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

Bouterse, Jeroen

# 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

[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

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[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:

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[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.