kusch (2004b), who also notes that it is unclear precisely what kind of reduction of meaning, rule and rightness to social institutions Bloor has in mind.
[357] Bloor (1997, 29).
[358] Bloor (1992, 279).
[359] Bloor (1996, 845-853).
[360] Kemp (2005, 712-715).
[361] Bloor (2007, 216-220).

because otherwise it is hard to explain why the pattern-matching machines operate with the same categories, given that things in nature do not come labeled, and given that the pattern-matching machines do not contain those labels *a priori*; but, *given* that our pattern-matching machines have disciplined each other into ordering things into dogs and cats, the fact that they identify something as a dog rather than a cat *must have something to do* with what they have in front of them. In the end, our speech is held together not just by the social world but also by reality.

There may be reasons to ignore this fact. That is, we may want to explain why something is called a 'dog', regardless of what it is. That, however, is Collins' position, not Bloor's; Bloor's position turns out to be, in fact, *not* that "there is […] no need to try to explain stability by appealing to truth or reality"[362] – his arguments do no work to prove that point, and in fact most of his statements go in another direction, in which an appeal to reality is always implicit, but is simply regarded as *less interesting* than the social processes that guide our relations to reality. To reiterate an earlier analogy: Bloor is careful not to claim that the rain doesn't matter to our getting wet or not; rather, he tries to acknowledge the weather as summarily as possible, and then go on to study our umbrellas or other ways to cope with the weather. All the explanatory weight is then put on those umbrellas; the rain 'drops out'.

There is nothing against paying special attention to social processes, especially if this is seen as a necessary corrective to earlier approaches; but in the extent to which Bloor does this, it means that, as Kemp has also noticed, "Bloor cannot provide an adequate way of linking scientific concepts and the natural world".[363] Consider the case of someone trying to understand the usage of different kinds of umbrellas and jackets, not taking into account the fact that all these things have something to do with the rain. The awkwardness of Bloor's position is that he notes the rain and is commonsensical enough to see (and repeatedly affirm) that of course the rain is there and that of course it matters, but that he still wants to avoid having the wetness of rain do any explanatory work.

5.2.6 Bloor on the Hermeneutic Circle

Seeing that Bloor's arguments concerning meaning finitism do not in fact support NN also helps in clarifying what we observed earlier (section 5.2.4), namely that Bloor seems to assume that society always needs to be immediately present.

In some sense, this is an oversimplification of his position, since current social interests and the ways in which they manifest themselves in society could very well carry the weight of the past; Bloor considers historical institutions such as 'convention' and 'tradition' to be relevant. However, what Bloor has in common with most of the inevitabilists discussed in chapter 4, such as Koyré or Bernal, is that he considers what is accumulated in tradition to be distinct from or even opposed to what comes from nature.

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[362] Bloor (1982, 281).

[363] Kemp (2005, 713. Cf. also Kemp (2007, 245).

To illustrate this point, let us consider Bloor's 17th-century example again. If representations of nature in the 17th century depend on social interests in the distinction between an active spiritual elite and a passive population, these interests themselves may be a result of a particular historical path. However, there is no hint that in order to understand 17th-century dealings with nature, we need to refer to, for instance, *16th*-century dealings with nature 'as such'. Thus, saying that our classifications are completely social, though correct in some senses, can work to downplay the fact that they stand in a line of attempts to understand the world. 16th-century natural philosophy was about something – though what it was about may have changed shape and will have been conceptualized in different ways between centuries. Whatever it was about may have influenced its content and development. Embracing NN means denying this in practice, and many of Bloor's remarks are in this spirit.

A more promising perspective is also worth mentioning, however. In a book written together with Barry Barnes and John Henry, Bloor includes a chapter on 'interpretation': studying Millikan's oil-drop experiment, Barnes, Henry and Bloor invoke the notion of interpretation mediated by tradition in order to bridge the gap between experiment and theory.[364] They characterize Millikan's position as being in a 'hermeneutic circle': "his experimental data are his fragments. The whole document is the unknown reality that underlies and produces them."[365]

Understandably, the authors employ this notion to focus on the local interpretive tradition that explains how Millikan selects and explains his results; they have a point to make against the idea that Millikan's procedures can be explained by their own rationality or rightness.[366] But their metaphor may well be used to make a further step, beyond their own argument that we should focus on the social aspects of scientific belief formation. After all, a hermeneutic circle crucially depends on a response from what you are trying to interpret: we are talking not just about society imposing categories on nature, but about nature 'responding' to society's attempts to deal with it as well.

Indeed, how nature can respond is not independent of the ways in which society deals with it; but precisely for this reason – precisely because nature's contribution is not uniform and independent – we ought to regard it as *entangled* in the history leading to the outcomes that we try to explain historically, not disconnected from it either in reality or in our explanations. Bloor, Barnes and Henry do not deny this,[367] but their explicit call for attention to social factors *rather* than reality is not of much help in formulating an adequate formulation of this hermeneutical perspective.

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[364] Barnes, Henry, and Bloor (1996, 18-45).
[365] Barnes, Henry, and Bloor (1996, 25).
[366] Barnes, Henry, and Bloor (1996, 28-30). Arguments concerning rationality slide somewhat too easily into arguments concerning rightness here.
[367] Barnes, Henry, and Bloor (1996, 33).

5.3 Harry Collins' Relativism

5.3.1 Introduction

Harry Collins has unambiguously endorsed NN. For example, he finds himself "refusing to put any demands at all upon reality to circumscribe possible individual belief",[368] and says that we must "treat the natural world as though it in no way constrains what is believed to be."[369] Some of his arguments for this position are the same as Bloor's, which have already been discussed in the previous section; but others are related to Collins' particular position in science studies debates.

Collins sometimes seems to take a constructivist view on (truth about) nature, where he says that the theory of relativity is true, "but it was a truth brought about by agreement to agree about new things. It was not a truth forced on us by the inexorable logic of a set of crucial experiments."[370] The idea that either truth is forced on us by experiments or reality does not matter at all would be a false dichotomy, to be sure; but we need to see this and similar statements of Collins within the context of a polemical argument against rationalism.[371] The point is that there is no scientific method which unambiguously tells scientists what they ought to believe.

There are other ways in which Collins rhetorically tries to make room for social factors against what we have called nature-based inevitabilism:

> If the answer does not lie in recalcitrant Nature, and throughout this book we have suggested that Nature imposes much less of a constraint than we usually imagine, this leaves scientific culture. Science works the way it does, not because of any absolute constraint from Nature, but because we make our science the way that we do.[372]

Collins' notion of the 'experimenter's regress' serves precisely this argument: by showing that theories and accepted experimental results consistently underdetermine each other, he makes room for social factors. This may be a point well worth making; however, we have already seen (section 5.2.2) that the argument from underdetermination does not support NN. Positively making room for social factors in a zero-sum game with natural factors can only go so far.

5.3.2 The Fourth Argument, Step 1: Methodological Relativism

However, Collins has another reason than underdetermination or the experimenter's regress for looking at social factors alone, and that is methodological relativism. This is:

[368] Collins (1976, 437).
[369] Collins (1981b, 218).
[370] Collins (1993, 54).
[371] E.g. Collins (2004, 14).
[372] Collins (1993, 138).

> little more than the scientific prescription to investigate one cause at a time by holding everything else constant. In this case, where the science is contentious, we hold the science constant (treat it as a not-causally contributing variable), and concentrate on the social variables.[373]

Or, even shorter, it comes down to: "deliberately averting the gaze from scientific arguments so as to investigate the social relations of the science more assiduously."[374]

This point is not metaphysical but methodological: it is just good practice not to look at 'scientific arguments'. One remark that we need to make here is that it would be somewhat disappointing if the scope of Collins's 'Empirical Programme Of Relativism' (abbreviated to 'EPOR') indeed needs to draw on a consensus about what counts as 'scientific'. That it needs to do this is suggested by what Collins says are the 'three stages' of EPOR:[375] first, "establishment of the extent to which experimental results allow 'interpretative flexibility'";[376] second, "analysis of the way the potentially ever-ramifying tree of scientific potential is closed down in particular cases by forces outside those that are normally considered 'scientific'"; third, looking at "the way that wider social forces influence the mechanisms of closure".[377] Why the interest in whether forces are 'normally' considered scientific or not? Why is the part of the causal chain that EPOR allows us to look at determined by a contingent demarcation of what causes are scientific and what causes are unscientific?

Is this nitpicking? The controversy between SSK as represented by Bloor and Larry Laudan was precisely about the question whether it was acceptable to distinguish between rational and non-rational motives for scientific decisions, and deal with the non-rational motives in a different way than with the rational motives[378] – Laudan would not equate this distinction with scientific versus non-scientific, of course, because he thought the demarcation problem to be a non-issue.[379] If we take Collins's words seriously, he needs to engage not just Laudan on the question whether it is meaningful to draw a distinction between scientific and non-scientific forces, but also Bloor on the question whether this

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373 Collins (2004, 14-15).
374 Collins (2004, 793). Cf. also Collins (1981b).
375 Cf. Collins (1981a); see Collins (1983, 96-97) for an account of what the third stage would look like. Here Collins proposes that if the side in a controversy that has more resources emerges victorious, we can look at why that side came to have greater resources, which may be because the relevant scientific industry has vested interests in the preservation of current scientific understanding: in this case, if gravity waves had been discovered, "the work of the scientific sector of industry would be thrown into chaos on many fronts." (97)
376 Collins (2004, 784).
377 Collins (2004, 785).
378 Laudan (1982b; 1984, 53-56) chastises Bloor for demanding that rational and irrational opinions are explained by the same kinds of causes; Bloor (1984) answers that reasons in the sense of collective standards can be reduced to forms of social organization.
379 Laudan ([1982a] 2009; 1984, 48-53).

distinction could legitimately result in different research interests, the EPOR approach apparently applying to the non-scientific forces rather than the scientific ones.[380]

But perhaps Collins is not so much hinting at different approaches as at different areas of competence. That is, give the scientists what is theirs – nature – and the sociologists what is theirs – science as a social phenomenon. With regard to nature, Collins says that "outside of scientific Marxism, no social scientist would expect sociology to tell us the proper apperception of nature. When we talk of access to knowledge of nature we must mean access through the sciences."[381] Scientists are our authorities with respect to nature, then. Apart from experts on nature, however, we have experts on society; and science, as a part of society, falls in their domain – natural scientists do not study science; sociologists, anthropologists and historians do that, and since they are *not* the experts on nature, they should regard nature as invisible. "This means that when the scientist says 'scallops' we see only scientists saying scallops. We never see scallops scalloping, nor do we see scallops controlling what scientists say about them."[382]

I think that this methodological relativism is the strongest argument for NN that does not depend on social constructivism, if we understand methodological relativism to mean this: refraining from claiming or assuming anything about nature *not* because it isn't there or because it doesn't make a difference, but because making claims or assumptions about nature is not within our competence as (in Collins' case) sociologists or (in our case) historians.

Still, there are two major objections. First, it is not so clear that a demarcation of disciplinary competence can legitimately exclude certain causally relevant factors categorically from our historical accounts. Does not every historian use information from fields that are outside her specialist expertise? Cannot a social historian note a rise in wheat prices and give this fact a causally important place in her story about a revolution, even if the historical dynamics of wheat prices are strictly in the field of economists or economic historians? Surely, we do not expect this social historian to explain the social unrest she is writing about *without at all* referring to wheat prices, or *as if* the wheat prices in question are fictitious, on the basis of the fact that in her capacity she only sees people *saying* things about wheat prices. We have our experts on wheat prices in history, and our social historian is allowed to copy what those experts say, and make the connection between those wheat prices and the complaints and riots about wheat prices that she writes about.

Of course, not all historians would make the same choices about which parts of the causal network to include in their stories. Our social historian can choose among a multitude of possible stories to tell, among which are:

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380 In Collins' study of the gravitational waves controversy, he assumes a zero sum game between what nature, experiment and method determine and what social forces do, and replies have answered his study by saying that the controversy was actually resolved on the basis of valid experimental evidence (e.g. Franklin [2008, 242-244]; see also Brown [1989, 88-93]).
381 Collins and Yearley (1992a, 386).
382 Collins and Yearley (1992b, 372), referring to Callon (1986).

[1] "Wheat prices rose, then people started complaining about wheat prices, then they overthrew their local government."
[2] "People complained about wheat prices, then they overthrew their local government."

In the first case, a footnote to the relevant secondary literature on economic history will be in order, given that *ex hypothesi*, our social historian did not study the actual wheat prices herself. In the second case, such a reference is not necessary, but the story also provides less potentially relevant information. The point is: what is the difference between the first option above, and the historian of science who chooses to trace the explanations of what happens in science back to (among else) electrons or scallops?[383]

A second objection is that if we agree that science is genuinely about nature, it is not so clear that we can understand science coherently without at least an implicit reliance on its supposed relations to nature. In the next section, we will revisit an older case that is analogous to this one.

5.3.3 Is Nature 'Quite Another Matter'? Ernest Gellner on Social and Biological Kinship

The question whether it is at all possible to study society while suspending beliefs about nature was a major issue in a short controversy in *Philosophy of Science* more than half a century ago.

In 1957, Ernest Gellner published a short paper on a proposal for unambiguous naming on the basis of biological kinship, in which he rather summarily noted that kinship structure could refer both to biological kinship, and to the correlation of social roles with biological kinship.[384] This triggered an irritated response by social anthropologist Rodney Needham, summarized by the claim that "biology is one matter and descent is quite another, of a different order."[385] Biological relations were universal, but descent systems in societies could be structurally and conceptually very different from these relations.

Needham's position bears an interesting resemblance to Collins' in accepting the authority of science over natural kinship relations while claiming that this did not matter to our understanding of social *claims* about kinship relations. Collins, too, effectively says that nature is one matter and science quite another.

Gellner gave an extensive and biting response to Needham's article, turning round precisely this claim that biology was one matter and descent quite another. "What, other than at least partial overlap with physical kinship, could conceivably lead a relationship to be classified as part of a 'kinship structure'?"[386] Theses about the relative importance of kinship relations in some types of societies would become vacuous if the identification of

[383] Cf. Goldman (1999, 9-17, esp. 14-16).
[384] Gellner (1957, 235-236).
[385] Needham (1960, 97).
[386] Gellner (1960, 187).

these relations bore no relation to physical kinship. Of course, social genealogies or kinship beliefs could diverge from physical kinship relations, but without some regular relation to these they could not qualify as a descent system at all.

Translating again to the study of science, we can say with Gellner that something that does not have anything to do with nature cannot be recognized as natural science, and that therefore, if we genuinely drop all presuppositions about what nature looks like, we cannot recognize natural science in history. We recognize both Ptolemy's *Almagest* and Copernicus' *De Revolutionibus* as being (among other things) about our solar system and about the night sky. This does not entail that either of these texts simply represents the solar system or that it cannot diverge from our understanding of this solar system – in this sense, nature is indeed one matter and science quite another. But it does mean that we recognize what Ptolemy and Copernicus are writing about partly because of our own beliefs about the solar system and the night sky (and, in this case, because of a historical connection between us and the text, which Gellner – imagining the anthropologist to be in some situation of radical translation – can ignore here).

The controversy developed further when another social anthropologist, John Barnes, added a distinction between the *pater* – the social father – and the *genitor* – the person supposed to have conceived the child. The identification of the latter depended on theories of procreation which themselves differed between societies. The genetic father and the "culturally-defined physical father" were therefore also distinct, and the interesting disparities were between culturally defined physical kinship and social kinship, rather than genetic and social kinship.[387]

Gellner was unconvinced (though on this particular point it seems he and Barnes talked past rather than disagreed with each other),[388] and reiterated his point that we could not identify brothers in societies without presupposing our own biological notion of brotherhood. He generalized this to a point about the functions and interdependence of social institutions, which would become inexplicable if the relation to physical reality was lost:

> Physical reality (including biological aspects of man) provides the milieu, the obstacles as it were, within which social life goes on. […] Many anthropological explanations presuppose physical facts in this manner, by showing how a practice or institution operates in the physical world and helps solve a problem set by that world. […] The assumption of the validity of the Western view of nature may or may not be justified, and it may be a piece of arrogant hubris: this question I do not propose to discuss. What is certain is that as anthropology is actually practiced this assumption is in fact made, and must be made.[389]

[387] Barnes (1961, 298).
[388] Cf. Barnes (1964).
[389] Gellner (1963, 199).

This is as far as the analogy goes; Gellner does not extend his point to the study of scientific belief itself. Nonetheless, at first sight there seems to be no reason why Gellner's general point – that if we study social institutions that regularly interact with natural relations, then suspending all our beliefs about these natural relations will render us less capable of understanding and even identifying those institutions – would not hold for science as well.

5.3.4 The Fourth Argument, Step 2: The Circularity Argument

To reiterate, we have collected two arguments against Collins' methodological relativism: one is that there seems to be no general rule against using in our explanations knowledge that is strictly outside our field of expertise; the other is that it is *a priori* unlikely that we can understand science as a social institution without taking into account its relation to nature.

However, these objections fail to take into account the complicating factor that in the case of history of science, what we try to explain is authoritative knowledge in our own society. In this specific case, matters look different, because assuming, even implicitly, the authority of science in explaining this same science seems circular, in a way that assuming external knowledge about wheat prices as a social historian (5.3.2) or assuming biological knowledge when studying kinship structures (5.3.3) is not.

Indeed, Collins sees avoidance of circularity as a strong motive behind the 'new way' of looking at science that developed in the 1970s of which he was a part:

> One of the features of the 'new way' was that scientific conclusions were to be *explained*, and this meant they could not figure as *explanations*. The analyst had to ignore the scientific facts of the matter on pain of producing a circular argument: 'This truth came to be established because it was true.' Scientific truth had to drop out of the explanatory equation if the new way was to make sense.[390]

The case for methodological relativism is especially strong in history of science, then, because of the problem of circularity.

It may not be self-evident what precisely the vicious nature of this supposed circularity is. To the extent that it simply consists in believing that the *explanans* occurred because the *explanandum* occurred – that a bridge must have been defective because it collapsed, or that opium must have a *virtus dormitiva* because it makes us sleepy – it is certainly not vicious.[391] For instance, we can believe that a comet passing earth explains some aspects of the historical fact that observers talked about a comet passing earth, even if these historical observers are our main source for this comet. What is special about the history of science is that the possibility is explicitly on the table that the very beliefs we assume when we causally relate a fact in a historical source to a fact in nature may be

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390 Collins (1994, 791).

391 Cf. Nozick (2001, 50); on the opium example, see Hutchison (1991). The *virtus dormitiva* explanation is neither circular nor tautological, and was recognized as excluding other possible explanations in 17th-century debates.

historically contingent, in the sense that we could have held other causal beliefs, and that our actual beliefs depend precisely on what we try to explain. This is an interesting problem, which we will unpack in the remainder of this section.

Furthermore, it is not necessarily the case that our own beliefs actually depend directly on the episode in the history of science we are writing the history of. We can also write an account of beliefs that are historically independent of our own. Even if we are writing the history of an episode in the history of science that is causally relevant to our own beliefs, our beliefs may be overdetermined in such a way that if that episode had not taken place, we would still have believed the same. (This is why we saw in section 3.4 that some scholars found it easier to condone Whiggish presentism on matters about which they were inevitabilists.) For example, the Galileo controversy may be a causally relevant part of the history of our beliefs about the solar system, but our rejection of geocentrism may have so many grounds, of which so many are independent of the outcome of the 17th-century controversy, that we can assume it without fear of circularity.

On the other hand, there may also be cases where our current beliefs can plausibly be regarded as a direct outcome of a particular controversy. In the relatively recent controversies of manageable size that Collins has studied, the historian of science may be regarded as being in this predicament. Even in that case, however, we should distinguish the issue of circularity from the much more straightforward issue of uncertainty. Perhaps we would like, as outsiders to the scientific controversy, to hedge our bets on the reliability of a tentative scientific consensus about, say, gravity waves, because we judge that this consensus is not yet robust enough. In that case, we ascribe to ourselves a kind of 'meta-expertise' that allows us to make this kind of judgments. The point is that suspending our judgment about particular scientific claims will decrease our readiness to assume the reliability of these beliefs in our history-writing as well, independently of considerations about circularity (and independently of NN).

The influence of historical and sociological studies of science upon our 'meta-judgments' is one way in which these studies are hermeneutically relevant. Their influence may consist, for instance, in increasing awareness of the many (kinds of) factors that are involved in the creation of scientific consensus: to a large extent, our awareness that scientific consensus is not the inevitable result of evidence and rationality is itself the result of historical knowledge. Thus, insights in the history of science may make us more skeptical of the objectivity of scientific results. In other cases, we may have been skeptics at the outset, but learn things about belief-forming processes in science that make us reconsider our skepticism. Generally speaking, we cannot know *a priori* how the next thing we learn about science in history will relate to our prejudices; and of course, this depends on what prejudices we hold as much as it does on the case we study.

In this sense there is a hermeneutic circularity at play, a dialogical interaction between what we believe we know about science, and what we can learn from its history. But this interaction can take place only if our actual beliefs about science – for instance, that it reliably teaches us things about the world – are part of it. As Philipp Pettit argued, the claims of the Strong Programme are much more exciting if we interpret them in a "non-conservative" way, where the findings of the sociologist *can* be subversive to the beliefs she studies.[392]

But to repeat: skeptical meta-judgments regarding the reliability of science resulting from historical or sociological knowledge have no relation to a general circularity argument. The circularity argument remains restricted to those cases in which what we believe depends crucially on the history we are studying: we need to believe that it was historically possible for *this* episode to end in such a way that *we* might have turned out believing something different. When we believe this to be the case depends precisely on our causal beliefs, including the role that objects of scientific interest have in determining the content of those beliefs.

Two options remain open for the proponent of the argument from circularity, then: first, she can maintain in specific cases that our beliefs are path-dependent upon this particular case. Second, she can say that even if there are few monograph-sized episodes in the history of science that are crucial to our beliefs in this sense, the prescription of methodological relativism depends on contingentism not with respect to this local episode, but with respect to the history of science as a whole. Our beliefs about the electron may not depend crucially on Millikan's oil drop experiment, or our beliefs about the solar system on Galileo's *Discorsi*, but both do depend on 'the whole' of the history of science, and it is this whole that is the proper object of study of our discipline. If the discipline as a whole relies on the results of the history it seeks to explain, it commits an error of circularity. Since the object of history of natural science is the whole of past investigations of nature, and all our knowledge of nature depends on that whole (except if this knowledge is globally inevitable), we are to suspend all that knowledge when we study past science, which means that we should adhere to NN.

This argument, while relying on global contingentism with regard to the content of natural scientific knowledge, relies on inevitabilism with regard to the boundaries between natural science and other authoritative knowledge, and the content of this other knowledge. After all, if our knowledge about society or culture depends as much on a previous history which is entangled with that of natural science, it is hard to see why we should not on the basis of this same argument suspend this knowledge as well. In the following chapters, we will encounter perspectives which undermine this inevitabilism.

Maybe this takes too literally the logical structure of the case for methodological relativism, without looking at the agenda behind it: its attempt to break down undesirable relations of authority between scientists and the sociologists or historians who study science in history. The point seems to be that we would not do a good job studying critically the development of scientific theories if our study depended on those theories. All is well if the

[392] Pettit (1988, 85-86).

arrows saying 'A can speak authoritatively about B' point from science to nature and from science studies to science; but if science studies need to presuppose something about nature, they need to presuppose something about a field over which science has the authority, and in that case science also has some authority over science studies.

The circle, then, is that of science and science studies talking about each other (science studies about science directly, science about science studies through telling science studies what to suppose about nature). This is not a problem for science, which gets its indirect authority over science studies at no cost; but it is a problem for science studies, similar to that of political journalists who are dependent on politicians for their information on the society in which those politicians operate. Criticism requires some kind of independence.[393]

Making room for criticism is a laudable agenda, but we need to consider the question (to which we will return later, see section 6.6) whether this agenda is indeed served best by methodological relativism and its adherence to NN.

5.4 Karin Knorr-Cetina's Constructivism

5.4.1 The Fifth Argument: Science Constituting Natural Facts

There is one way of arguing that science is about nature, but that nonetheless science studies' authority over science subsumes the authority of science over nature: that is, if nature itself is a construction of science. This argument has been defended most clearly by Karin Knorr-Cetina, who wants to demonstrate the "active constitution of facticity through science."[394]

We can imagine this constructivism to be built upon some philosophical idealism of a more or less sophisticated kind – an *a priori* argument for the dependence of the physical upon (individual or collective) mental life. Knorr-Cetina does not take this route, and pleads rather for a 'genetic approach' based on "direct observation of the *actual site of scientific work* (frequently the scientific laboratory)."[395] Hers is an "empirical, constructivist epistemology".[396] Indeed, the empirical aspects of Knorr-Cetina's own studies are centered upon participant observation.[397] Her constructivism is intertwined with her empiricism.

After all, the kinds of things that manifest themselves to the observer as relevant to the production of science are largely in the realm of locally situated decisions. The observer first sees things being regarded as open-ended, uncertain, and subjective. Then scientists do things and make decisions in specific local settings, and things end up being (regarded as) closed, secure, and objective. This transformation, then, seems to result from their

[393] Cf. Forman (1991) and section 1.4; for an example of what Forman has in mind when he talks about historiography defined (and mangled) by the interests and prejudices of science, see Forman (1983).
[394] Knorr-Cetina (1981, 2).
[395] Knorr-Cetina (1983a, 113-115).
[396] Knorr-Cetina (1983a, 136 [italicization removed]).
[397] E.g. Knorr-Cetina (2000).

constructive operations.[398] Non-constructivist accounts need to appeal to factors that fail to manifest themselves in the empirical world of the observer of science:

> Even the briefest participation in the world of scientific investigation suggests that the language of truth and hypothesis testing (and with it, the descriptivist model of enquiry) is ill-equipped to deal with laboratory work. Where in the laboratory, for example, do we find the 'nature' or 'reality' so critical to the descriptivist interpretation?[399]

Scientists, Knorr-Cetina goes on to say, are not busy with truth but with making things work.

There are several things to note about this argument. First, what scientists talk about in the lab is not necessarily what science is about. This seems an obvious point, but it does lead to the question how we can derive from the fact that scientists in the laboratory don't talk much about truth and faithful description that science is not about those things. A Marxist observer such as Hessen could perfectly harmonize his belief that science is at least partially about class interests with the observation that scientists do not talk about class interests in the laboratory, and that in general class interests are not manifestly visible in the laboratory. The point is not just that class interests could be somewhere else rather than in the laboratory and that therefore asking the rhetorical question 'where in the laboratory' we can find them may simply be looking in the wrong place (like asking 'where in my computer' we can find the internet); it is that they might actually be in the laboratory, but in another way than by being mentioned – in ways that you need to be attuned to in order to see them.

A second point is the choice of the ethnographer of science to restrict herself to what is observable to her in the laboratory. In that case, by definition, she does not have access to the things 'behind' the observable entities in the laboratory that scientists try to study – just like an ethnographer witnessing an evangelical church meeting can observe the behavior of the churchgoers, but not the entities that, according to the faithful, motivate that behavior. Two attitudes towards the status of this restriction are possible. First, that it is a local methodological decision, which does not touch upon the question which entities are *really* behind the behavior of the churchgoers or scientists. In this case, however, it is also impossible to conclude on empirical grounds that these entities do not play a role. The second possible attitude is to say that this restriction is a general imperative, and that we should refrain in all contexts from invoking entities not observable by witnesses participating in this local context. But in that case, the ethnographer's account competes with 'believer's accounts' concerning what entities count as manifestly present in the local context. Where Knorr-Cetina may consider the question where in the laboratory we find 'nature' to have an obvious answer ('nowhere'), the scientist might consider the opposite answer to be just as obvious.

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398 Knorr-Cetina (1983a, 122).
399 Knorr-Cetina (1981, 3).

5.4.2 Fabrication and Adaptation

Nonetheless, if Knorr-Cetina can show that we can give a plausible account of what happens in science and how scientific facts and theories get to be produced, without referring to 'nature' or 'reality' (except possibly as something the production of which is also subsumed under this explanatory account), this is indeed a strong argument to say that notions of nature or reality are, if not meaningless, then at least dispensable when the goal is to explain what happens in science.

In Knorr-Cetina's argument, then, it is important that "models of success which do not require the basic assumptions of objectivism are both thinkable and plausible."[400] She gives two examples at this point: that of psychiatrists, who do not need to have descriptively adequate explanations of the disorders of their patients in order to be able to treat them effectively; and of a mouse which does not need to have an adequate representation of a cat in its mind in order to be able to run away from it. "Like the progress of evolution itself, the progress of science can be linked to mechanisms which do not assume that knowledge mimics nature."[401]

It is important to note here that anti-representationalism is not the same as constructivism. With this last quote, Knorr-Cetina is in the good company of Larry Laudan, for instance.[402] But there are ways other than correspondence in which the content of science can be influenced by nature. Like Bloor, Knorr-Cetina explicitly draws on 'adaptationist' language taken from biological evolution.

However, this analogy between the fabrication of scientific theories and biological evolution works *against* constructivism.[403] Knorr-Cetina's own analogy of the mouse fleeing from the cat already serves to illustrate this. She may be right to say that the mouse has no descriptively true theories about the cat or about its current situation in its mind, and yet it can behave adaptively and flee from danger. But this characterization of the situation makes sense only if the mouse has not 'constructed' the cat. In general, the idea that behavior is adaptive assumes that there is something that it is adapted *to*.

Is this too quick a dismissal? Is there not a constructivist reading possible of the behavior of the mouse? I think the most charitable way of phrasing such a reading would go like this: something is going on in the mouse that induces it to flee; that 'something' is not the cat. If we could enter into the mind of the mouse, then, what we would find there that prompted the fleeing would not be a cat but something of the mouse's own making – much like when we enter the laboratory, the things that we find there that prompt scientists to say certain things are not 'nature itself' but things of their own fabrication.

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400 Knorr-Cetina (1981, 2).
401 Knorr-Cetina (1981, 2).
402 Laudan (1981).
403 Hesse (1988).

Still, however, this is not enough to dodge the question: whether if there hadn't been a cat, the mouse would still have fled. If the answer to this question is 'no', then the cat is causally relevant to the mouse's fleeing. Even if we can at first sight ignore this fact by zooming in just on the mouse, in the end, if we want to understand the difference between a fleeing and a resting mouse, we will need to account for external influences. Talking in terms of adaption rather than correspondence does not take this away.

None of this is to say that it may not happen that two systems – be they mice or laboratories – have (for all relevant purposes) identical relations to their environment and that they nonetheless respond differently: the presence or absence of the cat may indeed underdetermine the behavior of the mouse. Still, especially if we take the evolutionary metaphor seriously, the environment may make a crucial difference – though I should add that it does not follow from this point that we can fall back to explanations like: "scientist X believed theory A because theory A is true."[404] After all, one important point that Knorr-Cetina has made and that should stay with us for the rest of this study, is that scientists make decisions in local contexts – it is not helpful to picture them as being confronted by 'nature *as such*', let alone with 'truth'; rather, they deal with rocks, microscopes, other scientists, bureaucrats, *et cetera*.

For the same reasons, by the way, they are not confronted by 'society *as such*' – SSK-representatives (and especially Bloor) as well as Marxists should pause to consider the question *how* their broader social interests do explanatory work given that they are not concrete *things* that scientists encounter in their daily work – any more than natural laws or valid proofs. Indeed, Knorr-Cetina's sensitivity to the local and the concrete hints at a perspective on the study of science that dispenses not just with 'elevator-words'[405] like truth and rationality, but also with both Nature and Society as explanatory categories. This is a perspective that differs fundamentally from the focus of the current chapter – which is about dismissing Nature as an explanatory category for the greater honor and glory of Society – and that we will address in the following chapter.

5.5 Conclusions

We have discussed the following arguments for NN, and can summarize our reasons for rejecting them as follows.

A1: *Nature underdetermines the content of scientific theories*. We have noted that the affirmation of underdetermination is not a denial of causal relevance.

A2: *Nature is common to all of us and therefore 'drops out' of explanations.* We have noted that because different actors can have different relations to nature, this argument gives no compelling reason not to refer to nature.

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404 Cf. Tosh (2006, 694-697).

405 Hacking (1999).

A3: *Neither external reality nor individual minds provide us with the categories we employ to study nature, and therefore real explanatory power resides in society.* We have noted that social institutions are not wholly self-referential. The importance of society does not imply that nothing outside it matters to our explanations.

A4: *If historians of science rely on knowledge of nature in their explanations of knowledge of nature rather than on their autonomous competence and expertise, their explanations are circular.* We have seen that in the end, this argument applies only where our current scientific knowledge depends on precisely the history that we study, and that in this case it also depends on inevitabilism with regard to the delineation of scientific knowledge. This is not a definitive rejection of A4, and we will return to the problem of circularity in later chapters.

A5: *Nature is the result of social constructions, not its cause.* When we looked at an advocate of this argument, it soon turned out that it could be made to look coherent only if it meant something other than that scientists autonomously created their own environment. In a more complex alternative, then, nature cannot be regarded as just a social construction. We will deal with this in detail in the next chapter.

Chapter 6: Bruno Latour and the Co-Fabrication of Nature and Society

6.1 Another Kind of Constructivism

The perspectives in the previous chapter had in common that they explicitly transferred explanatory power from nature to society; they were aimed at showing what nature could or should not explain in the history of science, and why society should fill the gap. We have seen that arguments for this failed. However, it is also possible to reject the very assumption of a zero-sum game between natural and social explanation. Bruno Latour has argued particularly strongly against the idea that the world is divided into nature and society, or that there is an interesting debate to be had about the extent to which explanatory power resides on either side of that distinction.

In his case, and that of similar approaches such as Andrew Pickering's, it is still strictly true that nature does not play a role in our explanations of scientific development – the position that we abbreviated to NN in the previous chapter. From some perspectives, then, constructivism in Latour's sense may be indistinguishable from Bloor's or Collins' sociologism.[406] However, Latour's position is rather different. Rather than saying that what nature does not explain society does, he argues that nature does not explain because it, together with society, is the *result* of what happens in history of science.

This is still a kind of constructivism, and one that I should perhaps at the outset confess to be unsympathetic to. I believe, for instance, that there was an external world already before the history of science, and that it is possible, both in theory and in practice, to distinguish between those aspects of the world with which humans have had something to do, and those aspects that exist independently of what humans have done. However, while being able to retreat to these fortresses of common sense may give me (and my fellow intuitive realists) some comfort, it is not so obvious that Latour needs to conquer these fortresses. He might just be able to march around them, in which case we will need to confront him on more interesting and challenging grounds, and can only hope to succeed in extending the safety of our fortress to those outside fields – much like Latour's laboratory scientists do with *their* favorite places.

6.2 Kayaking over Bridging

6.2.1 Two Banks, One River

A metaphor that Latour has used multiple times for what has happened in science studies so far is that of a river with the 'social' on one bank and the 'natural' on the other.[407] Too much of the debate, in his opinion, has been spilt on choosing between the two banks, or trying to

[406] E.g. Brown (1989, 92).
[407] Latour (2008a, 13; 2003, 39).

bridge the two banks. Latour describes his position as one not of bridge-building but kayaking with the flow of the river.

For what kind of mistakes and solutions is this a metaphor? Latour applies his point not just to explanations in history of science, but to *all* cases in which attempts are made to identify what is due to nature and what to society. For example, the distinction between "guns kill people" and "people kill people" contrasts a materialistic interpretation with a sociological one, but *both* assume that it is clear what is due to people and what is due to guns.[408] Now, Latour's point is not just that *both* guns *and* people contribute something to the outcome, because that would suggest that we could still speak of them as different kinds of entities that subsequently come together. That is why 'bridging' the two banks is a metaphor for what he *rejects*: the bridge, after all, still departs from both banks, and therefore assumes the very division that it seeks to overcome. As Latour says elsewhere:

> To distinguish a priori 'material' and 'social' ties before linking them together again makes about as much sense as to account for the dynamic of a battle by imagining a group of soldiers and officers stark naked with a huge heap of paraphernalia – tanks, rifles, paperwork, uniforms – and then claim that 'of course there exist[s] some (dialectical) relation between the two'. One should retort adamantly 'No!' There exists no relation whatsoever between 'the material' and 'the social world', because it is this very division which is a complete artifact.[409]

The satire works, but we should subject it to some closer scrutiny: why should we say 'No!' to this? Why couldn't we think of a battle in terms of people and their materials? That is because with that division comes a whole range of associations and expectations about what people and objects can and cannot do: that agency,[410] intentions,[411] and speech[412] are exclusively human properties;[413] that the role of material things is restricted to that of a blind force, being a building block that merely supports human ingenuity or exercises a 'resistance'[414] (and finally, as Latour jokingly adds mainly against David Bloor: proving that you are not an idealist).[415] Some entities have come to be defined as animal or material, others as free; some as conscious, others as mechanical.[416]

These definitions and expectations are not inevitably chiseled into the eternal fabric of the universe; they are the result of a history, the marks of a specifically modern 'Constitution' which separates the power to represent things from the power to represent

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408 Latour (1999, 176-7).
409 Latour (2005, 75-76).
410 Latour (1999, 182).
411 Latour (1999, 192).
412 Latour (2004, 65).
413 Cf also Latour (2005, 107).
414 Latour (2003, 32).
415 Latour (2003, 32; 1999b, 116).
416 Latour (1993a, 15).

subjects – science from politics.[417] (That this Constitution is a historical artefact can be seen from the fact that non-Western cultures have never found use for adopting nature as a category.[418]) Latour considers this Constitution to be unfair in multiple ways, and one of those ways is that it is unfair to objects. These objects have been wrongly portrayed as simply matters of fact, whereas in fact they are "much more interesting, variegated, uncertain, complicated, far reaching, heterogeneous, risky, historical, local, material and networky than the pathetic version offered for too long by philosophers. Rocks are not simply there to be kicked at, desks to be thumped at."[419]

6.2.2 Behind the Two Cultures

Latour's main objective is, then, to return objects to what he thinks is their proper place: not a separate category *outside* society (that can then be made to relate to society), but an integral part of collectives that also contain humans. "You discriminate between the human and the inhuman. I do not hold this bias but see only actors".[420] This brings him in conflict with both sides of the 'two cultures' divide, which in a Latourian diagnosis is a direct reflection of the modern Constitution in its complete separation of talking about things from talking about humans: scientists want to be free from the suggestion that what they say about nonhuman objects has anything to do with human subjectivity or politics; humanists want to protect humanity from objectification. Science studies, according to Latour, undermines those demarcations. "We tell the scientists that *the more connected a science* is to the rest of the collective, *the better* it is, the more accurate, the more verifiable, the more solid [...] But against the other camp, we tell the humanists that *the more nonhumans share existence with humans, the more humane* a collective becomes – and this too runs against what they have been trained for years to believe."[421]

Latour takes some time to explain to the humanists that they need not worry: when action is redistributed among all parts of the collective rather than reduced to a small number of those (i.e. humans), a new form of humanity is gained;[422] and when humanists "add interpretation of machines to interpretation of texts, their culture will not fall to pieces; instead, it will take on added density".[423] Indeed, in my opinion this call for an interpretation of the role of material objects in science studies is a great step forward from the categorical neglect of them that we have seen advocated in theory in the previous

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417 Latour (1993a, 29).
418 Latour (2004, 43).
419 Latour (2005b, 20-21).
420 Latour (1988a, 303). 'You' refers to sociologists. The key idea in this witty article is that humans and non-humans can replace each other; if humans can 'delegate' work and competences to non-human actors, this means that what defines our social relations is not just something human (310). Cf. also Latour (1988b, 9).
421 Latour (1999, 18). The first part of this quote also reiterates the refusal to separate the social from the technical side of science that can already be found in Latour and Woolgar (1979).
422 Latour (1993a, 138).
423 Latour (1996a, viii).

chapter – from the insistence, for example, that we ought not talk about scallops because they are not accessible to us (see section 5.3.2). However, are the fears of Latour's humanist opponents not justified; do we not pay for this step forward by taking two steps back, if we have to talk about objects as though they share in human intentionality, and to talk about humans as though they are simply another part of a network of objects?

To be sure, the commonsensical observation that viruses don't talk to us has not escaped Latour's notice.[424] The point is "not to say that scallops have voting power and will exercise it, or that door closers are entitled to social benefits and burial rites, but that a common vocabulary and a common ontology should be created by crisscrossing the divide by borrowing terms from one end to depict the other."[425] 'Crisscrossing the divide' looks a bit like 'bridging', but Latour's desire not to separate propositions into two ontological realms is something that he states on multiple occasions,[426] and elsewhere he says that if we refuse to distinguish human from natural societies, this does not mean *naturalizing* human societies.[427] "Here we begin to see the advantage of *kayaking* over *bridging*: naturalization is what happens when you try to transport, to transfer the 'senseless hurrying of matter' from the nature bank to the social or human side. That is when you treat the human with the strange notion of primary qualities handed down to you by the *already* bifurcated nature."[428] It should be clear that when Latour declares human societies to be nothing special, nothing worthy or requiring of ontological distinction, he does so not from a perspective of scientism or reductionism: human intentions are not 'actually only' brain states or chemical reactions, or anything else that is already described by the vocabulary of one or more of the natural sciences. Rather, we should find a language in which to talk about all these things.

6.2.3 Initial Problems

But could this satisfy the humanist historian? Or better: can it be done at all – can a common vocabulary be developed that describes the contributions of both humans and nonhumans to collectives without a severe loss of information or clarity? I tend to see Latour's commonsensical admission that scallops don't have voting power as a hint that the answer to that question is negative: voting is something that humans do and that scallops don't. At least, that is the way we usually talk about it, and if we want to flatten out this distinction, we will either have to make a case that scallops *do* vote, or a case that humans do *not* vote, or re-describe what scallops and humans do in terms other than voting.[429]

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[424] Latour (2004, 67).
[425] Callon and Latour (1992, 359).
[426] Latour (1999, 147; 1993, 79-81).
[427] On the limits of naturalization see Latour (1993a, 5-6).
[428] Latour (2008a, 15).
[429] Cf. Lynch (1996, 249-250) on Latour's use of language, and Collins(2012) on the 'flattening' of ontology with Latour and ANT (411) and the, according to Collins, resultant turn to animism: "[Latour and his colleagues] talked of non-humans as though they were human" (412). For Collins, Latour's Actor Network Theory becomes a reactionary opponent of the relativism implicit in Collins' own

Latour takes one or more of these strategies at different locations: when he says that "in the course of [an] experiment Pasteur and the ferment mutually exchange and enhance their properties, Pasteur helping the ferment show its mettle, the ferment 'helping' Pasteur win one of his many medals",[430] the symmetry in word-choice is intended to show that the type of activity attributed to the human actor (Pasteur) can equally well (except for the scare quotes) be attributed to the non-human actor (the ferment). Elsewhere, he explains a general claim that "speech is no longer a specifically human property, or at least humans are no longer its sole masters".[431] What he means is that who *speaks* in the laboratory is not simply the scientist herself – that would be the kind of object-free speech that Latour likes to accuse SSK of seeing everywhere – nor simply 'the facts'; rather, something more complex is happening, namely "that lab coats have invented *speech prostheses that allow nonhumans to participate in the discussions of humans, when humans become perplexed about the participation of new entities in collective life*."[432]

This is a re-description in which neither humans nor non-humans can speak 'on their own', and this seems to be more illustrative of the nature of the move that Latour wants to make than his attempts to show that what non-humans do can be described in the same terms as what humans do (which more often than not require more than a little bit of goodwill to swallow): a move that consists in re-describing *all* activity in such a way that it is distributed over *all* the involved actors. "Purposeful action and intentionality may not be properties of objects, but they are not properties of humans either. They are the properties of institutions, of apparatuses […]".[433] This is a kind of social holism that brings to mind Bloor's insistence that there are social phenomena – like norms – that by definition cannot be localized in individuals;[434] but if it is a social holism, it is a social holism *with things*. Things, after all, are to be involved in proper social science, because there is no 'social stuff' with which they are to be contrasted;[435] they can (and have) *become* part of societies. "Humans, for millions of years, have extended their social relations to other actants with which, with whom, they have swapped many properties, and with which, with whom, they form collectives".[436]

As an argument about the almost inextricably complex interplay of human and non-human causal contributions to developments and outcomes, Latour's point is well taken: indeed, it is impossible to find a human action that does not in some way, however

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sociological approach, precisely by shifting the "power of the world" back to objects, by "[flattening] everything out so much that the old prosaic powers of things can return for those who want them." (413).

430 Latour (1999, 124).

431 Latour (2004, 65).

432 Latour (2004, 67).

433 Latour (1999, 192).

434 But cf. Latour (1996, 234-238).

435 Latour (2005, 1-9).

436 Latour (1999, 198).

trivially (because obviously), relate to non-human factors. Moreover, even if Latour may not have done full justice to SSK perspectives in suggesting that they deny this, I think that his point, that because of this near inextricability it is a bad idea to insist on an exclusive focus on human factors in science studies (e.g. because only those really matter (Bloor, Knorr-Cetina) or because that is the only terrain we are competent to say something about (Collins)), is to be applauded: history does not allow itself to be split into two domains that can be understood *in isolation*.[437] For the history of science, this means that there is not a history of *thinking about nature* that can be understood in isolation from the history of *nature*. If anything, that should be the takeaway of this entire thesis; and to a large extent, Latour proves a valuable ally to this cause, in his many examples of how in all circumstances, a specific outcome is the result of the influence of many different kinds of entities of which we cannot without consequence 'think away' the nonhuman ones.

However, this is not a sufficient reason to collapse all *conceptual* distinctions between human and nonhuman entities, or to claim of specific kinds of actions that they are distributed over different entities in different ways than we usually think. Different things happen in different parts of the world at different times, and the things that happen in scientists' brains may best be described with words that we don't need to describe what happens in the rocks they study.

This is a rather pragmatic rebuttal, a retreat into the fortress of common sense from which I can shout "we don't have these words for nothing!". Latour can answer this objection from several angles: by noting that it invokes an implicit distinction between epistemology and metaphysics (pleading, after all, for a distinction in thought that is not there in reality), as if these are separable domains; by noting that it fails to account for the *constructed* status and the *historicity* of our current ontology; and by noting that it fails to *suspend* our current ontology in order to look more openly to how actors *make* ontologies (and have made our own). These answers need to be dealt with.

6.3 The Construction of Real Things

<u>6.3.1 Collapsing Nature and Its Representation</u>

One of the most intriguing aspects of Latour's thought is his repeated insistence on the co-production of nature and society.[438] Already in *Science in action*, he summarizes this in two of his famous rules of method:

> 3 Since the settlement of a controversy is the *cause* of Nature's representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled.

[437] Cf. Latour (2004, 33-35).

[438] E.g. Latour (1990, 147); Callon and Latour (1992, 349).

> 4 Since the settlement of a controversy is the *cause* of Society's stability, we cannot use Society to explain how and why a controversy has been settled. We should consider symmetrically the efforts to enroll human and non-human resources.[439]

Both nature and society are the result of certain settlements, then, and not causes to which we can appeal. It will be noted that Latour slides from 'Nature's representation' to 'Nature' here,[440] in a way that seems to undermine the logic of his argument: if he does not hold his content terms stable, the conclusion certainly does not follow.

However, the confusion is not the result of a slip of the tongue or another kind of thoughtlessness: Latour believes that, indeed, it is not just *representations* of nature that are the result of a history, but *nature itself* – and not just in the sense that natural entities evolve over time.[441] Latour is well aware that his position is counterintuitive, and therefore he emphasizes that he indeed means to defend this counterintuitive claim, by explicitly rejecting the intuitive alternative that banks on a distinction between one natural world and different historically developed representations of that world:

> There may be thousands of ways of imagining how kinships bring children into existence, but there is only, it is argued, *one* developmental physiology to explain how babies really grow in the womb. There may be thousands of way to design a bridge and to decorate its surface, but only one way for gravity to exert its forces. The first *multiplicity* is the domain of the social sciences; the second *unity* is the purview of natural scientists. […] This is just the solution that ANT [Actor-Network Theory] wishes to render untenable. With such a divide between one reality and many interpretations, the continuity and commensurability of what we call the associations would immediately disappear, since the multiple will run its troubled historical course while the unified reality will remain intact, untouched, and remote from any human history.[442]

Elsewhere, Latour summarizes what is wrong with the position he tries to undermine in the slogan: "multiculturalism acquires its rights to multiplicity only because it is solidly propped up by *mononaturalism*."[443] It is this 'mononaturalism' that has to go, then; and the point is not just that what humans do to the world changes it, that human history influences the world – if that were the case, "the fact remains that there are two histories, or rather one history full of sound and fury that unfolds *within a framework* that itself has no history, or creates no history. Now, this good-sense conception is precisely what we are going to have to abandon."[444] No, there is actually a multiplicity of worlds, a 'pluriverse';[445] and that

[439] Latour (1987, 258).
[440] Bloor (1999, 87).
[441] Latour (1999, 145-146).
[442] Latour (2005, 117).
[443] Latour (2004, 33).
[444] Latour (2004, 34).
[445] Latour (2005, 116).

pluriverse does not consist of worlds that are 'just there', but of worlds that are the result of the actions of actors. How does this work?

6.3.2 Science and Technology

The way in which Latour makes this position work, is by attacking the distinction between the independence of reality and the work that is done to create that reality. We may, again, proceed from an intuitive trade-off between reality and construction, from the idea that we can distinguish between what is due to how the world is and what is due to the way in which human societies look at that world; under this light, if something is *constructed*(-by-subjects) it becomes *ipso facto* less real (because grounded to a lesser extent in the way the world really is). This, however, *assumes* the clear distribution of different kinds of causal influence among different kinds of entities; and we previously saw that Latour was onto something at least in his insistence that those causal influences are not isolated enough to assume such clarity beforehand.

So why not, instead of assuming that the reality and constructedness that we ascribe to something are involved in some zero-sum game, assume that *real* things can be *constructed*? Is that not how we talk about buildings, for which it would hardly make sense to ask whether they are real or constructed? Then why couldn't we do the same for science, where currently there "seems to be no plausible way to say that *because* something has been constructed and *well*-constructed it is *thus* solid, durable, independent, autonomous, and necessary"?[446]

Indeed, Latour likes to draw and emphasize analogies between science and technology, because the construction and reality of technological products are, like those of buildings, much more obviously complementary than they *seem* to be in science: "no one would dare assert that the Diesel engine 'was always already there, even before it was discovered.'"[447] The analogy is an interesting one, especially for historians: it suggests that history can produce solid and independent things whose solidity and independence does not derive from their history-transcendence, from the fact, that is, that they were 'always already there'.

The big question, then, is: can we, in the case of scientific entities like in the case of Diesel engines, account for their solidity without assuming that they were already there before they were discovered? To sharpen the focus of this question once more: given the position that Latour has taken, we are *not* just talking about how historically developed scientific *theories* can become stable – we may agree that a theory like the second law of thermodynamics is probably there to stay, even if we also agree that it hasn't always existed, and we may subsequently ask where it gets its stability. In that case, the answer to the latter question could still refer to stable structures in a history-transcending outside world, which is the very thing that Latour so urgently wants to dispense with. No, the question extends to

[446] Latour (2003, 36). Cf. Latour (2002, 16).
[447] Latour (1996a, 23).

the *entities* described by those theories as well: so, can we explain why microbes or exoplanets *themselves* are stable elements of the furniture of our universe *without* presupposing that they were 'already there' before they were discovered? (The simple recognition that they were not 'always' already there, because they evolved over time, is not the point, as we have seen.) Or perhaps even more radically: can we explain this about whole *time periods*? "The billions of years since the Big Bang date from the 1950s; the pre-Cambrian era dates from the mid-nineteenth century; as for the particles that make up the universe, they were all born in the twentieth century."[448]

6.3.3 Adding Actors

For all these things, Latour advocates the notion of a 'relative existence': "an existence that is no longer framed by the choice between never and nowhere on the one hand, and always and everywhere on the other."[449] He demands a 'generalized historicity'. No-one, he argues, will ask the question "Where was Pasteur *before* 1822?"[450] – well then, why *do* we ask this about the microbes that he discovered? According to Latour, this question is just as meaningless.[451] This seems like a disingenuous analogy: surely, those questions are not the same? Surely we can all agree that Pasteur as a human individual came into being only in a specific year and did not exist before that, *in the same way* that we can agree that the individual microbes that we find under our microscope in 21st-century laboratories did not exist in ancient Egypt; but that in both cases, *other* individual humans and *other* individual microbes *did* exist before – e.g. in ancient Egypt?

But if Latour's analogy fails, it is just one rhetorical strategy that turns out not to work. Latour is trying to get across an intuition in which the application of *any* concept in times or places other than where it has been developed becomes something problematic – not necessarily impossible, but problematic; and one way in which he tries to get this across is in trying to establish analogies between material objects and concepts. Just like we don't expect humans or machine-guns to time travel and *do* something before they actually exist, we shouldn't simply expect concepts to aid us at such a temporal distance.[452] The second aspect of the analogy, then, is the interchangeability – in Latour's thinking – of microbes and the *concept* of microbes; these are not different entities, one being a natural kind in the universe and the other being a nineteenth-century interpretation of that stable thing in nature. No, it is microbes *themselves* that come into being in the nineteenth century.

For the point is that entities, and classes of entities, can *pass* from nonexistence to existence *through* fabrication. "It is possible to go from a nonexistent entity to a generic class by passing through stages in which the entity is made of floating sense data, taken as a

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[448] Latour (2004, 35).
[449] Latour (2000, 252).
[450] Latour (2000, 253).
[451] Latour (2000, 264).
[452] Latour (2000, 250).

name of action, and then, finally, turned into a plantlike and organized being with a place within a well-established taxonomy."[453] This whole process is the result not of *discovery*, then, but of *work* being done: "let us say that in his laboratory in Lille Pasteur is *designing* an *actor*."[454]

This, of course, Pasteur is not doing on his own – we can predict now that Latour will be consistent in his insistence on not giving all agency to one type of actor (in this case the human scientist). In fact, Latour emphasizes that even among the human actors, Pasteur is not the autonomous innovator from whom scientific change simply emanates;[455] what happens is that Pasteur and other agents translate their own and each other's interests in such ways as to forge alliances.[456] Networks are being built and in the specific network in which Pasteur moves, he *adds* a new agent:

> Pasteur adds to all the forces that composed French society at the time a new force for which he is the only credible spokesman – the microbe. […] If you reveal microbes as essential actors in all social relations, then you need to make room for them, and for the people who show them and can eliminate them. Indeed the more you want to get rid of the microbes, the more room you should grant Pasteurians. This is not false consciousness, this is not looking for biased world views, this is just what the Pasteurians *did* and the way they were *seen* by all the other actors of the time.[457]

Here we seem to approach an answer to our question: what happens when new entities are discovered is not that they are being created as if by divine decree, nor that they were already always there, but that they are *made in* being added to a network.

6.4 The Inexplicable Development of Networks

<u>6.4.1 Which Napoleon?</u>

The question now – and, in my view, the question on which the plausibility of Latour's entire program hinges – is whether this adding of entities can be shown to be non-arbitrary in a way that doesn't silently assume something about extra-historical nature, about how things were and are *outside* the network *before* and *independently of* how the network came to be modified. Is there any way in which we can understand that it is *microbes* that Pasteur adds to the 19th-century French network, rather than anything else?[458]

One way to see that this is not arbitrary is that there were alternatives that did not make it – like Pouchet's defense of spontaneous generation. Are the different fates of Pasteur's and Pouchet's propositions to be explained by something else than the existence

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[453] Latour (1999, 122).
[454] Latour (1999, 122).
[455] Latour (1993b, 25).
[456] Latour (1993b, 65).
[457] Latour (1983, 157).
[458] See Shapin (1988, 541-543) on the lack of explanatory ambition in Latour's program.

(rather than non-existence) of microbes? Yes, because those fates are the fates of the entire networks with which they are connected; and those networks hardly overlap.[459] Especially not since apparently identical elements between them are, on a second look, not identical: Pouchet and Pasteur both write to Napoleon III for support, but Pouchet wants this support in the form of an endorsement of spontaneous generation; Pasteur in the form of money. They write to different emperors, then, who have different relations to the demarcation between science and politics.[460] Pasteur's Napoleon III respects the modern Constitution; Pouchet's doesn't.

Latour's remark that the two gentlemen did not write to the same emperor has prompted a sarcastic response from Nick Tosh, who noted that if we wanted to understand what Pasteur and Pouchet were thinking and doing, it might make sense to remember that they probably had different beliefs about Napoleon III, but that otherwise, "the man whom we call Napoleon III was an everyday, macroscopic object who did little to excite the retrospective attentions of philosophers of science".[461] This is an important point: it highlights the fact that Latour's radical metaphysics, if we went along with it, would have consequences not just for the scientific entities in our historical accounts, but for *all* entities. There is, after all, no reason for Latour to say that the existence of microbes needs to be regarded as relative to a network, but that Napoleon III is an unproblematic object. And indeed, if we take him seriously, he seems to be consistent and *does* say the same about Napoleon III.

In that case, however, deciding for a non-relative existence of Napoleon III will already be a sin against Latour's metaphysics. We do not need to be anachronistic historians of science who believe that there were microbes before Pasteur; the simple belief that Napoleon III existed independently of either Pasteur's or Pouchet's network would suffice to fail at doing science studies right. If good practice in general history and good practice in history of science are indeed so similar, perhaps this analogy works rather the other way round: the fact that most of their colleagues in general history unproblematically use Napoleon III in their accounts should serve as a sign to historians of science that, at least until the philosophers have settled the issue for all entities, they can unproblematically use microbes in theirs.

But suppose that general historians do not yet do Latourian actor-network metaphysics, but would want to do so; how would they need to proceed? To what would they 'relativize' Napoleon III's existence? Isn't there an infinite number of networks from which they can choose? And aren't many of those networks *independent* of both Pasteur and Pouchet up until the point where they make contact – e.g. when the scientists write to their emperor? If so, isn't the way Napoleon III is going to behave when his network and those of Pasteur and Pouchet extend in such a way as to partially overlap, determined in large part

[459] Latour (2000, 260-261).
[460] Latour (2000, 261n11).
[461] Tosh (2007, 205).

by his previous movements in all the *other* networks of which he is a part? And isn't, then, the capacity of the Pasteurian network to *fabricate* its own Napoleon III very limited in the face of all those other networks?

Perhaps this is underestimating the absolute primacy of the one particular network that we study – for instance, that of Pasteur. When we are genuinely 'in' Pasteur's network, after all, there is no meaningful speech possible about networks independent of him. The idea just suggested, of having a bird's eye perspective on a multitude of networks, may be regarded by Latour as begging the question, if the right way to proceed is simply to immerse yourself in one network and follow the actants that make up that network. We may encounter a Napoleon III in the Pasteurian network, and he may do some work in there; and if we start all over again and immerse ourselves in Pouchet's network, we may again encounter an actant called Napoleon III; but there is no way to identify these two Napoleons, since they more or less literally operate in different worlds.

This reply, admirably consistent though it would be, would worsen two problems for Latour: one is that the features of the Napoleon III to whom Pasteur writes are not completely determined by the prior Pasteurian network. The other is that if the two networks are so thoroughly incommensurable that we cannot identify their Napoleon IIIs as the same entity, then any attempt to explain their different fates will beg the question how we could even conceive of comparing them.[462]

Latour's constructivism here is, I think, untenable, for a reason that is – after all and in spite of Latour's attempts to keep it at an arm's length – analogous to the reason why social constructivism fails: no more than physicists are able to single-handedly or collectively *create* quarks, is the specific Pasteurian network (both human and non-human actors included) strong enough to *create* its own French emperor in all relevant aspects, *or* to create microbes. Sufficient explanations for how this emperor or these micro-organisms behave are not to be found in the network: phrased differently, the entire network underdetermines who the emperor is or what microbes are. Where does the resistance to certain possible developments of networks take place, if the external world does not exercise an influence of its own and the internal structure of the network fails to determine its entire development? This is a question to which I believe Latour has, in the end, no answer.

6.4.2 Tracing Networks

Then again, perhaps Latour has no problem embracing a certain open-endedness of events in one network. And perhaps the point of limiting oneself to one locality lies precisely in taking seriously the actor's perspective.[463] We can write a separate history of Napoleon III and the networks in which *he* moves, but if we write a history of *Pasteur*'s networks, Napoleon III only enters the stage at a specific moment, and what we can assume and know

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[462] Latour seems to deal with the question of how Pasteur succeeded in "*withdrawing* Pouchet's common phenomenon [i.e. spontaneous generation] from space-time" (Latour [2000, 253]; see also ibid., 255).
[463] Latour (2005, 235); Crease (2000, 22).

about who he is and how he reacts to letters will need to be restricted to what this specific Pasteurian actor-network allows us to trace. If we happen to know *other* things about Napoleon III from different studies, we need to suppress this knowledge, because it would detract from our purpose of seeing the internal consistency and stability of the Pasteurian network. Does following this maxim mean that we cannot talk about how things *really* are? No, and the reason why not should be obvious by now: what is real is a construction *of* this network. Latour has said that ANT's main tenet is that "actors themselves make everything, including their own frames, their own theories, their own metaphysics, even their own ontologies."[464]

If we write one story about microbes (if they have left behind traces)[465] and a separate story about Napoleon III, we do not just provide different interpretations of the same reality that can be made to match later; in tracing Pasteur's movements, we follow the construction of *ontologies* that are different from (and often incommensurable with)[466] the ontologies of other networks. And we need to take seriously those local ontologies.

But how do we demarcate networks? How do we know that Napoleon III or microbes are not part of Pasteur's network at a given time? Isn't it up to our arbitrary judgment how we cut up or unite different parts of history? No, because the network can be located only where it has left traces: we cannot arbitrarily say that we are studying the network of 'all of history' or of 'the universe', because that universalizes the local rather than *extending* it.

For that is how scientific theories increase their reach: by extending the networks of which they are part. Again, technology provides the paradigm. "Is a railroad local or global? Neither.[467] It is local at all points, since you always find sleepers and railroad workers, and you have stations and automatic ticket machines scattered along the way. Yet it is global, since it takes you from Madrid to Berlin or from Brest to Vladivostok."[468] This is the same for scientific theories. "No one has ever observed a fact, a theory or a machine that could survive outside of the networks that gave birth to them."[469] It is not just that theories are produced locally and then applied with more or less success *outside* (the laboratory, the local network); no, their application is synonymous with an extension of the local network to a bigger local network.[470]

There is no 'outside' to the 'inside', then,[471] and we can see this best by studying the movements on a micro-level: in the case of Pasteur, "if you watch carefully the prior displacement of the laboratory to capture farmers' interests, then to learn from veterinary

464 Latour (2005, 147).
465 Latour (2005, 150).
466 Latour (2000, 261).
467 Latour seems to mean 'both' here.
468 Latour (1993a, 117).
469 Latour (1987, 248).
470 Cf. also Latour (2005, 173-190; 1990, 153).
471 Latour (1983, 155).

sciences, then to transform the farm back into the guise of a laboratory, it is still interesting, extraordinarily clever and ingenious, but it is *not* a miracle."[472] These are all concrete interactions: there is no miraculous action at a distance, which is about the only thing that Latourian metaphysics, in its Cartesian predilection for matter in motion, categorically forbids. Concretely, this otherwise open-ended metaphysics, in which every event in the history of science has to be understood in terms of things (of whatever kind) circulating in networks (made up of whichever entities), underlines and supports the call for an exclusive attention to actor's categories: we can be confident that we will not *need* to look beyond what traces have been left behind within the network that we want to study, because what it hasn't touched and what hasn't touched it doesn't exist to it; and we can be sure that we *ought not* look beyond the ontology created by the actors within the network, because that amounts to an invocation of non-existent entities and a betrayal of the actors.

And of course, this also excludes the ontology that *we* might bring to our investigation.

> Abandoning the fixed frame of reference offered by ether, as physicists did, appears in retrospect a rather simple affair when compared with what we will have to let go of if we want to leave the actors free to deploy the full incommensurability of their own world-making activities. Be prepared to cast off agency, structure, psyche, time, and space along with every other philosophical and anthropological category, no matter how deeply rooted in common sense they may appear to be.[473]

6.5 When Are Electrons and Microbes Anachronisms?

6.5.1 Conceptual Anachronisms as Causal Anachronisms

The discussion has been expanded, then, to the general question to what extent we are allowed to bring our own categories and our own opinions or knowledge about the world with us when we study past or other societies. Latour clearly does not simply reiterate old arguments against anachronism,[474] but our considerations of 'Whig history', presentism, and anachronism in chapter 3 become relevant here.

One distinction we made there (section 3.3) was between 'conceptual' and 'causal' anachronisms: a conceptual anachronism consisted 'merely' in using current words and concepts in describing the past, where a causal anachronism consisted in asserting that something happened in the past that could not have happened – where 'could not', of course, still meant: could not *according to us*. This distinction, it seems, explains why we can legitimately say that there were electrons and microbes in Antiquity, while also upholding that saying that there were laboratories in Antiquity clearly constitutes a genuine error of anachronism (since not only did laboratories not exist, but their existence contradicts our

[472] Latour (1983, 151).
[473] Latour (2005, 24-25).
[474] Cf. Latour (2000, 249).

positive beliefs about what was possible and impossible in Antiquity). More subtly, it shed light upon why things become more complex when we debate the question whether it is viciously anachronistic to call Aristotle a biologist: for some things, the existence of certain concepts in a culture is among the conditions for their existence in that culture, and this is evidently true for certain social roles. Since 'biologist' has a lot of meanings – among which are someone who studies living things, or someone who has a PhD in a modern academic field – and there are no clear rules that decide which of these meanings are essential, it can be unclear whether calling Aristotle a biologist is only conceptually anachronistic (applying a concept to him by which he did not actually identify himself) or causally anachronistic (calling him something that he was not and that he could at the time not possible have been).

Now, how does this distinction relate to the questions we are presently dealing with? First, it brings the stakes into a clear focus: when we say (or silently assume) that there were electrons or microbes in the past, we do not need to be afraid to be accused *just* of conceptual anachronism: the easy counter-move there would be to profess that we are aware that 'electrons' here have a lot of cultural connotations that they did not have, say, in Antiquity, because the ancients did not have a concept of electrons. We mean 'just' the electrons themselves.

What we need to be afraid of, then, is the accusation of causal anachronism: the argument that the *conditions* for the *existence* of what *we* call electrons were not met in Antiquity. And we have also seen how such an argument might get off the ground: it might get off the ground by saying that among the conditions for existence of electrons is the existence of a concept of electrons – or better, that the two go hand in hand. This, indeed, seems to capture precisely what Latour is doing. Microbes, according to Latour, came into being in the 19th century because that is when they were added to our ontology: the histories and fates of the microbes themselves and of the concept of microbes are not different.[475] Latour rejects dualism of every flavour; we can expect that for him, conceptual anachronism merges completely with causal anachronism. And if every conceptual anachronism is a causal anachronism, then every anachronism is a vicious one. Which is precisely why we should only ever listen to the actors and study their networks.

How would this identification come about? One way to make sense of this is to say that 'electron' doesn't refer to anything other than what happens when we do experiments with oil-drops and write down the results, or when we apply any of the other modern scientific procedures in which electrons play a role. Electrons, we can imagine Latour saying, are everywhere now, since we have added them to our ontology; but all the places where we do the work through which this addition has become possible are places that did not exist in Antiquity. If *none* of the things (and relations between things) that work to fabricate electrons existed in Antiquity, then what do we refer to when we talk about electrons in Antiquity?

[475] Latour (1999, 147-148).

6.5.2 Relativizing Relativized Existence

Does this not bring us back to square one, to the same impasse with which we started, namely that there is no way to get around Latour's insistence that things come to exist only when they are added to ontologies in local networks? No, for two reasons.

First, Latour's own vocabulary provides a solution: not only electrons and microbes are recent addition to our ontology, after all, but so are time periods like 'Antiquity' or 'ancient Egypt': a case can be made that in the sense in which we understand these things, they have been constructed very recently – and, crucially, that the addition of electrons and microbes to our ontology has effected a new construction of Antiquity and ancient Egypt. Before microbes were fabricated, we lived in a world with an Antiquity that did not contain microbes; but after the fabrication of microbes, our ancient Egypt changed as well as our present, and in this new ancient Egypt, there may well be microbes. Taking Latour's radicalism seriously, there is no point at which it stops, and its final implications turn out to be rather conservative. Rather than altering the way we look at the entities in our world and relativizing their existence, our whole world, including its past as we commonsensically tend to think of it, springs back into existence; the only thing that has changed is that it has been folded up, so to speak, in our own locality.

To clarify: Latour's notion of relativized existence is radical only if it is selectively applied: for instance, if we implicitly still believe that there is a nineteenth century independent of our own local networks, with which we can make contact and in which we can trace the actors until we discover that at one point (t1) in this nineteenth century microbes were not among those actors, whereas at a later point (t2) they were. In fact, t1 and t2 are both constructions fabricated in the present – and the present, as we know, is a locality in which microbes are everywhere. There is no reason why t1 would be excluded from this present – unless, again, we believe that something happens between t1 and t2 that is independent of our locality but that still pertains to it. For electrons in Antiquity the same holds: unless we believe that Antiquity independently existed long before our present, we need to consider the possibility that the ways through which we have fabricated and are continuing to fabricate electrons in the present have enabled electrons to travel to Antiquity as well.

This argument should satisfy anyone who is convinced by Latour's way of thinking about ontology; in fact, Latour applies it when he says that "just as historians are not forced to imagine one single nature about which Pasteur and Pouchet would make different 'interpretations,' neither are they forced to imagine a single nineteenth century imposing its imprint on historical actors."[476] One way to answer the question what we refer to when we talk about electrons in Antiquity, then, is to say that we are talking about a recently fabricated Antiquity that contains our recently fabricated electrons.

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[476] Latour (1999, 165).

Second, if we do not want to outflank Latour in his Latourianism, we do not need to. We have also seen in all our examples that we can differ about the question to which extent specific kinds are dependent upon the existence of specific concepts. And the recognition that it is *our* beliefs about the relations between concepts and reality that matter in history-writing, means that positions other than Latour's have become possible: Latour's approach is not the most general or neutral; it is one of a range of possibilities.

We have seen that when we use different concepts, we have different expectations regarding the conditions under which the phenomena described by those concepts can be realized. Now, we commit causal anachronism only when we assume the existence of phenomena in the past whose existence was impossible at the time because at that time specific necessary conditions for their existence were not met. Whether we believe this to be the case, then, depends on our expectations about these conditions and about the nature of these phenomena. Is what we mean by electrons exhaustively described by all the inscriptions of them in our own network? Possibly, but it is equally possible to mean something else; most scientists would claim that when they talk about electrons, they mean to refer to something in nature (whether or not they do so successfully). The dynamics of our own expectations, therefore, may just be more complex than the simple rule that the conditions for existence are always and always only met as soon as the related concept is added to our ontology. At the very least, this simple rule is just one end of the spectrum – the end taken by Latour.

Sure, there is room for more people at that end of the spectrum; perhaps Latour's metaphysical position is convincing to some, in which case they are right to avoid assuming the existence of electrons, microbes and the pre-Cambrian before the 19th century (or at least: before *some* 19th centuries; as we have seen, it is rather unclear why this should hold for our own 19th century). But these Latourians will then have to answer the objection that historicists have always had to struggle with, and which I consider to be insurmountable, that approaching the past without any prior concepts is impossible. Though Latour tries to be as consistent as he can in letting the actors decide on their own ontologies (a most admirable example being that when he says that Pasteur extends a network that spreads laboratory products "all over France", he adds that 'all over France' is itself "a construction made by statistics-gathering institutions"),[477] in the end it is not the actors talking but Latour – even 'actor' or 'actor-network' is not necessarily an actors' category, as both Latour himself and Nicholas Jardine have noticed.[478]

We always proceed from our own ontology. This we may modify and restrict for numerous reasons, including historical knowledge; but those reasons do not reduce to the

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477 Latour (1983, 152).

478 Jardine (2000, 263-265); Crease, Ihde, and Jensen (2003, 22), where Latour says: "But if you begin your fieldwork by presupposing a common world in the sense of positing that there exists a culture, that humans are defined by being in culture, have a body, genes, and neurons, then you are finished. You are not an anthropologist because you have already decided for the actors what is the world they have in common even if they refuse to have a body and a culture ... or for that matter to be actors at all."

simple rule that we are completely undecided and should ask the actors as if we had no clue what the world looked like.[479] In the end, this is nothing more than the pragmatic acknowledgment that we get our categories from multiple sources, not just from the ones we are looking at presently. Why should our study of 19th-century networks containing microbes refer only to concepts as defined by (and circulating in) the network we are studying? We don't use one case study of a 17th-century painting to define colors, even though we are allowed to refer to colors when we describe a 17th-century painting; we don't use one case study of ant societies to define insects, even though we are allowed to call the ants insects. Similarly, we don't use one study of 19th-century scientists talking about microbes to define microbes. We could, of course – it is possible to say: whatever Pasteur is doing in his laboratory, that is what constitutes microbes – but that would amount to a redefinition of what we usually mean by microbes. Therefore, the description of how Pasteur fabricates his microbes would fail to be an answer to the question how we came to believe in the existence of what we usually call microbes. This is a good reason to avoid such a redefinition. And unless we demand a definition of microbes along these lines, there is no reason why our own definition of microbes – and to be sure: we don't need something fixed and Platonic here; just think of something that covers 'the practices that determine when to speak of microbes; the states of mind we are in when we read the word', or whatever you think constitutes something like a definition – should be disqualified from being used in a study on 19th-century science only because it does not completely follow from the specific 19th-century network.

The assumption here, which Latour would probably find far too modest and unexciting, is that as historians we do not have a monopoly on the creation of ontologies. I assume that by and large, historians use the concepts available in their culture: they do not have the goal of radically changing our perspective on the universe we live in, of rewriting from scratch the list of entities that populate that universe. They have the rather modest aim of understanding a bit of the past – for its own sake but not therefore (because that is an impossibility) on its own terms, using whatever current-day explanatory concepts they can use without anachronism as defined by the current usage of those concepts and our current ideas about the past.[480]

This does not preclude the possibility that what they find in the sources modifies those concepts; it is important that it can, as will be discussed more extensively in chapter 8. It just means that their ontologies are not created merely by the historical sources that happen to form the basis of a particular historical study. They have been created by history, but this history is always larger than the aspects of it that are being investigated in one specific research project. Our concepts come with beliefs about the world and with expectations about which circumstances need to be met in order for the things that those concepts refer to, to exist. Those beliefs and expectations are usually not the *result* of one

479 Cf. Rule 5 in Latour (1987, 258).
480 Tosh (2003; 2007, 198-209).

specific case-study (which would be the only proxy to their arising from the past local networks themselves, which are by definition inaccessible), and there is no reason why they should be.

But – retracing our steps further – if Latour has to concede this much (which is simply that there needs to be no wholesale *a priori* rejection of our inherited ontology for the sake of exotic local ontologies), then there is more that has to give. For if we are allowed to bring our own concept of microbes to our history of the 19th century, rather than being stuck with 'the things that are fabricated by Pasteur and added to the network', then what *our* concept means may be something *different* from what *we* think Pasteur's concept meant – even if this meaning, to paraphrase Putnam, was not just in Pasteur's head, but was a property of (what *we* think were the relevant aspects of) the entire local network. That is, the history of microbes can, again, have a relation to the history of the *concept* of microbes other than of identity. The things that Latour glued together to look like one thing immediately fall apart again.

6.6 Nature, Politics, and Critical Science Studies

When we phrase it carefully, it seems so simple: what *we* mean by microbes now is something conceptually distinct from what *we* think went on in 19th-century laboratories, farms, and statistics-gathering institutions – even if (no, *especially* if) it is not separable from what goes on in *21st*-century laboratories *et cetera*. In fact, this is something that would have required no explanation to David Bloor or Harry Collins, as we saw in the previous chapter: the distinctness of objects and beliefs informed their program of explaining beliefs independently of objects.

The major reason Latour has tried so hard to conflate these things and to deconstruct or hide the distinctions, is his aversion to the mutual isolation of subject and object in what he calls the modern Constitution, of which the separation of microbes and beliefs about microbes would be a clear example. I am sympathetic to his forceful and consistent insistence that objects and beliefs do not have segregated histories that can be understood separately (and the whole previous chapter was an argument to that effect), but I think that he goes too far, and that this is in part because of his assessment of the political implications of the modern Constitution.

This is that the separation of things and people in this Constitution serves to "use nature to abort politics."[481] By separating reality into two domains – nature and society – and decreeing that one of these domains should be one of certainty and unity, and the other one of disagreement, the open-endedness of politics becomes unduly restricted. This Constitution, essentially, gives an awesome power to those who can bridge the gap – or who

[481] Latour (2004, 19 [italicization removed]).

can, in another metaphor now, leave the cave of society to look at nature and return[482] – because they are supposed to bring objectivity and unity to society.

> The subtlety of this organization rests entirely on the power given to *those who can move back and forth between the houses*. […] these few elects, as they themselves see it, are endowed with the most fabulous political capacity ever invented: *They can make the mute world speak, tell the truth without being challenged, put an end to the interminable arguments through an incontestable form of authority that would stem from things*.[483]

It is scientists who claim the power to represent things – and it is in this sense that, for example, Latour wants to call Pasteur political: not that the content of his scientific beliefs somehow mirrors his orientation in the constellation of human political interests, not that he sought to get involved in elections and law-making and used his scientific work for that purpose; but that he put himself forward as a spokesman of forces that he had made visible (the microbes).[484] Because the scientists speak for objects to which they have exclusive access and which are so construed as to be unable to speak in any other way than through the scientists,[485] what they say acquires a disproportionate authority that serves to depoliticize science and immunize it from criticism. "Like all modernist myths, the aberrant opposition between mute nature and speaking facts was aimed at making the speech of scientists *indisputable;* thus, this speech passed through a mysterious operation resembling ventriloquism, to 'I speak' to 'the facts speak for themselves' to 'all you have to do is shut up'".[486]

This is an interesting perspective on the source of authority of the natural sciences in modern societies, and the political aim to draw science from a realm of immunity to criticism to one of pluralism and open-endedness is an understandable and, if I may say, sympathetic one. However, the question is whether the diagnosis is right and whether Latour's metaphysical radicalism is a proportionate and effective antidote. The separation between science and politics is nowhere as strong as in Max Weber's lectures on *Science as a vocation* and *Politics as a vocation* – in that sense, they are excellent examples of the modern Constitution. However, their separation here is one not between things and people, but between 'is' and 'ought': science seeking to provide knowledge about what is the case, and politics deciding what, given this information, should happen.[487] There is a plurality of possible answers to both questions, given that which questions science seeks to answer is decided by values that are, in the end, subjective; but true answers to the question what is

482 Latour (2004, 13-14).
483 Latour (2004, 14).
484 Latour (1983, 156-159).
485 Latour (1993, e.g. 30-32).
486 Latour (2004, 68).
487 Weber (1919a; 1919b). Latour (2004, 115) makes the same distinction, but it remains associated with the divide between nature and society.

the case are indeed, in the end, logically in harmony because there is just one reality, whereas answers to the question what should happen can be in conflict, and are therefore the domain of power struggle – of politics.

Latour should not forget that the social sciences, too, strive for objectivity: they seek to 'represent' people just like natural scientists seek to 'represent' things – and this goes to show the double meaning of 'representing':[488] claiming to say true things about X is different from claiming to defend the interests of X – the second is clearly not implied by the first. Still, it is possible to say that, in practice if not in theory, reality, claims about what reality is like, and claims about what reality should be like, are interwoven in such complicated ways that it is misleading to subject them to completely different kinds of expertise in society, with completely independent sources of authority and mechanisms of control. Latour may be right to that extent.

However, I do believe that he makes a severe misjudgment when he tries to put the blame on nature, on the idea of a shared external reality. "When the most frenetic of the ecologists cry out, quaking: 'Nature is going to die,' they do not know how right they are. Thank God, nature is going to die. Yes, the great Pan is dead. After the death of God and the death of man, nature, too, had to give up the ghost. It was time: we were about to be unable to engage in politics any more at all."[489] The reason why Latour cheers the death of nature, as we understand now, is because supposedly apolitical, it has inescapably served political goals – since of course the river is *not* separated in two banks, and the separation of science and politics is therefore inescapably something of a fiction: "never has anyone appealed to nature except to teach a political lesson."[490]

Different radical conclusions may be drawn from this: the modernist, Weberian one would be to say that, if this is the case, science and politics have not yet been separated *enough*; the other, Latourian one would be to say that the modern Constitution is dead and something completely different should come in its place. Any other normative conclusion could be built on the same insight, including the more moderate conclusion that things should go on roughly as they do now. An interesting question at this point is which move would be most conducive to the possibility to subject science to external criticism. This, after all, seems to be Latour's main point in his rejection of nature: "if we have to give up nature, it is neither because of its reality nor because of its unity. It is solely because of the short-circuits that it authorizes when it is used to bring about this unity once and for all, without due process, with no discussion, outside the political arenas, and when something then intervenes from the outside to interrupt – in the name of nature – the task of gradually composing the common world."[491]

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[488] This undermines the symmetry suggested in Latour (1990, 154-159). Here as before, Lynch (1996, 249-250) has a point when he suggests that Latour may be 'bewitched' by his language.
[489] Latour (2004, 25-26).
[490] Latour (2004, 28).
[491] Latour (2004, 91).

Latour's program of studying 'science in action' and 'following scientists and engineers through society' comes down in the end to a prolonged attempt to deny scientists any shortcuts: to extract from them precisely what they actually do, without any myths or other embellishments. Latour's is the laudable goal of a criticism of *power*:

> People who speak of nature as if it were an already constituted unity that would make it possible to throw back onto social representations everything that calls for disunion – such people *exercise a kingly power,* the most important of all, a power superior to all the purple mantles and all the gilded scepters of civil and military authorities. I ask no more of them than one minuscule concession: since you have granted yourself the power to define what unites us and what drives us apart, what is rational and what is irrational, show us also the proofs of your legitimacy, the traces of your election, the motivations for your choices, the institutions that permit you to exercise these functions, the *cursus honorum* through which you have had to make your way.[492]

But is dispensing with nature in the sense defined in this passage a good way to increase the possibility for such criticism? I would say that it is precisely because of the intention to talk about a common world that communication, and therefore criticism, becomes possible.[493] If I say that the earth goes round the sun, and you say that the sun goes round the earth, the conclusion that apparently we live in two different solar systems is not one that fosters mutual criticism. Note that this is not an argument against the idea that we live in two different solar systems; only an argument to the effect that the assumption that we do is not a step towards a criticism of scientific authority. The 'minuscule concession' that Latour demands *is* a step towards that – an insistence on being able to retrace where my claim that the earth goes round the sun came from (and, possibly, to discard that claim depending on where it came from) – but this retracing becomes easier the more the worlds we live in overlap.

In that sense, the strategy that we saw Harry Collins take in the previous chapter – to look at what scientists claim to be talking about, and then proceed to show that the dynamics of scientific decision-making can often be understood in terms other than those that scientists claim are important – is much more critical than the strategy that Latour advocates: to criticize absolutely nothing because actors are supposed to build their own ontologies.[494] Only when we agree (however tentatively and potentially subject to revisions based on new insights in either nature or history!) on what scientists are trying to do can we discuss questions as to whether what they do fails, succeeds or goes further than they claim to, if we are interested in such questions.

It may be that SSK and Latourian actor-network theory both fail in making historical knowledge about science a departure point for possible criticism. If SSK fails to do

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[492] Latour (2004, 222).
[493] For a similar view, see Rescher (1987, 136).
[494] Cf. Collins (2012).

so, it is because in refusing to refer to nature in what it says about science, it cannot account for those features of science that relate to nature. When it says: "you think you are talking about nature, but in fact what you do refers only to society", it states not its conclusions but repeats its axioms – axioms that, moreover, cannot be plausibly upheld, as we saw in the previous chapter. Nonetheless, SSK provides the possibility of saying that what scientific actors want or claim to achieve is not actually what they are achieving – the claim that scientists' actions are oriented only upon the social world rather than upon the natural world is already an attempt at criticism. Subsequent descriptions of the nature of this orientation upon the social world – e.g. based on furthering one's own professional interest rather than on finding out truths – might constitute a further criticism. Latourian actor-network theory fails in a more fundamental way, because it collapses the external in the internal; it conflates nature with what happens in local networks, and thereby takes away even the conceptual possibility that what is said about the world is not what is actually going on in the world.

6.7 Science and the World

This, in fact, is a repeatedly stated aim of Latour: to remove the *problem* of the relation between science and the world; to show that the way scientific thought relates to nature is not something magical or unreasonably effective, but a direct result of their co-creation. "Most of the difficulties associated with science and technology," says Latour, "come from the idea that there is a time when innovations are in laboratories, and another time when they are tried out in a new set of conditions which invalidate or verify the efficacy of these innovations. This is the 'adequatio rei et intellectus' that fascinates epistemologists so much."[495]

It is now clear where Latour thinks the fascination comes from, why according to him the *explanandum* does not exist, or at least not in such a radical form, and why it is important to make this point and to reinterpret the problem. The fascination results from the idea that something constructed by humans in society can 'match' something that has come to exist completely independently of those humans. This leaves the huge problem of understanding how the gap between subject and object has come to be bridged in such an effective way. Meanwhile, the experts whose job it is to bridge this gap derive a huge amount of authority from their success at doing so. All of this, however, makes sense only under the assumption of the 'bifurcation of nature':[496] the idea that nature and ideas about nature exist separately, like the two banks of a river, and that their connections are problematic.

Latour thinks that we tend to see scientific theories as abstract objects floating above the (empirical or experimental) world. When they then turn out to be 'applicable' to

[495] Latour (1983, 155).
[496] Latour (2008a, 10-13).

what happens below them, it seems to be miraculous. In fact, Latour says, they function in no more miraculous a way than any other centers or hubs within networks: "Miracle indeed to see a clover-leaf intersection fitting *precisely* with the freeways whose flow it redistributes!"[497] It is, of course, not a miracle, because it is evident that freeways and their intersections are constructed to match each other. In the same way, microbes and the laboratory environment that identifies them are constructed in the same movement, and when Pasteur's predictions 'work' outside the laboratory, this is the result of work done to extend the reach of the laboratory; "it is […] *not* a miracle."[498]

Especially, it is not a miracle that takes place in the minds of the scientific genius: if there is this miraculous match between reality and theory, an unexplained *adequatio rei et intellectus*, it may seem like a good strategy to take a close look at the minds that have generated these adequate ideas. For Latour, this is looking at the wrong place, and he devotes one of his rules of method to this point: "before attributing any special quality to the mind or to the method of people, let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors."[499] Latour's move has, again, a bit of a Cartesian flavor to it: apparently we could try to explain science by the special intellectual capacities of some humans, but it is much better to explain it by the movement of things.

Thus, in this methodological materialism or monism, the dualist problem of the correspondence between intellect and world disappears. What are we to think of this position, in the light of the preceding discussion with Latour in this chapter? The most important conclusion would be that, in rescuing and affirming the possibility of divergence between what we think about microbes or elementary particles and how we think past scientists thought about these, or between how our network fabricates microbes or elementary particles and how our network fabricates past networks containing these, we have placed a wedge between Latour's monism at an important point: the structure once again cracks neatly into its two constituent parts of (our views – always our views – of) the world and (our views of) its past representations.

What, then, remains of Latour's glue- and patchwork? It is important to note that we did not place the wedge at the point that matters most to Latour: the conceptual split between nature and society. He can be satisfied to see – and we have learned this from him – that we have remained undecided about how much of our own representations of things is 'due to' nature and how much is due to society. We need to think of our notion of microbes as neither culture-free nor nature-empty, and the same holds for past notions of microbes. We are even permitted to think of past microbes 'themselves' in ways other than as things on one side of a nature-society-divide – I say 'permitted', because this would be the point

497 Latour (1987, 242).
498 Latour (1983, 150).
499 Latour (1987, 258).

where I might like to opt out and permit myself instead to say that microbes 'themselves' are natural and not part of human society.

But that is not the point now. The point is rather this: the relation between science and the world can, again, become a problem – a status that Latour would deny it. But this is not the problem of a pre-established harmony between nature and intellect, a problem to which there would indeed be no mundane answers; rather, it is the problem of how one conceptually delineated part of the world relates to another conceptually delineated part of that world. (The reason why these are two conceptually distinct parts of the world is not that they are substantially different, but 'simply' that we relate to them in different ways.)

Nor is this problem a problem of correspondence, mimesis or adequacy. Almost all the views we have come across in the last two chapters commit the straw-man fallacy of identifying the view that reality independently constrains views about itself with the idea that scientific theories correspond to the world, and reason from the untenability of correspondence to the omnipotence of society (in Bloor's case) or to the untenability of a nature-science distinction (in Latour's case).[500] Once pointed out, it is easy to see that this reasoning fails, for the simple reason that science *could* have another relation to the world than one of 'matching' or 'corresponding'.

What relation, then? Can we say things, in general, about ways in which the world can *constrain* science – exercise causal influence upon it? And can we do this while taking into account Latour's lesson that we should not overlook the fact that 'science' and 'nature' are not causally isolated entities with one or two designated points where bridges between them may be built – while taking into account, in other words, that we have only conceptually distinguished them, and that in fact they are connected by causal interaction throughout history on an indefinite number and variety of occasions?

Perhaps a promising way to do this is by seeing science as adaptive to nature, in a way analogous to the adaptation of life to its natural environments. In that case, after all, there is no problem in saying that we can conceptually separate animals, for instance, and their natural environment; or in saying that these animals rarely 'correspond to' their environment simply by 'representing' or 'mimicking' it (instances of camouflage notwithstanding of course). Both these claims can be harmonized with the idea that environments underdetermine what animals live in them, *and* with the idea that there are indefinitely many local interactions between animals and their environment, rather than a bunch of animals on the one hand and an environment on the other which are subsequently 'bridged'.

Last but not least, there may be a hint here at how to solve the problem of their relation without resorting to miracles. After all, it may seem that animals are miraculously well attuned to their environment – just like it may seem that science is miraculously well attuned to nature (or that clover-leaf intersections would be miraculously well attuned to

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500 Cf. Galison (1987, 11) for a similar point on Andrew Pickering.

freeways were we not allowed to assume intelligent design by humans here). But in the case of the biological world, we have access to theories that provide completely naturalistic explanations of this adaptation – explanations that have in common that they do not approach animals and their environment as having come to be independently and complete in their current shapes, but that the shape of animals (and of their environment) is the result of a historical evolutionary process.

In the next chapter, we will explore a family of approaches to the relation between science and the world that attempt to grasp this relation in a naturalistic, causal way.

6.8 Conclusion

From our evaluation of Latour's perspective on the study of science in history, we can draw the following conclusions:

1) Latour has successfully undermined an essentialistic opposition between nature and society, where science belongs wholly to the social domain and its relation to nature corresponds to the bridging of an ontological gap. He has done so especially by appealing to the constant interactions between human and non-human entities in networks.
2) Latour succeeds in showing that there are things that are fruitfully considered as *both* robust *and* a historical product – both 'real' and 'fabricated'. This can be the case for the content of our science as well; its explanation is then properly historical.
3) Latour's conflation of the world with its representations does not hold up to scrutiny. This means, first, that it is still possible to talk about the relation between science and the world that it is about, and, second, that there is no reason to believe that our representations of the world are causally anachronistic with respect to periods in which they are unavailable.

Chapter 7: The Invisible Hand of Science

7.1 A Naturalistic Perspective

So far, we have looked at a few different ways in which to talk about the relation between science and the rest of the world: the first spanned chapters 4 and 5 and looked at what happens if we suppose that nature makes the final state of science inevitable (chapter 4) or, on the other hand, completely fails to play an explanatory role (chapter 5). Then, in chapter 6, we looked at an attempt to create a vocabulary for talking about science without dividing up the world in natural and human elements with their corresponding features.

In this chapter, we will look at naturalistic accounts of how the world influences science, namely at invisible hands accounts, including most notably the evolutionary account of science provided by David Hull. We have already seen in previous chapters that it is conceivable for nature to play a causal role in the history of science even if there is no logic that completely and unequivocally determines how what is given to us by nature gets processed by science. A naturalistic account of the relation between science and the world may do justice to that observation, by foregoing reference to transcendental rationality, idealism or laws of history, and looking only at causal relations.

In doing this, naturalistic accounts may also succeed in closing the ontological gap that is created by the division between 'what science studies' and 'what science is part of': in the previous chapter, we by and large agreed with Latour that to artificially cut all the connections between 'Nature' and 'Society' belies the fact that these categories are the result of constructive work and artificially inflates the problem of the relation between science and the world, by suggesting that they are different *kinds* of things. We did not agree with Latour that by deconstructing the self-evidence of the boundary between nature and society the question about the relation between science and the world collapses entirely: we may still make a distinction between a natural phenomenon and what a scientist (or a discipline, or a culture) believes about that natural phenomenon, and we may ask the question how these two facts are related. The point is that in answering this question, we can assume that both facts are about objects in the world: both natural phenomena and scientific beliefs and practices are particular and concrete entities, which may have a causal relation to each other.

A naturalistic approach, then, may help in treating both science and the world as referring to entities that are in the same causal nexus. Hull calls his approach 'naturalistic' if "naturalism is the 'view that theories come to be accepted (or not) through natural processes involving both individual judgment and social interaction'".[501] The intended message seems to be that what scientists do does not take place in complete independence of the world that their work is about; that the judgment they exercise and the interaction they engage in do not need to cross a broad river, a deep ontological gap, but that they are simply among the actions that take place in the world. Saying that individual and social actions are part of

[501] Giere (1988, 7), as cited (though with another page number) by Hull (1988b, 3).

nature is not the same as saying that they are the proper domain of the natural sciences; it can also simply be to counter the intuition that they are 'made of radically different stuff', and thereby to weaken the 'bifurcation of nature' that we saw Latour oppose in the previous chapter. What makes an approach naturalistic is then the notion that the social is a subset of the 'natural' in a broad sense of that word; not something different from it, but not necessarily indistinguishable from the *rest* of the natural either.

In this chapter, we will zoom in first on invisible hand accounts, and then on an invisible hand account that is also an evolutionary approach: the account of scientific development formulated by Hull. Of course, there are naturalistic approaches that are not invisible hand accounts, and there are invisible hand accounts of scientific development that are not evolutionary accounts – we will meet some examples of the latter. Also, it is well conceivable that there are evolutionary accounts that do not meet the criteria of invisible hand accounts. However, both invisible hand accounts and evolutionary accounts have properties that make them interesting from the perspective of a philosophy of history of science, and their combination in Hull's account is therefore especially interesting, as we will see.

7.2 Invisible Hand Accounts

7.2.1 The Promise of Invisible Hands

One type of approach that tries to link science to the world by processes that are 'natural' in the sense described above can be filed under the label of 'invisible hand explanations'.

The promise of invisible hand explanations lies in the extent to which they might harmonize belief in the authority of scientific opinions about nature with an emphasis on the thoroughly social nature of science. One thing that SSK and similar approaches have been very good at is 'unmasking' scientific ideologies, and bringing the actors in science back to worldly proportions: it has shown time and again that the persons whose aggregate actions constitute science are not ascetics motivated purely by a desire to find out and preach the truth about nature – that, in fact, science is all too human, and that we had better approach it not as if it were something pure, but rather "as if it was produced by people" who were, among else, "struggling for credibility and authority."[502] Though we should not underestimate the extent to which previous generations of scholars in science studies were capable of apprehending this – was not Mertonian sociology doing precisely this? – this attitude is a genuine improvement over approaches that had to see scientists and science as disinterested and unattached to society,[503] if only because a sustainable place *in* society for people who are genuinely (as opposed to mythically) detached from society is hard to find.

And so, to state it bluntly: if what scientists are systematically striving for is not finding out the truth about nature but pursuing their social interests, then perhaps science is

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502 Title of Shapin (2010).

503 E.g. Dubos (1950).

not about finding out the truth about nature after all, but about pursuing the social interests of the groups that do science. This blunt conclusion, however, is also rather cynical, to such an extent that it renders inexplicable the success of science in dealing with nature. SSK can set out to explain *belief* in the success of science, including our own belief, as a result of social processes; but because it leaves nature out and orients scientists upon society rather than nature, it has no way of making sense of the possibility that science is 'actually' successful in its attempts to investigate nature. Again, as has been emphasized already in chapter 5, this does not imply unbelief in the existence of an outside world, or idealism; it means rather that we are never in a position to link science to this outside world.

What invisible hand mechanisms may be able to do, now, under conditions that we will address shortly, is to recognize and affirm the demythologization of the people and institutions that make up and carry science – to say that scientists are fully the social beings that other people are, with the same drive for money and status as all of us (or at least within reasonable distance to the average on the same bell curve) – but to move back into the picture a notion of dealing with the world and even of success in dealing with the world for science as a whole, by presenting these not as matters of teleology and design, but as the emergent results of social structures in science. As Petri Ylikoski, one (critical) commentator, has summarized the reasoning behind invisible hand approaches to understanding science:

> one can say that scientists are *humans without a great secret of success* (that is to say without the Scientific Method). So we might have to get rid of some {of} our usual ideas about the nature of science. Are the ideas of objective knowledge and of the cognitive authority of science among these? The idea of the invisible hand is supposed to save us from throwing them away along with other things. It refers to a naturalistically acceptable process, in a way that a naturalistic philosopher of science can accept it.[504]

An invisible hand process can be defined as a case in which the actions of individuals lead to a stable and understandable order that was not necessarily intended by them.

In this way, invisible hand explanations distinguish between immediate appearances – what people think they are doing, what they seem to encounter while they are doing these things – and the causal processes that actually lead to the results. For instance, people may all be interested in their own profit rather than in maximizing collective utility, and yet there may be mechanisms of which their own selfish actions are a part that ensure that the latter happens – mechanisms of which no individual needs to be aware and the working of which is not explained by individual intentions.

We can see how this is a promising answer to some of the arguments mentioned in the previous chapters (see, for instance, the section on Karin Knorr-Cetina) where it was said that scientists do not encounter nature in their everyday work in the laboratory, and are usually more occupied with 'making things work' in their dealings with all kinds of actors

[504] Ylikoski (1995, 36).

and materials than with 'nature' or 'truth' (section 5.4.2). An invisible hand account may grant this, but add that it can replace this level of the immediate experience of the actors with a 'deeper' level, and provide us with mechanisms that ensure that the aggregate actions of these pragmatic individuals progressively lead to a better account of the natural world.

It may even be better to speak of three levels, since there is also a level at which scientists do see themselves as being occupied with realizing values like truth or objectivity. The point of ethnographers like Knorr-Cetina is that this is a misinterpretation of what 'really' happens in the laboratory, where truth and objectivity seem to play no causal role. An invisible hand explanation counters this reading by one where what seems to happen in the laboratory is itself not what constitutes the real order behind the working of science.

7.2.2 An Economic Account: Alvin Goldman

Invisible hand accounts of science are associated with neoclassical economical language,[505] but they clearly do not overlap with laissez-faire economics.[506] Alvin Goldman, for instance, who has developed theories that can excellently be classified as invisible hand mechanisms, has written rather critically about the extent to which pure market mechanisms favor epistemically desirable outcomes. We will look briefly at this argument in order to get a clearer and concrete picture of what invisible hand theories are and what they are not. Goldman, together with James Cox, investigates whether a free market for speech or ideas is the optimal solution for encouraging the production of true beliefs. The measure for truth possession, in their definition, is the number of true beliefs divided by the total number of beliefs. Under such premises, truth cannot be *defined* as the result of free competition;[507] just like in other markets, the measure of the quality of the products needs to be independent in principle of market mechanisms. Like in other markets, economic theory does not imply that the products made under free competition are of the highest quality, but that they are produced most efficiently relative to production possibilities and consumer preferences. It does not categorically predict what these goods are.[508]

A complicating factor in the case of truthful information is that perfect information is usually regarded as a condition for market functioning, not as a result. Even markets for regular commodities can fail under circumstances of imperfect information, Goldman and Cox explain. If markets are unregulated, the costs of checking the truthfulness of information lie with the consumers.[509]

The most noteworthy aspect of this account is the strict requirement that truth be defined *independently* of the outcomes of the social and economic process. This is a major

505 Davis (1998).
506 Leonard (2002, 152-155).
507 Goldman and Cox (1996, 1-8).
508 Goldman and Cox (1996, 17-20).
509 Goldman and Cox (1996, 20-24).

point in Goldman's 'social epistemology', and one that he sees as the main point of difference with the 'social doxology' of scholars like Shapin, who are, according to him, uninterested in truth.[510] Goldman leaves no misunderstanding about his insistence that truth is about the world, that "only the world confers truth and falsity".[511] A large part of his *Knowledge in a Social World* is devoted to substantiating this realist claim.

An article by Goldman and Moshe Shaked, which provides an economic model of scientific truth, is a good example of an exposition of invisible hand mechanisms and the extent to which they seem to need such a realist account of truth. The premise in this argument is that scientists try to maximize their individual expected utility, and that this utility is defined exclusively by professional success.[512] Goldman and Shaked develop a rational choice model to formalize decisions of these credit-seeking agents about which experiments they are going to perform, given certain subjective probabilities regarding world states and given the subjective probabilities of other scientists, and assuming they try to maximize the expected credit they are going to get by modifying the subjective probabilities of others. They conclude that under most circumstances, the probability revisions will lead to more accurate beliefs; only if the initial subjective probabilities are absurdly inaccurate will the experiments lead to an increase of error.[513]

Goldman and Shaked's is effectively an invisible hand account, which sees scientists as credit-seeking and the amount of credit they receive as determined by their influence upon the beliefs of others. Their interests are socially defined. But their actions in pursuing these interests are not understandable unless some notion of accuracy is involved: scientists believe things about the world – that is, they are able to attribute more or less explicitly certain probabilities to statements about the world – and in their experiments, the world gives clues as to how accurate these probabilities are; moreover, scientists' beliefs can be rationally adapted to these clues, so that the world plays (to the third-person spectator who knows what the world looks like and what the subjective probabilities of the scientists are) a predictable role in scientific belief formation, as scientific belief formation will respond predictably to the clues it received from its (no less predictable) investigations.

Now, how we evaluate the effectiveness of this invisible hand account depends on what we believe it aims to achieve. To the extent that it is an account of how scientists can be motivated by aims other than an accurate description of reality while reality nonetheless has explanatory value with regard to the formation of scientific beliefs, it works. It does not try to give a radically new role to reality, let alone a new conceptualization of it; but that is not its point. Its point is to show how science can be successful in what it aims to achieve, even if

510 Goldman (1999, 7-9). For a critical assessment of Goldman's view of what SSK has to contribute to our understanding of science, see Kusch (2011).
511 Goldman (1999, 21).
512 Goldman and Shaked (1991, 31-32).
513 Goldman and Shaked (1991, 40).

scientists are not structurally motivated by a desire to further science but rather by a desire to further their own professional interests.

However, some of the criticisms raised by SSK against earlier rationalistic philosophies of science certainly apply to it.[514] After all, is this not just correspondentism, together with some very doubtful suppositions about scientific rationality? Didn't we already agree that evidence did not bear upon belief in an unequivocal way? What about the contingency of scientific concepts and categories? If we endorse Goldman's account, are we not simply replacing an individualistic rationalism with an almost identical rationalism on a social level?

These are questions that need to be addressed: they are important to assessing in what sense, and under what assumptions, invisible hand accounts provide an alternative to other perspectives such as SSK. We will now take a closer look, therefore, at the role of truth and correspondence in Goldman's 'veritistic' social epistemology as described in his *Knowledge in a social world*.

According to Goldman, a correspondence theory of truth is indeed the most natural account of truth. Alternatives do not work: pragmatist or instrumentalist theories which define truth in terms of desirable outcomes run into problems regarding the fact that what is desirable differs by person; verificationist approaches, which identify the truth of a proposition with its justification, have the problem that exactly the same proposition can be true first and false later. Coherence theories run into similar problems.[515]

Another possibility are deflationist accounts of truth, which explain statements about truth as performative actions, or instruments of semantic ascent, or see 'true' as a predicate while denying it is a substantive property. What these theories have in common is that there is no metaphysical relation between a statement and the world that 'makes' a statement true, but that truth is just a useful linguistic instrument.[516] Goldman, on the other hand, sees truth as something that requires *truth makers*. According to classical versions of correspondence theory, these truth makers lie in a structural isomorphism between fact and world, but Goldman claims that his theory doesn't need this.[517]

If he is right about this, then the correspondence on which Goldman's invisible hand account rests may be able to avoid becoming a kind of 'mirroring', the kind of representationalism that SSK advocates often accuse their opponents of adhering to. Perhaps we can retain the realist intuition behind Goldman's invisible hand account while providing a more subtle analysis of what the role of 'reality' in this account actually entails. In the following I provide an interpretation of Bernard Williams' *Truth and truthfulness*, in which he does precisely that.

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[514] Cf. Kusch (2011).
[515] Goldman (1999, 41-48).
[516] Goldman (1999, 51-53).
[517] Goldman (1999, 59-62).

7.2.3 Accuracy and the Resistance of the World: Bernard Williams

Williams makes a broad philosophical argument for the idea that it is meaningful to want to find out the truth on an issue; if someone wants to do this, "we can say that this is equivalent to his wanting to get into the following condition: if P, to believe that P, and if not P, to believe that not P."[518] According to Williams (referring to Goldman), some methods of inquiry have the property of leading to true belief (are truth-acquiring), and some have not, and this is what is meant with accuracy.[519] Williams goes on to say that it is important that there are external and internal obstacles to finding truths, and that this suggests a realist idea of truth, in the sense of "an independent order of things to which our thought is answerable. […] It has often been recognized that the idea of a reality independent of us can involve an implication of resistance, resistance to the will."[520]

Williams goes on to observe that this idea of resistance has usually been related to physical objects (which can resist our movements), but that it seems to be that "any case of necessity will be an example of radical resistance to the will."[521] We cannot change truths about the past or about mathematics either; but Williams goes on to connect the notion of independent reality specifically to those states of affairs to which there is a conceivable alternative. This means that he can distinguish between the status of truths about, for instance, the past – of which we can wish that it had been different, but cannot begin to think that we can *do* anything that would make it different – and the status of mathematical truths – of which we cannot even conceive of what would be involved in other things being the case.[522] The Pythagorean philosophers may have wished that the square root of two was not an irrational number, but only if they did not think in a determinate or focused way about what this desire involved.[523]

I should remark at this point that this difference as based on Williams' terms may be less clear than he suggests: do we really know precisely what would be involved in other truths about the past (or about other parts of reality that are independent of our will)?[524] But this does not undermine Williams' larger point, that "it is the sense of conceivable alternative that is particularly associated with realism. Realism invokes the idea of an order of things that is independent of us, where that means, in particular, independent of our will."[525] Accuracy, as a virtue, is resistance to subversion of truths about this independent reality by the wish.

Like the invisible hand theorists (to which Williams' own genealogical account of the primary virtues of truthfulness bears some resemblance in its attempt to show how these

518 Williams (2002, 133).
519 Williams (2002, 126).
520 Williams (2002, 136).
521 Williams (2002, 136).
522 Williams (2002, 137-139).
523 Williams (2002, 139).
524 Cf. Rescher (2008, 113-126).
525 Williams (2002, 140).

desirable features of thinking can be thought of as having emerged out of non-intentional processes, though it goes further than just being a functional account),[526] Williams believes that if science possesses the virtue of accuracy, this does not mean that scientists themselves live up to a Platonic ideal of personal disinterestedness, or that abstract natural science itself liberates from interestedness by transcending human affairs.[527] The crucial issue is the question whether the thing scientists are interested in – even if this is a socially constituted good like prestige or power – depends on their succeeding in finding truths about nature, "just as those who in the ancient world or in the Renaissance sought fame through writing notable verse recognized that they would not achieve it without the notable verse."[528] The virtue of accuracy would be undermined if scientific recognition were itself a function of an antecedent social position; but that this is the case the sociology of knowledge has failed to demonstrate, Williams says.

"Science is, in game-theoretical terms, not a two-party game: what confronts the inquirer is not a rival will, and that is the key to the sense of freedom that it can offer. To be free, in the most basic, traditional, intelligible sense, is not to be subject to another's will. It does not consist of being free from all obstacles."[529] The world, in Williams' account, does resist; but the status of this resistance in Williams' thought is completely opposed to that in Latour's, since the resistance of the world and the objects it contains cannot be thought of as agency without the dualism between science and the world breaking down and the game becoming a multi-party game again.

7.2.4 The Limits of Normative Invisible Hands Accounts

These invisible hand accounts are not a solution to the problems posed by SSK concerning the relation between nature and society, let alone to the challenge of ANT; rather than answering these problems, they have to ignore them and 'revert' to a dualism between society and nature. This is not necessarily a problem in itself: as we have repeatedly observed, invisible hand accounts are there to show how nature can figure in the explanations of science even if it does not figure in the intentions or the immediate experience of scientists. This means that they are allowed to be conservative or commonsensical about the nature of the external world.

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526 Williams (2002, 20-40).

527 Williams (2002, 141-142).

528 Williams (2002, 142). Cf. Brown's (1989, 78-81) spin on the idea that scientists are motivated by peer recognition, by a comparison with capitalists who are motivated by making money: "But do successful capitalists only make money? Don't they also make carpets and clothes pegs? And, in fact, don't they make money because they make artefacts?". A similar point is made by Papineau (1988, 53) within the context of an argument that naturalized epistemology can accommodate Edinburgh-style sociology of science and its symmetry principle, as long as the reliability of science is not considered to be explained sufficiently by exclusively social factors, but also by "the facts that scientific beliefs are supposed to be about" (51).

529 Williams (2002, 144-145). Cf. Dennett (1984, 50-73).

Both Goldman's and Williams' invisible hand accounts need to employ a concept of accuracy – something which, if it is supposed to say something substantive about the relation between nature and beliefs about nature, is closely related to correspondence and will in that case need to answer a whole range of skeptical and pragmatist objections.[530] If accuracy does not pertain to the relation between reality and belief but purely to a virtue within science, this problem does not arise, but it becomes less clear what the conditions for this virtue are and how we can recognize them.

Goldman's and Williams' accounts are inevitabilist, in the sense that science converges to a more truthful account of what nature looks like. This holds especially for Goldman's correspondentism, if interpreted descriptively: as the claim that beliefs about the world will converge to a more truthful account of it if science works by and large according to Goldman's social epistemology, combined with the claim that science by and large works according to that epistemology. If this is the case, if we want to understand why scientists believe what they believe, we need to know only what the world they are studying looks like.

However, this is the case only if Goldman provides an account of scientific rationality in which the normative and the descriptive coincide: otherwise his mechanisms either lose explanatory value, or they shed light only upon fictional, idealized developments in science.[531] In fact, Goldman is clear about the fact that his project is a social *epistemology*.[532] It is not primarily an account of how actual science works; it is an account of how social belief-forming practices of a certain kind can lead to more accurate beliefs about an existing reality. Similarly, Williams is not providing a history of science; he is providing a genealogy of accuracy and sincerity as virtues of truthfulness, insisting that these virtues presuppose 'obstance' by an independent reality.[533]

The existence of these plausible fictional accounts could strengthen belief that plausible actual accounts based on similar presuppositions might also be within reach. Moreover, these descriptive accounts need not necessarily be inevitabilist. We will look at one project that explicitly identifies as an invisible hand account,[534] but aims at the *explanation* of scientific beliefs rather than at their justification,[535] and does so under the

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530 E.g. Rorty (1982, xiii-xxii).
531 See Kusch (2001, esp. 188-190), a criticism of Goldman (1999), where Kusch says that Goldman does not consider "what kinds of belief or knowledge are necessary for norms or social institutions to exist in the first place" (188).
532 Goldman (1999, 1-9).
533 Williams' employment of a 'genealogy', incidentally, belies the assumption made by Bevir (2008) that genealogies are to be associated with radical historicism.
534 Hull (2001b). In Hull (1982, 273), he says that "I find a scientific theory of sociocultural evolution a vastly more significant goal than an evolutionary epistemology" Cf. also Hull (1988b, 12-13).
535 Hull (1988b, 12-13). According to Munévar (1988, 211), Hull cannot maintain that his goal is restricted to explanation rather than justification if he speaks of rationality, but he also fails to deliver an evolutionary epistemology. For similar reasons, Grantham (2000, 449) says about Hull what I have just said about Goldman: that his account "blends descriptive and normative claims" (cf. also 455-457). I

supposition that science is by and large very good at "realizing its manifest goals".[536] This is the project of David Hull, who has tried to look at scientific development as analogous to biological evolution, and subject to similar selective mechanisms.

7.3 David Hull's Evolutionary Invisible Hand Account

7.3.1 Science as a Process

As said, Hull's project is to give an evolutionary account of scientific development, rather than an 'evolutionary epistemology'.[537] That it is an evolutionary account does not mean that it tries to extend a biological vocabulary to conceptual developments.

Though Hull obviously proceeds from some presuppositions about the natural inclinations of humans as a species with regard to curiosity about their environment,[538] he makes it clear that strictly biological accounts fail to explain the kind of conceptual developments that science exhibits: after all, our natural tendencies when it comes to, for example, classification of plants, are patently unscientific, and wrong.[539] Hull, then, emphasizes that he does "not propose to extend a gene-based biological theory of evolution to include conceptual development in science. Instead, I provide a general analysis of selection processes which is intended to apply equally to both biological and conceptual change."[540]

Thus, Hull sees selection processes as something that can be defined independent of the specific substrate on which they operate.[541] The notion of a 'gene' is not just made into a metaphor which is subsequently applied to the notion of a scientific 'concept';[542] rather, we could say that both are involved in instances of a 'selection process' that can be abstractly defined – there is a weak hint of idealism here in what is otherwise a thoroughly naturalistic metaphysics, which, as we will see shortly, dispenses with essentialism about science in a very useful way.

The abstract terms involved in selection processes are those of replicators and interactors – a replicator being "an entity that passes on its structure largely intact in successive replications", an interactor being "an entity that interacts as a cohesive whole

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believe this relies on a confused image of what Hull is doing: the point is not to be able to justify scientific theories and methods on the basis of an epistemology built on evolutionist presuppositions, but to provide an explanation of how the social organization of science leads to an effective realization of its goals. Though this may in some contexts 'justify' this organization, Hull does not seem to want to provide such a justification. I believe, with Hull, that the distinction between Hull's explanatory aims and normative epistemology can be upheld here. See also Griffiths (2000, esp. 306-307).

[536] Hull (1988a, 98).

[537] Thus, Hull fits neither of Bradie's (1986, 403) characterizations of evolutionary epistemology.

[538] Hull (1988b, 14).

[539] Hull (1988b, 284).

[540] Hull (1988b, 20).

[541] Hull (1988a, 98).

[542] Hull (1988a, 109).

with its environment in such a way that this *causes* replication to be differential."[543] A process that differentiates between the fates of different interactors in such a way as to differentiate between the replication of different replicators, is a selection process.[544]

Selection processes influence the temporal change of *lineages*, a lineage being "an entity that persists indefinitely through time either in the same or an altered state as a result of replication."[545] Hull's 'lineage' is a key concept to grasp, as it provides an excellent point of overlap between philosophy of biology and philosophy of history; it is through this concept that historical entities can become the center of scholarly attention. This is of immediate relevance to history of science in more than one way.

In *Science as a process*, Hull deals with the controversies concerning the applicability of the term 'scientists' before the modern period, judging that "the terminological convention being suggested by purists is so patently silly that it hardly warrants refutation."[546] The larger issue at stake here, however, Hull identifies as being the choice between calling everyone a scientist who performed activities that we would recognize as science, or treating terms as referring to particular times and places. This issue he rephrases in turn (the rephrasing effecting a slight change in content) as "similarity versus descent."[547] Hull decides to look upon science as something general, and Western science as an instance of this.[548] 'Western science' is a particular instance of science in a very general sense, and as such we do not need to specify its formal characteristics; we just need to be able to identify it as a lineage. For purposes of delineation, its uniqueness or its similarity to Chinese or Greek science is neither here nor there, just like the question whether whales look like fish is irrelevant if we want to define whales as a species in the sense of a lineage.

What is the relation between lineages and selection processes? It is not tautological: the shape of lineages can result from something other than selection processes.

> Lineages are historical entities formed by replication. Differential perpetuation caused by interaction is not necessary for something to count as a lineage. In fact, differential perpetuation itself, regardless of its causes, is not even necessary for something to count as a lineage. However, when the interplay between replication and interaction causes lineages to change through time, the result is evolution through selection.[549]

This makes Hull's thesis that science is a lineage that evolves through selection into a synthetic claim, rather than the analytic one that it could have been.[550] It would have been an analytic claim if Hull had held, for instance, that in order for something to be part of

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[543] Hull (1988a, 109).
[544] Hull (1988a, 110).
[545] Hull (1988a, 110).
[546] Hull (1988b, 75).
[547] Hull (1988b, 77).
[548] Hull (1988b, 81).
[549] Hull (1988a, 111).
[550] Hull (1988b, 280-281).

science at all, it needed to be subject to selection pressures. A definition of science in terms of 'conjectures and refutations', or something similar, could lead to the claim that science evolves through selection being analytic. But Hull has not defined science as a class in any but extremely broad terms; and the 'lineage' of Western science has not been defined formally, but pointed to. This lineage changes over time – scientists believe, write and do different things now than before – and since historical change may be the result of something other than the interplay between replication and interaction, the question whether Western science has evolved through selection remains open.

7.3.2 Selection Pressures and the World

Much will depend, then, on the mechanisms that Hull identifies behind this supposed selection process. What is the thing that is being replicated differentially, what are the interactors, and with what do they interact?

Hull answers all these questions: the replicators are "elements of the substantive content of science – beliefs about the goals of science, the proper ways to go about realizing these goals, problems and their possible solutions, modes of representation, accumulated data reports, and so on".[551] Conceptual replication is "a matter of ideas giving rise to ideas via physical vehicles, some of which also function as interactors. Replicators are generated, recombined, and tested by scientists interacting with the relevant portion of the natural world."[552]

So to rephrase only slightly: the replicators are more or less abstract entities, which can exist in different physical vehicles; these physical vehicles, some of which are scientists, interact with the natural world, presumably in a differential way so as to cause differential proliferation of the scientific 'ideas'.

Things are complicated because science is a social process; Hull's account gets its subtlety from dealing with the social structure of the scientific process, analyzed through the concepts of *credit, use, support* and *mutual testing*.[553] Scientists act the way they do, not because they get the rewards for adequate ideas about nature directly from nature itself; they do not physically die sooner (or fail to procreate) if their theories about nature are inadequate.[554] Rather, what they strive for is recognition by other scientists;[555] they are

[551] Hull (1988a, 116).
[552] Hull (1988a, 117). For a discussion of in what sense the environment of science in Hull's model is the non-conceptual world, and the role of interpretation, see Gross (1988).
[553] Hull (1988b, 281).
[554] For Munévar (1988, 210), this is a sign that scientific change is, under Hull's assumption, not actually the result of a selection process. Cf. also Campbell (1988, 176), about the question whether selection in science does not work much more directly upon the replicators.
[555] Hull (1988a, 282).

differentially successful *as* scientists, in interacting with *other* scientists; and the extent of their success in this regard determines their ability to spread the ideas they hold.[556]

If the social (scientific) success of the scientists-interactors is dependent on the content of the replicators to which they are connected, the conditions for evolution through selection are realized. This is the case if science is organized in such a way that the goods that make scientists successful are in the end rewards for the content of the ideas that they hold. This is why it is crucial that scientists make use of each other's ideas, and test them.[557]

For this to be the case, it seems the community as a whole needs to interact with the natural world. At least Hull strongly believes that this is the case. Strictly, it is not necessary; it has been noted that Hull's analysis is applicable to any community of experts in which these experts are simultaneously sellers and buyers of the goods they produce.[558] These experts might be theologians just as well as physicists. In that case, the supposedly crucial role of the world drops out. It still makes sense to attribute differential success of theological opinions in the Middle Ages to the differential social success of the theologians carrying these opinions. But is it necessary to suppose that the successes of the interactors can be related to the content of the replicators; that the opinions of the more successful theologians bear a different relation to the Supreme Being than those of their less successful counterparts?

It seems not. There may be a different 'outside world' against which these theological opinions were tested by peers, of course; the texts of the Bible and the Church Fathers, for instance. But perhaps not even this is necessary, and perhaps we can just say that the differential success of different theologians holding different opinions can be related rather to particular historical circumstances – social, political, cultural – to which these opinions were better suited than those of competing schools. This keeps the entities causally relevant to the selection processes in the realm of society; it would be the church-historical equivalent of SSK. Since the social, political and cultural circumstances would of course be somewhat less stable than the Deity about which the medieval theologians intended to write, we will not see these theologians converging to stable opinions and reaching stable goals, even though the evolution of the lineage(s) of theological thoughts involves differential replication through differentially successful social interaction.

In order to follow Hull, similarly to Goldman and Williams, we need to believe already that it is possible for the success of scientists to depend on those features of the

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556 The duality and trade-offs between recognition and support resonate with the paradox that Latour's actors face according to Shapin (1988, 537-538).

557 Criticism of Hull's use of the concepts of replicators and interactors in science is often based on the idea that the relation between replicating scientific entities and interacting scientists is too unlike that between genotype and phenotype in biological evolution. Cf. Sterelny (1994, 50-52) on genes building their interacting vehicles: "An electrical engineer is not a voltmeter's way of making another voltmeter." (50) I think the analogy holds and is actually instructive: if usage of voltmeters increases the fitness of the engineer *as* engineer relative to other instruments, this will lead to a differential proliferation of voltmeters and other instruments.

558 Kantorovich (1988, 200).

substantive content of their theories which pertain to their relation to the natural world. Again, this is not necessarily a problem; we have established that the arguments that say we should completely omit the natural world from our account of science are unconvincing, and that it is very plausible that features of the natural world can be of explanatory value for the history of science. The question is where the natural world comes in and how its causal influence works.

Hull gives an example of causal links between the non-conceptual, non-social, natural world on the one hand, and scientists on the other:

> because I see a ball accelerate as it rolls down an inclined plane, I come to hold beliefs about the motion of balls as they roll down inclined planes.[559]

This is straightforward enough, if we don't read the condition here as a sufficient condition, and make no trouble about the 'seeing a ball accelerate' being hardly 'non-conceptual'. Observations can cause us to believe certain things, depending on how we are disposed to respond to these observations. Here I would like to call into mind what we discussed in chapter 3: that observations of nature can cause us to form certain beliefs is not the same as saying that nature on its own forces these beliefs upon us. After all, what kinds of beliefs we form when we see a ball roll down an inclined plane depends very much upon our previous beliefs, and upon our judgment of this observation, et cetera. From this quote, there is no reason to think that Hull does not realize this or that he disagrees; he is simply pointing out that there are obvious causal links between what we see and what we believe.

However, he also speaks about the role of the world in a more inevitabilist register: he finds himself believing, not just that events in nature are partial causes of opinions in science, but also that science gets progressively better at what it tries to do, and that this is because the things it is looking for in nature actually exist.[560] Laws of nature, for instance, are really there to be found:

> Conceptual evolution, especially in science, is both locally and globally progressive, not simply because scientists are conscious agents, not simply because they are striving to reach both local and global goals, but because these goals exist. If scientists did not strive to formulate laws of nature, they would discover them only by happy accident, but if these eternal, immutable regularities did not exist, any belief a scientist might have that he or she had discovered one would be illusory.[561]

The claim about the role that nature plays in science according to this quotation, I hope it is clear, does not follow from that in the previous quotation, though it also does not contradict it. In the gap between the two lies the question how nature gets to be not just a cause of

[559] Hull (1988a, 117).
[560] Cf. the discussion by Henson (1988).
[561] Hull (1988a, 124-125).

beliefs, but also something that beliefs in some way converge toward. How precisely do we need to read this, and how does this come about?

This puzzle, as Donald Campbell states it, is how it happens that "the beliefs of physicists come to fit the physical world they refer to".[562] Selection processes are an answer to this question, similar to the case in which we are puzzled by the whiteness of the polar bear which so well fits the whiteness of the surrounding terrain.[563] If we are not convinced of, or interested in, a striking 'match' between science and its 'surrounding terrain', like SSK scholars; or if we think science and its environment have come to be in the same movement, so that their correspondence is not a puzzle, like Latour, then the reference to scientists interacting with the physical world in order to test the ideas-replicators of other scientists becomes uninformative, an answer to a non-existing question.

Hull, like the other invisible hand theorists, believes that science is trying to do something and that it is successful at doing so. What is so appealing about Hull's approach, however, is that he foregoes the rationalism inherent in, for instance, Goldman's epistemology; he does not provide an account of what it is about the theories selected by science that made them fit for selection rather than their alternatives. Following Laudan,[564] Hull embraces the idea that it is not just scientific theories that change over time, but methodologies and goals as well – "the nature of science is constantly under negotiation".[565] It is not that the theories possess some ahistorical value like truth; it is not even that the scientists holding some specific theories excel at some transcendent virtue like 'accuracy';[566] no, the way to speak about this is to say that scientists are trying to increase their 'conceptual fitness', and this is a wholly contextual term, depending on what counts as 'fitting' to the relevant communities, and what counts as a successful test. Hull affirms the primacy of the use scientists make of terms over the philosophical analysis of these terms.[567]

Hull's sympathetic mention of Laudan and his insistence that the 'nature' of science is dynamic and under constant negotiation suggest that he believes that the goals of science are themselves part of the evolutionary process, rather than something that remains stable throughout the scientific lineage. This is indeed more in tune with a naturalistic approach than retaining the notion that science has always had the same final goals – except of course if it be in the broad sense needed to identify a particular lineage of opinions as 'scientific', which might involve a minimal requirement related to finding out things about nature. It is certainly not necessary for science to try to find eternal and immutable laws of nature; and if Hull (in the passage quoted above) really means to say that the progress of

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562 Campbell (1988, 175).
563 Campbell (1988, 175).
564 Laudan (1977). Laudan considers it useful to investigate the rationality of these developments over time (see Laudan (1977, 167-170) on the relation between this and Lakatosian 'rational reconstruction').
565 Hull (1988b, 297).
566 Cf. Downes (2000, 436-438), who employs Hull's account in a critical discussion of the link between truth and selection.
567 Hull (1988b, 298).

science he seeks to explain is progress towards a stable goal, namely the discovery of eternal laws of nature, then this belief would be hard to square with a belief that the axiology of science is under constant negotiation.[568]

This belief would also be just a bit too much like the belief that 'current polar bears are better at fur colors than their ancestors'. Polar bears as a lineage don't try to be white; they try to survive (– in fact, not even that claim is conceptually necessary; selection processes might explain the whiteness of the polar bear's fur without any polar bear ever really trying anything). Similarly, we are supposed to believe that scientists don't try to find eternal laws of nature; they try to increase their conceptual inclusive fitness (or at least, they fare better as scientists if that is what they do).[569] No or very little explanatory value is supposed to lie in their intentions to solve problems.[570] Under certain circumstances, convincing other scientists that you have found a law of nature, or testing or using claims by other scientists to this effect, may be the best strategy to increase your conceptual fitness. But these circumstances are not universally present: the beauty of Hull's account is precisely that it is so permissive when it comes to strategies that scientists can employ, and that it can recognize that the aggregate effects of these strategies can be a 'redirection' of not just the theories, but also the goals of science.

The 'progress' we are trying to explain, then – our observation that current science is so good at what it tries to do – is progress from *our* point of view; the point of view of the current aims and methods of science. This takes away some of the magic. If the substantial goals of science (as opposed to the attempt of scientists to increase conceptual inclusive fitness, which is potentially a general feature of scientists – or otherwise it is something that successful scientists turn out to have done[571]) have developed together with its other features, then these goals may be the result of strategic adaption to what is possible. In that case, we are like polar bears that realize how good we are at being as white as our environment, and who are puzzled by this fact. The solution to this puzzle is not the Latourian solution that our environment and ourselves result from the same process (because this is not the case); but part of the solution may lie in the realization that our *valuing* of being-white (which is a useful trait specifically in our snowy environment) is not causally independent of our *being*-white (which is the result of evolution through selection in our snowy environment).

To say this more bluntly: that we find our science to be so good at doing what we expect of it is not only because it has found better ways to do the things we expect of it, but also because our culture has grown to expect of science the things it simultaneously found out how to do. This is one reason why we reformulated the question of contingentism and

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[568] On this issue, cf. also the very lucid treatment by Grantham (1994).
[569] Hull (1988b, 282).
[570] Hull (1988a, 123).
[571] Heyes (1988, 194) notes that Hull's empirical claims suffer from a sampling problem, since most historical databases show only those scientists that succeeded in gaining recognition.

inevitabilism at the beginning of this thesis (section 2.1), to free our questions about historical causality and path dependence from our criteria of success.

This co-evolution of means and goals is a concept that will be especially appealing to historians who embrace the historicist intuition that scientists in different times and places are playing different 'games',[572] with goals that cannot be easily translated into each other. It may be the case that the underlying aim of early modern natural philosophy was the understanding and praise of the Creator, whereas modern scientists *qua* scientists usually do not have this aim. However, the essential incomparability of scientific goals should not be a postulate of history of science. Goals (and scientists or science need not have had just *one* goal at any time) may have been stable for some time in some respects: the ancient Greeks have never tried to split the atom, for obvious reasons, so in that sense they were not engaged (as a scientific community) in an intentional activity identical to that of some modern research communities; but on another level of abstraction ('trying to identify the fundamental elements of physical entities') they may have been pursuing the same goal. The verdict of progress will differ according to the ways the goal can plausibly be phrased, and according to our measure of technical progress in the realization of those goals. We are never just better at 'science' than the Greeks, just like polar bears are not better at 'fur colors' than their ancestors; we always need to find a plausible way of interpreting the purpose of what went on, then and now.

Much of what I have been saying here, including this last point, suffers from an equivocation of functionality and intentionality. The complex interplay we see developing here between individual intentions, the functioning and goals of science, and the blind forces of selection pressures, certainly merits further attention.

7.3.3 Scientists as Agents

If we are to provide an account of historical change in science in terms of evolution by selection, then we should be able to distinguish processes that cause mutations in replicators from processes that select these replicators.

Hull has emphasized that his account is not Lamarckian.[573] This he has done in answer to objections to analogies between biological and conceptual evolution: it is intuitively plausible that conceptual change is a directed process, since it is carried out by intentional agents. A Darwinian process is, by definition, 'blind'; usually this is taken to mean that genetic mutations are random. In a slightly less strict sense, it can be understood to mean that interactions with the environment do not influence the mutations of the replicators; their differential interaction with the environment (caused by differences in their corresponding replicators) only causes the differential proliferation of different replicators, and has nothing to do with the processes that lead to the differentiation of replicators itself. In biology, this means that those accounts fail to be Darwinian that fall short of 'hard

[572] Cf. the discussion of Cunningham in section 3.3.
[573] Hull (1988a). See Smith (1988, 216); Bechtel (1988); Heyes and Plotkin (1989, esp. 154-157).

heredity': if acquired characteristics can be genetically passed on, then the independence of replicators and interactors gets compromised.

It seems that most of the things that happen in culture are the passing on of acquired characteristics, and this may seem like a complication for a Darwinian account of scientific development. However, this is not necessarily so. According to Hull, the intentional agents are not the replicators, but the interactors; scientists are not reproducing themselves, but they are, through their interactions with the social and natural environment, causing differential reproduction of replicators. The memes are replicators, analogous to genes in biological evolution.[574] Just like the fact that some biological entities have intentions does not negate the Darwinian nature of biological evolution, the fact that scientists have intentions does not negate the Darwinian nature of cultural (scientific) evolution.

That is, as long as the evolution of scientific concepts is not *directed* in any sense by the actions of scientists.[575] Peter Skagestad, an early critic of evolutionary epistemology (which is, again, not the project Hull is engaged in, but the criticism is relevant to his project as well), has attacked the application of a model of purely blind variation and selective retention to science held by Campbell (mentioned above in connection to the polar bear analogy).[576]

For this attack, it is not enough simply to say that scientists direct conceptual change because they base adaptations of previous theories on heuristic methods. This might provoke the reply that these heuristic principles are themselves the product of blind variation and selective retention, and that their existence and what it implies must be taken to be part of the already-acquired knowledge; any acquisition of knowledge that is really new might still be a result of blind variation.[577]

However, Skagestad adds to this that the accumulated tradition works to decrease the "range of permissible guesses".[578] This is a crucial point. "Prior adaptation in biological evolution *raises* the probability of further adaptation, while the prior guessing embodied in an intellectual tradition may as often *lower* the probability of further progress through a novel, correct guess."[579]

It may be worth to drive this point home. After all, in one sense the possible further development of phenotypes in biology is limited by the previous evolutionary history of the organism, just like the possible development of an intellectual tradition is limited by its previous history: evolutionary change is path dependent in both cases. In biological

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574 Hull (1988a, 144).
575 Cf. the discussion by Grantham (2000, 452-454). For Grantham, this problem suggests that Hull's social explanations of science (especially based on its demic structure) are to some extent independent of his selectionist account, since the former can be accepted without the latter. Cf. also Wray (2000), which focuses on 'hidden hand' explanations of scientific institutions rather than theories.
576 E.g. Campbell (1960). See esp. ibid., 392-295 for Campbell's responses to anticipated objections.
577 Campbell (1974, 421). Cf. also Stuart-Fox (1999, 43).
578 Skagestad (1978, 615).
579 Skagestad (1978, 615).

evolution, however, genetic mutations that are physically possible are not ruled out by the makeup of the phenotype such as it is as a result of prior adaptation. In cultural history, on the other hand, not all the changes that are conceptually possible are always equally 'culturally possible': it may be possible for one scientist to replace a falsified heliocentric theory about the solar system by a geocentric one, while for another the only viable option is to replace it by another heliocentric hypothesis.[580]

The differential viability of heliocentric and geocentric hypotheses may simply mean that we should expect a differential proliferation (*as* scientists) of scientists holding heliocentric and geocentric theories in different ages, and that this differential proliferation will lead to a differential success of heliocentric and geocentric theories in different times. Moreover, Hull does not require that the social environment of scientists is only a proxy to nature; he allows for differential and culturally determined historical influences on theory selection.[581] But the objection, as I interpret Skagestad and as I would maintain myself, is that the likeliness of a heliocentric theory being 'altered', in one generation, into a geocentric rather than a heliocentric theory is itself not stable, and dependent on a historical context – the same historical context with which the scientists-interactors are confronted. The same forces that would operate to influence the selective retention of proposed theories may be anticipated by the scientists, and influence the ways they develop and publish their theories.[582]

Cecilia Heyes has called it problematic that in Hull's account individual scientists have so much agency, since: "it would be unfortunate if an evolutionary analysis of scientific change were crucially dependent on our understanding the beliefs and motivations of individual scientists since […] the content of these states is very difficult to specify."[583] She proposes to drop the idea that scientists themselves function as interactors; rather, if they conform to certain specified cognitive characteristics, they can allow other entities (such as texts, diagrams, and gestures) to function as interactors.[584] Needless to say, if scientists and their interactions with other scientists and with experiments influence not just the selective retention of theories but also the way in which they develop, then the role of individual scientists will become even greater, much to the dislike of those who find the understanding of their beliefs and motivations so cumbersome to deal with. It is my position that this understanding is, indeed, necessary.

I agree with Hull, however, that scientists do not, on a large scale, foresee what history will do to the conceptual change they have carried.[585] The reasons why a scientist develops and publishes a theory need not bear a clear and direct relation to the forces that

580 Cf. Cain (1988) on the emphasis in Hull's work on selection rather than the problem of variation.
581 Cf. Skagestad (1978, 616-617).
582 Cf. Thagard (1980).
583 Heyes (1988, 198). On the complicated role of scientists (and, in particular, of their social and conceptual relatedness) in Hull's system, see also Griesemer (1988, 181-182).
584 Heyes (1988, 199).
585 Compare e.g. Ghiselin (1988, 178).

allow her to increase her conceptual inclusive fitness through this theory and thereby allow her theory to flourish. Mendel need not have anticipated the circumstances that led to the eventual success of his findings. Newtonian mechanics may owe its spread to its service to lot of purposes that Newton did neither intend nor consider desirable.[586] I think it is, in general, a safe bet to say that no early modern scientist was consciously striving towards the current state of science – or laboring against it, for that matter.

This is in line with the invisible hand motif running throughout this chapter: we are looking at mechanisms that potentially transcend the scope of individual intentions. There may be something understandable about the dynamics of theory acceptance and rejection in science that does not at any stage need to be traced to individual intentions.

Nonetheless, I think it is important to have established – and the possibility that scientists anticipate at least some of the forces that influence the survival potential of themselves as scientists given their commitment to a certain theory, is only one way in which this may happen, albeit a conceptually important one – that there is something to be understood about the dynamics of conceptual change as well; and that these dynamics are historically conditioned in a way not captured by the notion of blind mutation.[587]

7.4 Adaptation, Realism, and the Necessity of Understanding

Hull's proposal for an explanatory account of the development of the 'lineage' of Western science in terms of evolution by selection is very elegant and sympathetic. We ought to keep in mind that the goals in this process have evolved along with the means and that this evolution has therefore not necessarily been one of linear progress or convergence. If we do so, Hull's account delivers all that it has promised: it explains scientific beliefs while undergirding the intuition that science is, by and large, good at realizing its manifest goals. The trick is that science is adaptive: that is has historically come to be structured in such a way that it evolves to accommodate new input from nature or changes in epistemic goals and methods.

We have also seen that Hull considers it essential to his invisible hand account that the things natural scientists are orienting their activities to – for instance, when they set out to discover laws of nature – actually exist. The question is to what extent we need to go along with this. For instance, do we need to be metaphysical realists when we say that science *adapts* to nature? This might seem to be so, since in this case we grant to nature a status independent of what science says about it – and is this not simply an instance of granting to the external world a status independent of what we think about it?

In fact, we need to be realists in this case no more than we need to be realists when we say that animal species (or lineages) *adapt* to their environment: what we need to believe is that we can intelligibly and meaningfully speak of 'natural entities' as something distinct

[586] See e.g. Jorink and Zuidervaart (2012) and the other essays in Jorink and Maas (2012) on the reception of Newton in the Netherlands, and its complicated relation to Newton himself.
[587] Cf. Sterelny (1994, 59-60) on the less ambitious options open to evolutionary theorists.

from 'things scientists say about nature'. It is possible to deny that we can do this, and in a simplification of Latour's position, we could say that he comes close to denying this; but *not* denying this does not mean that we are holding the metaphysical position that the world exists completely independently from our minds, let alone that our minds somehow have access to this mind-independent world.

The analogy with biological adaptation illustrates this. The opinion that biological entities adapt to their environment is not restricted to metaphysical realists. The analogy is complicated, of course, by the fact that, contrary to the case of biological adaptation, in which we have access to descriptions of the environment independently of our access to descriptions of biological entities, we do not always have access to nature independently of the science that we study.[588] Sometimes we do, but when we study the historical development of our *own* scientific opinions we do not.

Then still, the notion of an independent world can be something other than 'unnecessary metaphysics'; Philip Kitcher has characterized it as a result of extrapolation: "Our purchase on the idea that some objects are independent of some of us (although observed by others) suffices to make intelligible the thought that some objects are independent of all of us."[589] In our case, we can extrapolate to our awareness that some objects in nature exist independently of what other scientific cultures and traditions have said about them, to an awareness that some objects in nature exist independently of what any scientific tradition has said about them – including our own. When we say of successful ways of dealing with the world that they are approximately correct, Kitcher says, we do not make a jump from "things-as-they-appear-to-us" to "things-as-they-are-in-themselves",[590] but from a situation we observe to a (possibly counterfactual) situation which we do not observe: we say that if we hadn't been present to see this person or this culture dealing with a world that we observe to be independent of her, their actions would have been just as successful because the same causal relations apply.

Kitcher makes his move from success to accuracy based on his idea that "we rely on our common experience of likely success rates with accurate and inaccurate representations".[591] I am not sure that it is necessary or desirable to make this step in history of science: though accuracy may be a virtue or value central to current science, it is only indirectly relevant to history of science, as a potential aspect of the explanation of the historical development of scientific beliefs. And in history, as Laudan has argued,[592] we have to recognize as successful at least some theories that we are also bound to call inaccurate – sometimes perhaps even less accurate (to our knowledge) than less successful theories. At the very least, the relation between accuracy (which I understand to mean a degree of

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588 Cf. Skagestad (1978, 618).
589 Kitcher (2001, 25).
590 Kitcher (2001, 28).
591 Kitcher (2001, 28).
592 Laudan (1981).

structural similarity between a scientific theory or model and the external world) and success is not linear.

The reason I refer to Kitcher's argument about the notion of an independent world is not because of his point about accuracy, but because it serves to show that the fact that we cannot think about 'the external world as independent of how we think it is' as something substantially different from 'the external world as we think it is' – it would be paradoxical to say that we believe these two things to have different properties (e.g. one containing laws of nature but not the other) – does not prevent us from conceiving of this external world as independent of what any third person thinks about it, and extrapolate to its independence of what *we* think about it. That is, the idea can make sense that *our* scientific culture has to some extent 'adapted' to a nature that is independent of it even if it is only known to us *through* it.

Now, in a thoroughly Darwinistic world, the natural environment will be the main explanatory factor for the makeup of a species (conceived of as a lineage) at a certain time, in combination with the preceding temporal parts of this lineage: the lineage as it is will adapt to fit the environment in one of the optimal ways. We cannot simply translate this to say that nature as it is will be the main explanatory factor for the makeup of science; and not just because this would be Whiggish and circular. It is also not possible because 'nature' does not exhaust the 'environment' of science; or phrased more precisely, the set of things a scientific discipline seeks to describe does not exhaust the set of things that constitute the environment to which it adapts, since the environment with which it interacts also contains other people and objects. An evolutionary account of (a particular discipline in) science predicts that it will not converge to theories and models which fit the *objects* it studies best – if this would even mean anything – but that it will adapt to fit its environment as a whole best.[593] An easy way to make sense of this is, if we follow Hull in identifying the substance of science as being primarily its conceptual content, the recognition that this content needs to be of such a nature as to be amenable to being handled by humans with the perceptual and cognitive capacities such as they are biologically given, in numbers such as the social and economic structure of a society can provide, and with the categories and prejudices that their culture has imprinted upon them. Even if we believe that a geocentric cosmology is 'less accurate' than a heliocentric one – which would be the closest thing to 'less well *adapted* to the actual state of the solar system' – it can, at a certain time and place, be the doctrine *best* adapted to the historical context as a whole. The goal of history of science would be to describe this context in such a way that we can see how this is the case.

We have also seen, in the previous section, that the mechanisms behind the evolution of scientific concepts necessarily fall short of pure Darwinism, because the likeliness of possible mutations within the pool of scientific entities is influenced by cultural and other historical factors, adding a stage to the process before the actual mutations go

[593] Cf. Giere's (2006) use of 'fitness' in his perspectivist account of science (esp. 71-72).

through the selection process through interaction with the environment. It is crucial to understand how this happens.

These things together ensure that the thesis that science develops through the mechanism of evolution through selection, though it deals successfully with many problems regarding the relation between science and the world, cannot sidestep the demand for a more detailed understanding of the historical context by appealing to the environment from which the selective forces that work on science proceed.[594] The word 'understanding' is, at this stage of the argument, not intended to be contrasted to the 'explanatory' activity of evolutionary mechanisms simply by means of a terminological divide; I do not want to suggest a fundamental distinction between these two aspects of the explanation without arguing for it, so until further notice, by 'understanding' the historical context I simply mean clarifying the *explanatory* function that this context serves.

However, I do think that there is some crucial work to be done in exploring the relevance for history of science of 'understanding' with its further connotation of 'interpretation'; and that a philosophy of history of science, where history of science is the discipline engaged in understanding science by grasping it in its 'environment', cannot be complete without further reflection upon what it means to understand science in the world. This is the aim of the final chapter.

7.5 Conclusions

From the preceding discussion, we can conclude:

1) David Hull's view of science as a lineage provides a historically fruitful alternative to a view of science as defined normatively or as a kind.
2) A naturalistic view of science as evolving in continuous interaction with the world, where this interaction causes a differential proliferation of scientific theories and practices, provides a very plausible account of the role of the world in science that can in principle harmonize historical causal explanation of scientific developments with an explanation of why science seems successful.
3) There is no reason why such an account would be globally inevitabilist, since the entities that science studies do not constitute the whole of its selective environment, and scientific goals, methods, instruments, and theories are also part of each other's selective environment, which as a whole develops historically.

   Since the selective environment influences not only the selection but also the mutations in scientific goals, methods, instruments, and theories, the evolution of science is not properly Darwinian and an explanation of historical change in science requires an understanding of the whole local environment in which change comes to be proposed.

[594] See also the criticism of evolutionary accounts by Jardine (2004, 272-273).

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.[595] 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.[596] 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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[595] Cf. e.g. Bruns(1992, 1).

[596] 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.[597] 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.[598] 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.[599] 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.[600] 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."[601]

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

[597] Crombie (1994, 19-30).
[598] Popper (1963, 120-135, 136-165).
[599] Kuhn (1962, e.g. 40-41).
[600] Kuhn (1959).
[601] 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.[602] Normal-scientific traditions are different local and historical instances of this kind.[603] 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."[604]

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.[605] 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.[606] 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?"[607] 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

[602] Kuhn (1962, 42).
[603] See also Dear (2012a, 426-427).
[604] Kuhn (1962, 150); cf. also Kuhn (1962, 111-135).
[605] Hacking (1993, 306).
[606] Kuhn (1993, 319).
[607] Feyerabend (1975, 19).

traditions must conform".[608] 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.[609] Nonetheless, here we can see a clearly historical and 'lineage-like' account of scientific traditions.[610]

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.[611] 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."[612]

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.[613] 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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[608] Feyerabend (1978, 7).
[609] 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.
[610] Feyerabend (1978, 17).
[611] Barnes (1996, 18-45).
[612] Kuhn (1962, 94).
[613] 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.[614] 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".[615] It has an element of *Sachlichkeit*, which literally translates as *objectivity* but is explicitly distinguished from it by Gadamer;[616] 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,[617] 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.[618] 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":[619] 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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614 Heelan (1997, 19-20).
615 Gadamer ([1960] 1986, 297).
616 Gadamer ([1960] 1986, 457).
617 E.g. Jardine (1991, 76).
618 Gadamer ([1960] 1986, 307).
619 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,[620] 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.[621] 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.[622]

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.[623] In this sense, Gadamer precisely does make the binary opposition between science and tradition that we seek to undermine here.[624]

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

[620] Jardine (1991, 71-72).
[621] Gadamer ([1960] 1986, 331-332).
[622] In general, on the relation between the inescapability of interpretation and the possibility of truth in Gadamer, see Gadamer (1957) and Wachterhauser (1994a).
[623] Gadamer ([1960] 1986, 287-288).
[624] 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.[625] 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.[626]

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.[627] 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."[628] 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

625 E.g. Gadamer([1960] 1986, 70-71, 479-580).
626 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.
627 Cf. Also Abadía (2011).
628 Crease (1997b, 1).

engagement with the world which is prior to the subject and object separation." Third, the priority of situation over abstract formalization.[629]

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".[630] 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.[631]

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'.[632] 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.[633] 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."[634]

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."[635] 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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629 Crease (1997b, 4).
630 On Rouse cf. Ihde (1997, 114-115).
631 See also the discussion between Robert Crease and Martin Eger (1995).
632 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.
633 Cf. Crease (2002, 39-40).
634 Ginev (2002, 44-45).
635 Compton (2002, 195).

known natural world."[636] 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.[637]

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.[638]

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

636 Compton (2002, 195).
637 Compton (2002, 197-198).
638 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."[639]

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.[640] 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,[641] 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."[642] 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."[643]

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.[644] We have seen

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639 McGuire and Tuchańska (2000, 72).
640 Gadamer ([1960] 1986, 445).
641 Cf. Gadamer (1971a, 57-58).
642 Kockelmans (1997, 48).
643 Kockelmans (1997, 48).
644 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.[645]

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."[646] 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."[647]

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

645 Kockelmans (1997, 55).
646 Eger (1993a, 2).
647 Eger (1993a, 9).

reinterpretation of interpretation as constructivist".[648] The circle ceases to converge.[649] 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.[650]

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"[651] – 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)."[652] 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'.[653]

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.[654] 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

648 Eger (1993a, 6).
649 Eger (1993a, 9-10).
650 Laudan (1981).
651 Eger (1993a, 13).
652 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.
653 Van der Heiden (2012).
654 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.[655]

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.[656]

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.[657] 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 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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655 Eger (1997, 99-100).
656 Eger (1993b, 306).
657 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.[658]

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."[659] 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,[660] 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."[661] 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."[662] How are we to evaluate these statements, having argued in previous

[658] Rorty (1980, 726).
[659] Rorty (1987, 43). Cf. also Biagioli(1996, esp. 196-202).
[660] E.g. Rorty ([1979] 2009, 318).
[661] Rorty (1980, 725).
[662] 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"[663] – and that there are no normative reasons why it should.[664] 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[665] – 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.[666]

8.5 The Hermeneutical Position of History of Science

<u>8.5.1 General Thesis</u>

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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663 Rorty (1987, 44).
664 Rorty (1987, 45-46).
665 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.
666 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 17th-century natural philosophers than by 21st-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 16th-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 16th century, we are removing important handles we have on understanding what was going on in the 16th century. We would still have the historical sources about the 16th 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*.[667] 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

[667] 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 17th-century beliefs.

Second, we need to be aware that between the vacuists and the plenists, neither represents '21st-century science as it is in the actual world' or its opposite – no more than any party in the 16th-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)[668] 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'.[669] 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 17th 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

668 Shapin (1984).
669 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^{th}-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^{st}-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.[670] 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

[670] 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.[671] 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."[672]

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

[671] Cf. Jonker (2011); Feyerabend (1978, 33).
[672] Eagleton (2008, 63).

means to criticize the tradition to which one's understanding is simultaneously bound,[673] as it happens, was also the subject of an exchange between Gadamer and Habermas in a volume about the relation between *Hermeneutik und Ideologiekritiek*, 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.[674]

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.[675] Consciousness, though situated within the contingent structure of traditions, should not let itself be reduced to 'sublimating' all social processes into cultural tradition.[676] 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.[677] It is not enough to understand everything as covered by tradition, then.[678]

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.[679]

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.[680] It is not

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673 See also the thoughtful and balanced discussion on criticism and tradition in Gadamer's thought by Warnke (2012, 14-20).
674 Cf. also Collin (2015, 56).
675 Habermas (1971a, 49).
676 Habermas (1971a, 54-55).
677 Habermas (1971a, 52).
678 Habermas (1971a, 55).
679 Gadamer (1971a, 68).
680 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."[681]

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-legitimating, 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."[682] 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.[683] 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*.[684]

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:[685] 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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681 Gadamer (1971a, 71).
682 Gadamer (1971a, 74).
683 Habermas (1971b, 154, 158).
684 Gadamer (1971b, 289-292).
685 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^{th} 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^{th} 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 17th 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.[686]

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.[687]

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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[686] Schickore (2011, 515). On the question of the relation between historical case studies and philosophical claims, see also Kinzel (2015, esp. 53).
[687] 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.[688]

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.[689] Part of their argument is based on textual analysis – for instance, the fact that throughout the seventeenth century, 'alchemy' and 'chemistry' were used interchangeably.[690] 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".[691] 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".[692]

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".[693] 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

688 See e.g. Dobbs (1975, 6-20, 210-213); Figala (2002, 370-371); Westfall (1975, 215-226); Westfall (1984, 325-331).
689 Principe and Newman (2001); Newman and Principe (2002, 358-360)
690 Newman (2006); Newman and Principe (1998, 32-65).
691 Newman and Principe (2002, e.g. 117).
692 Newman and Principe (2002, 177).
693 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."[694]

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 17th-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 17th-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.

[694] 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".[695] 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".[696] 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".[697]

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."[698] 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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695 Lehoux (2012, 134).
696 Lehoux (2012, 145).
697 Lehoux (2012, 150).
698 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*.[699]

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.

[699] Lehoux (2012, 232).

Conclusion: Nature in History

The answer to our main question – the role of the world in historiography of science – works on two levels, since the question itself has turned out to have two sides: it is the question *what* explanatory work nature does, and how what explanatory role it has in historiography of science relates to other explanatory factors; but it is also the question in what sense *our* knowledge of nature is capable of providing us any insight into a history of science that predates that knowledge. In other words: it is the question of the explanatory role of nature as well as the status of our beliefs about nature in history of science. These different readings of our question correspond strongly to different parts of the thesis. But in the end, the position defended in this thesis can be understood fully only if we bring the two together.

The answer is that the role of nature is itself historically variable, not just because nature itself develops historically – in this development, there may, after all, be significant continuities that some sciences seek to identify – but because its mode of influence is dependent on historically formed scientific traditions. Upon these traditions themselves, the natural world has also exercised a causal influence, dependent on previous modes of interaction between nature and culture.

It follows that nature is causally integrated within the whole of history of science, without thereby rendering final beliefs about itself inevitable. In this, we depart from the inevitabilist perspectives in chapter 4, which, in so far as they were genuinely inevitabilist, turned out to involve an explicit or implicit belief that the final shape of science reflects only the shape of nature; and we depart from the perspectives dealt with in chapter 5, which said that science can be understood without referring to nature at all. The reason is not just that nature is neither completely impotent nor all-powerful in the history of science, but that its mode of causal influence upon the history of science is itself historical and cannot be simply disentangled from non-natural influences. Here we agree with Latour, who, as we have seen in chapter 6, has done a lot to counter ways of looking at science that need to draw a chasm between nature and society. But what we take from him is not the suggestion that we are not allowed to use what categories and labels we possess for the mere reason that these labels are a contingent product of history; rather, we follow him in saying that there is no ontological chasm, and that the entities that constitute science and the entities that constitute nature (as the object of scientific interest) are in continuous interaction with each other. There is no meaningful sense in which we can claim there to be a 'net influence' of society, or of nature.

This inextricability of nature and culture has repercussions for both sides of our original question. With regard to the role that nature plays in determining what science looks like, it pushes us, in effect, towards a naturalist perspective. It entails the rejection of an ontological dualism between nature on the one hand and science or society on the other hand; it is a perspective that recognizes entities for their causal interaction with other entities. The most sympathetic account of scientific development, when it comes to the explanatory role of nature, is that by David Hull, discussed in chapter 7. His combination of evolutionary and invisible hand mechanisms leads to a plausible picture in which natural and non-natural entities are seamlessly integrated, without thereby forgetting that it is science and its development which have our interest, and without foregoing the possibility to explain this development. His account allows us to say that what physically happens in air-pumps matters because different events in air-pumps would have led to different observations and a different differential proliferation of beliefs. That is, the selective environment of science – which comprises both natural and non-natural entities – would have been different if air-pumps behaved differently. However, we need to embrace even Hull selectively, and ignore the inevitabilist claims he sometimes makes, especially in so far as these are based on the idea that the natural objects that science investigates render the development of science inevitable; his own model does not lend support to such an inevitabilism, since the content of science does not adapt to an environment that consists only of the natural entities it studies.

With regard to the other side of the question, the answer is that our current beliefs about nature are hermeneutically inevitable (or 'inescapable', in order to distinguish this sense of inevitability from the thesis that our current science is historically inevitable), precisely because nature is causally integrated in the whole history of science: we cannot understand past science without assuming anything about the natural world in which it developed, and there is no reason why in so doing, we would opt for any assumptions about the natural world other than those we actually hold. Here our alignment with many authors discussed in previous chapters gets inverted relative to that concerning the causal question: though we may disagree with Weber, Merton, Koyré and the Marxists about the historical inevitability of our science, we embrace their presentism. Though we disagree with Bloor and Collins about the explanatory relevance of nature, we follow them in their acceptance of current science as providing the best understanding of nature available to us. And though we agree with Latour about the contingency and historicity of our current beliefs and categories, and about the causal inextricability of nature and science, we decline the methodological prescriptions he draws from this, that in order to understand, we need to be radical empiricists and simply follow the actors.

What we have shaken off, then, is the doublet of assumptions that in order for something to be a legitimate point of departure for understanding in history of science, it needs to be historically inevitable; and that we can find the history-independence this requires in nature more easily than in society. Most other perspectives we have dealt with worked by at least one of these assumptions: the inevitabilists we discussed could be presentists because they were also inevitabilists; the contingentists we discussed mostly decided that since nature failed to render current beliefs inevitable, it could not be used as a stable vantage point for historical understanding. Latour consistently concludes this logic, by the idea that since everything we have is the result of a contingent history, nothing of it can be of help when we want to account for that history.

The hermeneutic perspective we are embracing here allows us to shake off both the nature-society polarity, and the preoccupation with identifying the history-transcendent and separating it from the historical. If a polarity still remains, it is between historical continuity and discontinuity: we build our understanding of the past on what we seem to share with it – on the extent to which the horizons of this past and our present overlap – and we seek understanding of what seems alien. What we share may be the way air-pumps or other experiments seem to behave; or it may just as well be ways of reasoning, or aspects of a cultural or political environment – the point here being that what is natural is as such not necessarily more robust than what is social or cultural, nor the other way round. Perhaps we cannot step in the same river twice, but we can read the same Platonic dialogue twice; but then again, perhaps human political systems change their form and behavior, in the sense in which those interest us, at a quicker pace than the solar system does.

Which things change, when, how much, and under what circumstances, are not questions to which we can decide the answers *a priori*, but neither are they questions to which the historical sources will single-handedly teach us the answers. Answers to these questions are based on historical knowledge, part of which is accumulated in the traditions of which we are part – historical *and* scientific; for these are not mutually exclusive categories, and our belief that air-pumps and other instruments exhibit stable behavior over time is itself based on historical knowledge. This is not to say that knowledge about vacuums turns out to be subsumed under the expertise of historians and that all other disciplines should concede their territories to history; rather, it is to remind ourselves again that the tensions between history and science sketched in the introduction of this thesis rest on taking a bit too seriously the neo-Kantian distinction between the idiographic and the nomothetic, or between the cultural and the natural sciences.

It is the productive tension between continuity and discontinuity that makes a hermeneutic perspective so well suited for a *historical* study of science – rather than a rationalist, idealist, Marxist, social constructivist, actor network-theory or generic invisible hand perspective. The evolutionary perspective discussed in chapter 7 comes closest, in its treatment of science as a historical entity and its transcendence of a nature-society dichotomy. We can treat the development of science, or developments in science, as contingent in the sense described in chapter 2 – with results that are path-dependent rather than inevitable, implying that it was historically possible for us to have other scientific beliefs, without thereby taking it any less seriously or denying ourselves any of the resources it might provide us for understanding its past.

How does all this help? Much like Gadamer's *Truth and Method*, the aim of these considerations is not to draw methodological lessons, but to elucidate what historical understanding means within the context of history of science. I believe that this elucidation can be helpful in, among else, the following ways:

1) Overcoming the Whig – anti-Whig polarity: in this hermeneutical perspective, "scientists' history" and "historians' history" are no longer separated by an unbridgeable chasm, representing incommensurably different epistemic aims and attitudes towards the historical sources.

   What remains, is a range of possible opinions about the modal structure of history: about what determines – in general, or in particular instances – scientific developments, and in what sense and in which ways these could have gone differently. In short, what remains is a debate about causality in history of science, and a competition between explanations, in which no side *a priori* carries better cards than the other – in which there are very few claims of which we can deduce from their language that they must be 'unhistorical'.

2) Enabling historians to employ more liberally their actual beliefs about what did and could have happened in the past, by showing the real problems that the concept of Whig history points at to reside in causal anachronism, rather than in presentism or conceptual anachronism.

   We can say that there were microbes before Pasteur, and that people died because of them, without worrying too much that we are committing a deadly historical sin. We must always remain open to new historical insights into the question under which circumstances microbes are and are not possible, but this is because we cannot know *a priori* which of our beliefs may constitute causal anachronisms, and therefore none of our claims is ever absolutely historically safe. It is certainly not because we are allowed to speak of microbes in our narratives only after our actors have started speaking about them.

3) Showing in what sense history of science can provide and has provided, both in principle and in practice, genuine corrections to our understanding of science. This is not by leaving our presuppositions about science and the world as we know it through science at the door, but precisely by bringing them to our historical investigation and keep an open eye to where tensions arise.

4) Giving a meta-account of what is good historiography: rather than defining the methods or desired outcomes of the study of science in history (for instance, Harry Collins' Empirical Programme of Relativism, which is supposed to show how science is determined by social rather than 'scientific' factors), a hermeneutical perspective on historiography of science does justice to the extent to which historiographical work is in dialogue with pre-existing understandings of science in history. It contextualizes and historicizes, rather than defining generally, the very notion of good historiography.

   Thus, it can admit the importance of Collins' EPOR, and of SSK in general, in a specific background, as a critical corrective of a rationalistic and transcendentalist view of science. It can see Latourian ANT as in turn in critical dialogue with rationalism and SSK, and invisible hand accounts as a response to

> constructivism. It recognizes that new historiographical perspectives themselves build on material from a contingent history. Thus, a hermeneutic philosophy of historiography of science unproblematically meets the criterion of reflexivity: everything it assumes about science in history it is prepared to assume about itself.

Above all, this thesis has attempted to outline how we can think of science as genuinely historical and genuinely about the world. We saw at the start of this thesis that Sarton set out to do precisely this but failed to deliver. The rest of our work has been a search for a consistent and plausible way to look at the relation between science, history, and the world in the writing of history of science. A hermeneutical perspective constitutes such a way, however contingent and path-dependent the formulation of such a perspective as provided in this thesis may be itself.

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Samenvatting in het Nederlands

Natuur en geschiedenis: naar een hermeneutische wetenschapsgeschiedfilosofie

De vraag die voorligt is: als we als historici de geschiedenis van de natuurwetenschappen bestuderen, wat voor rol speelt de wereld die die natuurwetenschappen op hun beurt bestudeerden en bestuderen dan in onze historische verklaringen?

Deze vraag raakt aan meerdere controverses en spanningen in de wetenschapsgeschiedschrijving als vakgebied, die ik in hoofdstuk 1 inleid. De belangrijkste hiervan is de vraag in hoeverre wetenschap überhaupt toegankelijk is voor historisering: is uit de voltooide producten van wetenschappelijke ontwikkeling de geschiedenis niet verdwenen? Een andere vraag is de verhouding van de geschiedschrijving van de natuurwetenschappen tot de vakgebieden wier geschiedenis zij bestudeert. Is er een 'kloof' tussen de humaniora en de natuurwetenschappen, waartussen de wetenschapsgeschiedenis een brug kan vormen? Maar hoe ziet die brug – en de kloof trouwens – er dan uit?

In George Sarton zien we een historicus die wel heel ver meegaat in de mythologisering van wetenschap als wezenlijk rationeel of geniaal. Een sympathiekere benadering lijkt dan die van Reijer Hooykaas: wetenschap schudt de historische omstandigheden waaruit ze voortkomt nooit helemaal af, en de geschiedschrijving laat de huidige wetenschap zien als erfgenaam van een traditie. Die traditie is mensenwerk, en we moeten haar als zodanig begrijpen. De wetenschapsgeschiedschrijving wordt zo een bemiddelaar tussen heden en verleden van de wetenschap.

Een vervolgvraag dient zich dan aan, namelijk in welke gezagsverhouding dit de historica plaatst tot de wetenschappen in kwestie. Waar Paul Forman betoogde dat historici zich om 'Whig history' te vermijden verre dienden te houden van de wetenschappen in kwestie, en zich dus ook maar beter konden onthouden van een appèl aan de werkelijkheid waartoe alleen wetenschappers toegang hadden, staat Hooykaas' benadering ons niet toe de deur zo stevig te sluiten: er blijft, in principe, een dialoog mogelijk tussen natuurwetenschappelijke en wetenschapshistorische kennis. Vanaf hoofdstuk 4 zal dit perspectief geleidelijk verder ontwikkeld worden, in confrontatie met recente perspectieven op de aard van wetenschapsgeschiedschrijving.

Ter voorbereiding analyseer ik eerst enkele centrale maar meerduidige concepten. In hoofdstuk 2 werk ik toe naar definities van 'contingentism' en 'inevitabilism' – termen die (meestal) verwijzen naar de extreme uiteinden van een spectrum van posities waarop men zich kan bevinden met betrekking tot de vraag of de wetenschap zich ook *anders* had kunnen ontwikkelen dan ze in werkelijkheid heeft gedaan. Een belangrijke stap is om in te zien dat dit spectrum *niet* hetzelfde is als de tegenstelling tussen indeterminisme en determinisme: als we vragen of de huidige of een latere stand van wetenschap onvermijdelijk was, bedoelen we (meen ik) niet te vragen of de geschiedenis deterministisch is, maar of meer of minder nabije mogelijke geschiedenissen allemaal ongeveer convergeren naar de bedoelde toestand.

‘Contingentism’ en ‘inevitabilism’ drukken dan opvattingen uit over de mate waarin ‘historisch mogelijke’ alternatieve paden (d.w.z. paden die niet uitgesloten zijn door onze historische beschrijvingen) in de geschiedenis divergeren of convergeren. Een ‘contingentist’ is, in tegenstelling dus tot een ‘inevitabilist’, van opvatting dat de wetenschap in hoge mate padafhankelijk is: eerdere gebeurtenissen beïnvloeden sterk de relatieve kansen van latere. Ze committeert zich daarbij dus niet aan de opvatting dat delen van de historische ontwikkeling van wetenschap onverklaarbaar zijn. Dit is in lijn met de manier waarop historici de term ‘inevitable’ gebruiken. Vaak is dat ontkennend: de uitkomst van wetenschappelijke controverse C of de algemene acceptatie van theorie T was helemaal niet zo onvermijdelijk als wel is gedacht. Daaruit volgt echter niet dat de zoektocht naar een vollediger en bevredigender historische verklaring wordt losgelaten; integendeel, die begint juist doordat de ontkenning van grootschalige onvermijdelijkheid de historische opdracht om specifieke episodes in de geschiedenis nauwkeurig te bestuderen van meer gewicht heeft voorzien.

De tegenstelling contingentie-onvermijdelijkheid houdt verband met de vraag naar de rol van de natuur in de wetenschapsgeschiedenis. De claim dat het eindpunt van wetenschappelijke ontwikkeling onvermijdelijk is, zou zich immers goed laten onderbouwen door de claim dat in dat eindpunt wetenschap enkel een afspiegeling is van tijdloze (dus niet-padafhankelijke) natuurwetten.

In hoofdstuk 3 ontleed ik de term ‘Whig history’. Losjes gedefinieerd verwijst deze term in de wetenschapsgeschiedenis meestal naar geschiedenis zoals wetenschappers zelf die zouden schrijven: presentistisch, anachronistisch en met een vanzelfsprekend geloof in wetenschappelijke vooruitgang. Mijn positie is dat *causale* anachronismen, waarmee ik bedoel historische onmogelijkheden, in de geschiedschrijving natuurlijk vermijdenswaardig zijn en dat geen historica daar anders over heeft gedacht; maar dat een afkeer van *conceptuele* anachronismen (gebruik van concepten en kennis die de historische actoren niet voorhanden was) moeilijk te onderbouwen is behalve door die weer, zoals Ian Hacking doet voor zijn ‘interactive kinds’, te verbinden aan causale anachronismen.

Wat *vooruitgang* betreft, betoog ik dat de wetenschapsgeschiedenis niet, zoals wel is beweerd, een uitzonderingspositie inneemt in de geschiedenis waarop de gebruikelijke notie van Whig history stukslaat; het oordeel van vooruitgang is principieel even problematisch als elders, en het heden is in de wetenschapsgeschiedenis niet objectief méér bevoorrecht dan in andere deelgebieden van de geschiedschrijving. Wat *presentisme* betreft, betoog ik dat presentisme en historisme beide geen bevredigend beeld geven van de relatie tussen onze eigen categorieën en opvattingen en de confrontatie daarmee met de bronnen, en dat de metafoor van een hermeneutische cirkel die adequater weergeeft. Voor de wetenschapsgeschiedenis betekent dit dat ik oproepen om de ‘eigen wetenschappelijke kennis’ te vergeten, of onszelf zelfs volledig van onze positie in de tijd los te maken, met scepsis bekijk.

Vanaf hoofdstuk 4 staat de rol van de 'natuur' centraal als een mogelijke verklaring voor het verloop van de wetenschapsgeschiedenis. In hoofdstuk 4 zelf is deze rol gekoppeld aan 'inevitabilism': als de wetenschapsgeschiedenis niet contingent is, kan dat komen doordat haar uitkomsten uiteindelijk altijd bepaald worden door niet-historische factoren zoals de natuurlijke wereld die de wetenschappen bestuderen. Bij Steven Weinberg neemt die wereld de functie aan van een 'onderwijsmachine', die ons met vallen en opstaan níet alleen over de structuur van de wereld leert, maar ook over de methoden van ware wetenschap. Het vermeende mechanisme daarachter – plezierprikkels bij succesvolle wetenschappers – is echter erg ongeloofwaardig, en wordt niet consistent toegepast.

Kansrijker lijkt de stellingname dat de natuur de uitkomsten van *rationele* wetenschap bepaalt, waarbij die 'rationaliteit' zich ook ahistorisch en cultuuronafhankelijk moet laten identificeren. Een denkbaar bezwaar is dat we in dat geval wel helemaal kunnen stoppen met historische en causale verklaring, maar een nauwkeurige lezing van Max Weber laat een manier zien waarop geloof in de normatieve universaliteit van (wetenschappelijke) rationaliteit te rijmen valt met een causaal-historische verklaring van zelfs wetenschappelijke claims: de crux is dat we dat wat we willen verklaren niet benaderen als *geldig*, maar als gewoon *bestaand*, ook al belichaamt het normen die we wel degelijk geldig vinden, en ook al zijn deze normen hermeneutisch relevant, voor het afbakenen van het historische object.

Dat betekent dat ook Robert Mertons 'Weberiaanse', normatieve afbakening van wetenschap onverlet laat dat die wetenschap zich op allerlei manieren causaal tot de rest van samenleving kan verhouden. Op zijn beurt betekent dit echter weer dat Mertons sociologie geen onderbouwing geeft voor zijn eigen 'inevitabilistische' intuïties; in het algemeen is er geen brug van de claim dat *rationele* wetenschap bepaalde onvermijdelijke uitkomsten heeft naar de claim dat wetenschap zoals die in de geschiedenis daadwerkelijk bestaat diezelfde uitkomsten met dezelfde onvermijdelijkheid heeft.

Twee andere uitwerkingen van 'inevitabilism' en de rol van de natuur erin passeren de revu: een idealistische die een noodzakelijke uitkomst ziet in de ontwikkeling van wetenschappelijke concepten (soms te bespeuren in het *Journal of the History of Ideas* in de jaren 1940 en '50, en explicieter in het werk van Alexandre Koyré), die alleen onder idealistische metafysische aannames houdbaar is en dan alsnog ondersteund wordt doordat de wetenschap de structuur van de wereld benadert (zij het primair via het intellect); en een Marxistische, die ik traceer via het werk van Boris Hessen en John Desmond Bernal. In Marxistische wetenschapsgeschiedschrijving blijkt de onvermijdelijkheid van de inhoud van wetenschappelijke theorieën uiteindelijk toch primair op het conto van de natuur te schrijven, en op het vermogen van de wetenschap de objectieve structuren in die natuur progressief beter te weerspiegelen. In die zin treden dezelfde problemen in werking als bij de eerder besproken varianten.

'Inevitabilism' is dus in de praktijk steeds geassocieerd met het idee dat wetenschap de unieke structuur van de wereld steeds beter doorgrondt, en geeft die wereld zo alle verklaringskracht. In hoofdstuk 5 bekijk ik het andere uiterste: de stelling dat verklaringen in de wetenschapsgeschiedenis de natuur helemaal niet als verklaarder mogen gebruiken. David Bloor suggereert vaak deze positie in te nemen en onderbouwt die via de onderdeterminatie van theorieën door data (maar ik laat zien dat dat principe niet sterk genoeg is), en via de stelling dat de natuur als gezamenlijke achtergrond waartegen theorieën zich ontwikkelen nooit het verschil tussen theorieën kan verklaren (maar dat is niet altijd de vraag die aan de orde is). Verder gaat Bloor ervan uit dat onze concepten volledig sociaal gedetermineerd kunnen zijn zonder dat hun relatie tot de natuurlijke wereld ontkend hoeft te worden, omdat de samenleving deel is van de natuur en er dus ook altijd toegang toe geeft. Taal en sociale instituties zijn bijvoorbeeld altijd deels zelfverwijzend en deels representerend. Maar daarmee doet Bloor eigenlijk al te veel concessies om de stelling overeind te houden dat referentie aan de natuur in verklaringen altijd overbodig is.

Harry Collins voegt enkele argumenten toe om die stelling toch te handhaven. Zijn belangrijkste bijdrage is de notie van methodologisch relativisme: historici en sociologen moeten niet naar experimentele data of andere 'wetenschappelijke' motieven kijken voor theoriekeuze, niet omdat die er niet toe doen maar omdat die niet hun belangstelling hebben of ze er niet competent over kunnen oordelen. Het is echter zonder verdere reden onverstandig om een categorie potentieel causaal relevante factoren uit te sluiten, en het is misschien zelfs problematisch wetenschap überhaupt te identificeren zonder veronderstellingen over de natuur die ze bestudeert. Collins' verdere reden is een circulariteitsbezwaar: we willen verklaren *waarom* conclusies uiteindelijk voor waar werden gehouden, en de waarheid van die conclusies mag daarbij volgens Collins niet verondersteld worden.

Het antwoord dat ik op dit bezwaar aandraag, komt neer op een uitgebreid "hoezo eigenlijk niet?". Als we het circulariteitsbezwaar ontleden, zien we dat het slechts ten dele daadwerkelijk om circulariteit gaat en ook deels om meta-oordelen over de betrouwbaarheid van wetenschap. Verder ontleent het circulariteitsbezwaar zijn kracht mijns inziens aan het idee dat het historisch mogelijk was dat de wetenschapsgeschiedenis of de episode die we bestuderen anders was afgelopen, en dat wij dan ook iets anders hadden geloofd over precies die opvatting waarvan we de geloofwaardigheid willen verklaren. Maar de historische contingentie van wetenschappelijke kennis mag voor de historica geen reden zijn die kennis te verwerpen. (Meer hierover verderop.)

Als laatste bespreek ik in hoofdstuk 5 Karin Knorr-Cetina en haar argument dat natuurlijke entiteiten constructies zijn van de wetenschap, dus producten en geen verklaringen. Haar argumenten en voorbeelden laten echter steeds ruimte voor de mogelijkheid dat de gedragingen van natuurlijke entiteiten wel degelijk cruciaal zijn voor de ontwikkeling van wetenschap, en tellen dus wederom niet op tot een onderbouwing van de stelling.

In hoofdstuk 6 bespreek ik een ander soort constructivisme, namelijk dat van Bruno Latour. Hij corrigeert de focus van SSK op 'sociale' oorzaken, voor zover sociale factoren in tegenstelling tot niet-menselijke of materiële gedefinieerd worden: *alle* soorten entiteiten spelen volgens Latour in hun interactie een rol in de fabricatie van nieuwe theorieën, en menselijke hoeven hierbij niet in andere termen benaderd te worden dan niet-menselijke. Latours interessantste innovatie is dat hij het onderscheid tussen de natuur en de weergave van die natuur op probeert te heffen, waardoor het in een veel sterkere zin dan bij Collins een cirkelredenering wordt om te zeggen dat, bijvoorbeeld, het bestaan van microben het geloof van wetenschappers in microben mede verklaart. Het bestaan van die microben is namelijk zelf het product van hun fabricatie in een wetenschappelijk netwerk – ze worden geconstrueerd door de collectieve uitspraken en daden van alle actoren in dat netwerk – en daarmee wordt het anachronistisch om ze als verklaringsgrond te laten optreden vóórdat ze opduiken in de sporen die dat netwerk nalaat.

Latours radicale voorstel is interessant, omdat het natuurlijke entiteiten 'historiseert' (microben bestaan *echt* maar zijn óók een historische fabricatie), en omdat het consistenter dan SSK doet appelleert aan een empiristisch ideaal in de geschiedschrijving: we mogen eigenlijk *niets* veronderstellen van onze huidige categorieën en opvattingen, omdat die *allemaal* (níet alleen de 'natuurwetenschappelijke' opvattingen, maar ook degene die betrekking hebben op de samenleving) het product zijn van een contingente geschiedenis. Zijn voorstel gaat tegelijkertijd in tegen het gezond verstand, en leidt in de los beargumenteerde vorm waarin Latour het presenteert tot veel begripsverwarring. We kunnen (en moeten) Latour dan ook linksom of rechtsom omzeilen. Rechts inhalen kunnen we hem als we hem dwingen zijn principe van (historisch) 'relatief bestaan' *nog* consistenter toe te passen, namelijk ook op de historische tijd waarin de historische fabricaties van in wetenschappelijke theorieën beschreven natuurlijke entiteiten zich afspelen. Over het midden kunnen we hem passeren door erop te wijzen dat zijn positie slechts één mogelijke is in een spectrum van opvattingen over de vraag in hoeverre het bestaan van objecten causaal samenhangt met wat er over hen gedacht en met hen gedaan wordt.

Zodra we in dat spectrum een andere positie innemen – een die bijvoorbeeld niet van ons vereist dat we bij wetenschapshistorisch onderzoek ál onze vooronderstellingen bij de voordeur achterlaten – kan er ook weer een onderscheid ontstaan tussen hoe wij denken dat de wereld in elkaar steekt, en hoe we denken dat vroegere wetenschappers dachten dat de wereld in elkaar stak. Dat onderscheid tussen 'wereld' en 'wetenschap' is niet identiek aan dat tussen 'natuur' en 'samenleving' (de 'wetenschap', als drager van theorieën over de wereld, omvat bijvoorbeeld ook een hoop niet-menselijke objecten!) en in die zin kunnen we Latours nadruk op de historische contingentie van dat onderscheid nog steeds ter harte nemen met betrekking tot al onze eigen opvattingen. Maar het voorgaande betekent wél dat de wereld tot de wetenschap nog altijd in een complexe relatie staat – en niet een waarin beide, zoals Latour meent, in één en dezelfde beweging ontstaan.

In hoofdstuk 7 bespreek ik een een aantal perspectieven op die relatie die gebruik maken van 'invisible hand' processen, waarbij de handelingen van wetenschappers een resultaat opleveren dat door hen niet beoogd is maar dat wetenschap wel succesvol maakt. Een voorbeeld is het economische model van Alvin Goldman, waarin wetenschappers niet naar waarheid streven maar naar erkenning: hun eigen doelen zijn volledig sociaal van aard, maar aangezien hun instrumenten interactie met de wereld vereisen, schrijdt de wetenschap wel degelijk voort in haar grip op de natuur. Door die volledig sociale oriëntatie van wetenschappers te rijmen met een mogelijkheid van objectiviteit, heeft Goldman althans deels een antwoord op SSK; maar zijn model rust wel op een normatieve rationaliteit – het is eerder een sociale epistemologie dan een descriptief model.

Wel uitdrukkelijk descriptief bedoeld is David Hulls evolutionaire model van wetenschappelijke ontwikkeling. Hull zegt dat we wetenschap kunnen beschouwen als een stamboom van theorieën of andere inhoudsdragers, die zichzelf repliceren en die via fysieke entiteiten – in het bijzonder wetenschappers – in interactie treden met de wereld. Door die interactie kunnen verschillende theorieën zich in verschillende mate succesvol repliceren; er werkt een zekere selectiedruk op wetenschap. Het is natuurlijk van belang dat de wereld die de wetenschap bestudeert deel is van de omgeving die die selectiedruk uitoefent. Soms slaat Hull daarbij wel erg teleologische taal uit, die door zijn eigen model niet volledig ondersteund lijkt te worden; maar meestal benadrukt hij dat alles aan wetenschap – niet alleen theorieën maar ook methoden en doelen – aan evolutie door selectie onderhevig is, en dat de 'fitness' van theorieën volstrekt contextueel bepaald is.

Hull dicht, in lijn met de nadruk op 'invisible hand'-mechanismen, weinig verklarende kracht toe aan de directe intenties van wetenschappers; en hoewel ik zeker met hem meega in de gedachte dat wetenschappers niet voorzien wat de geschiedenis zal doen met de theorieën die zij dragen, dunkt mij dat de culturele omgeving waarin wetenschappers opereren niet alleen invloed heeft op het selectieve behoud van eenmaal ontstane of veranderde theorieën, maar ook op de manier waarop die ontstaan en veranderen – onder meer omdat wetenschappers intentionele wezens zijn, die op die omgeving anticiperen en inspelen. Gedetailleerd begrip van de lokale context waarin een episode zich afspeelt – die onder Hulls model sowieso al erg belangrijk is – is dus van des te meer gewicht.

In hoofdstuk 8 duik ik verder in die vraag naar 'begrip', en vertaal ik wat Hull een stamboom noemt naar de minder naturalistische term van een traditie. Dan doemt meteen een spanning op tussen de contingentie en historiciteit die met zo'n traditiebegrip zijn geassocieerd, en het geschiedenisontstijgende van de wetenschap. Tot die spanning hebben verschillende wetenschapsfilosofen zich verschillend en genuanceerd verhouden – zowel bij Popper, Kuhn als Feyerabend is wetenschap in een weliswaar verschillende zin steeds ook deels een historisch specifieke traditie. Bij SSK wordt dat traditiebegrip sociologisch begrepen en gezien als in tegenspraak met het ('traditionele') zelfbeeld van wetenschap; maar we kunnen de notie dat wetenschap een traditiegebonden activiteit is ook omarmen en tegelijk welbewust een perspectief van *binnen* die traditie aannemen. Dat is de positie die ik hier uitwerk. Daarbij beschouw ik de hermeneutiek van Hans-Georg Gadamer – waarin historiciteit en traditionaliteit niet meer in een noodzakelijke tegenstelling staan tot legitimiteit, en elk begrip juist vanuit traditie vertrekt – als een goede gids, ondanks het feit dat Gadamer zelf de natuurwetenschappen van zijn stelling uitzonderde.

Andere theoretici hebben wel uitdrukkelijk een hermeneutische visie op wetenschapsbeoefening geformuleerd. Terugkerend motief daarin is de praktijkgeladenheid van wetenschappelijke begrippen – hun relatie tot een historisch specifieke 'levenswereld'. Die praktijkgeladenheid staat zelf dan weer op gespannen voet met een ander motief, namelijk dat van de structurering van de wereld door betekenisvolle taal – maar die spanning laat onverlet dat wetenschap bij deze theoretici zelf een hermeneutische activiteit is, waarin wetenschappers vanuit een specifieke historische situatie in interactie treden met de wereld die ze bestuderen én met de traditie waarin ze staan. Anders dan bij SSK wordt dit niet gepresenteerd als een demythologiserend inzicht of als een tekort dat aangevuld moet worden: de onontkoombare historiciteit van wetenschap is geen defect, maar simpelweg deel van de menselijke toestand. En: de interpreterende activiteit die wetenschap is laat onverlet dat het belangrijk is dat ze *ergens over gaat* – zoals Martin Eger bijvoorbeeld benadrukt. Dit onderscheidt een hermeneutische blik op wetenschapsbeoefening van een constructivistische.

Grotendeels in lijn nu met zowel het naturalistische perspectief van David Hull als met de hier genoemde hermeneutische denkrichtingen stel ik dat de wereld (en daaronder begrepen ook dingen waarvan we geloven dat hun bestaan en eigenschappen onafhankelijk zijn van de menselijke geschiedenis) een causale rol speelt in de wetenschapsgeschiedenis, die evenwel historisch variabel is en ingebed in verschillende wetenschappelijke interpreterende activiteiten. Juist vanwege die historische veranderlijkheid is er geen nauwkeuriger stelling over de rol van de wereld mogelijk dan deze – maar die wijkt dan wel sterk af van de eerder besproken theoretische perspectieven. Ze betekent bovendien dat we vroegere wetenschap niet begrijpen dan vanuit onze huidige historische positie. De natuurlijke wereld heeft op de ontwikkelingen die daartoe hebben geleid voortdurend invloed uitgeoefend, op zo'n manier dat we – en hierin volg ik Latour – niet moeten trachten die invloed eerst te scheiden van niet-natuurlijke 'sociale' factoren. We moeten erkennen dat ook de wetenschapsgeschiedschrijving in de geschiedenis staat, evengoed als de wetenschap wier ontwikkeling ze beschrijft en verklaart. Er is geen reden waarom we, als we vanuit die positie een interactie met de sporen uit het verleden aangaan, eerst een deel van onze overtuigingen opzij zouden moeten zetten.

Ik voorzie vier belangrijke bezwaren tegen deze positie. Ten eerste, dat we wel degelijk in de derde persoon over onze eigen cultuur kunnen spreken. Ik erken de mogelijkheid van de historica om zich, werkelijk of fictief, buiten een wetenschappelijke consensus te plaatsen; maar wordt de afstand daartoe te groot, dan zal die juist ook wel degelijk de dialoog met collega-onderzoekers bemoeilijken.

Ten tweede: dat de wetenschappelijke traditie in deze benadering bij voorbaat veel te veel gezag toegekend krijgt, en kritiek zo onmogelijk wordt. Ik stel dat dit bezwaar, dat ook wel tegen Gadamer te berde is gebracht, berust op een misbegrip van een Gadameriaans traditiebegrip: traditie is niet absoluut of zelflegitimerend, maar is beter te begrijpen als de som van historische invloeden op onze positie, waaraan geen volledig ontkomen is – noch door de wetenschap, noch door haar geschiedschrijvers.

Ten derde: dat mijn positie indirect alsnog ‘Whiggish’ is: de historica ‘stuurt’ naar iets wat lijkt op de huidige toestand, omdat ze immers alsnog aanneemt dat onze eigen wetenschap het wel zo’n beetje bij het rechte eind zal hebben. Ik meen dat de opvattingen van de historica over de natuur in dit verband geen andere status hebben dan haar andere opvattingen, en dat huidige wetenschappelijke opvattingen meestal niet te identificeren zullen zijn met één kant in een vroeger wetenschappelijk debat.

Ten vierde: dat de geschiedschrijving de wetenschap zo nooit echt kan corrigeren. Ik ontken dat dit zou volgen uit mijn positie: door te bemiddelen tussen heden en verleden van de wetenschap, kan de geschiedschrijving *juist* ook licht schijnen op die wetenschap. Voor zover de scheikunde zichzelf definieert in contrast met de haar voorgaande alchemie, kan nieuw inzicht in de vroegmoderne alchemie (mede mogelijk door kennis die we menen te hebben van hoe de natuur zich in die tijd gedragen zal hebben) dat historische zelfbegrip bijstellen. En begrip van hoe de excentrieke Romeinse opvatting dat knoflook een demagnetiserende werking had de status kon krijgen van een robuust ‘empirisch’ feit, leert ons iets over de status van onze eigen omgang met de grens tussen empirische en niet-empirische kennis – en dit is des te meer zo als we, zoals de meesten van ons zullen doen, ervan uitgaan dat knoflook niet écht een demagnetiserende werking heeft! Als we er, anders gezegd, van uit gaan dat die rare Romeinse wetenschap wel een historische afstand heeft tot onze eigen wetenschap, die door geschiedschrijving overbrugd moet worden; maar dat ze wel over een gedeelde wereld gaat.

Curriculum Vitae

Jeroen Bouterse was born in Rotterdam in 1988. After completing his secondary education at the Gymnasium Erasmianum in Rotterdam, he completed his BA in history at Leiden University (*cum laude*), and research masters in Ancient History and Historical and Comparative Studies of the Sciences and Humanities at Leiden University and Utrecht University respectively (*cum laude*). Between 2011 and 2015, he worked as a PhD student at the Institute for Philosophy at Leiden University, participating in an NWO-sponsored project on the 'Philosophical Foundations of the Historiography of Science', led by dr. James W. McAllister. During this time, he also obtained a teaching qualification in history at the University of Amsterdam. He now works as a mathematics teacher at the Krimpenerwaard College in Krimpen aan den IJssel.