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Contingency, Nature and Hermeneutics in History of Science

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Abstract

When, as historians, we want to explain developments in the history of natural science, how are we to do justice to the role of the natural world – the thing scientists investigate – in our explanations? The idea that the structure of the natural world renders the development of science inevitable seems to be inadequate, but so does the idea that we should explain the history of science without any reference to nature, as if what scientists study made no difference at all to what they believe. Is 'nature' even a feasible category, however? To what extent is it a problem that in referring to the result of scientific development in our explanation of scientific development, we are assuming the authority of science? Does this undermine the possibility of critical and independent historiography? This article deals with several possible solutions to these problems, and outlines an alternative to rationalism as well as to the Strong Programme in the Sociology of Scientific Knowledge and Latour's Actor-Network Theory.

Keywords

historiography of science – rationalism – SSK – ANT – Latour – hermeneutics

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In recent decades, history of science has experienced the mixed blessing of a multitude of methodological debates – debates about core questions like which kinds of things can actually be invoked in accounts of science, and who can say what about science. These debates had everything to do both with philosophical positions concerning the status of scientific knowledge, and with the historical development of the emancipation of history of science from the sciences whose history it seeks to write.[1]

At stake were, for example, the idea that science delivers eternal truths about nature; or the idea that when we study past science, we can measure it by our own scientific knowledge. The general consensus in history of science is now that ideas like these are tainted with Whiggism – the wrongheaded supposition that the beliefs that we hold now have always been a *telos* of the history that produced them, and that they can therefore retrospectively provide structure to this preceding history. Just like historians of political thought have learned not to *start* at the truth or universal desirability of liberalism and then explain its historical triumph, self-respecting historians of science will think twice before slipping into remarks that could be interpreted as assuming that science has its current form just because that is the form it has in the best of all possible worlds. Indeed, the history of science is decidedly not the removal of obstacles to some history-transcending Science-with-a-capital-S that, given free rein, developed in its most rational form.

In practice, this anti-Whiggism seems to be related to the idea that our own ideas of what nature is like ought to play no role in our historical accounts of past science. This is understandable, given that the alternative view has connected (as we will see later) the inevitability of the content of science with its special relation to nature.

If I seek to modify the consensus, then, I only seek to do so in a very specific way. In particular, I will try to do justice to the historian's belief in contingency and path-dependence, and not assume the inevitability of current science. Under those restrictions, I will argue the following points: that we cannot categorically exclude nature from playing an explanatory role in the history of science; and that relatedly, history of science cannot be completely autonomous with regard to the science whose history its writes.

The argument will be built up in the following way. Section 1 analyzes the concepts and stakes in the debate around the contingency or inevitability of science, and suggests that there may be an affinity between the position in this

1 One example of a polemical contribution to this debate is P. Forman, 'Independence, not transcendence, for the historian of science', *Isis* 82.1 (1991) 71–86. *Cf.* S.G. Brush, 'Scientists as historians', *Osiris* 10 (1995) 214–231.

debate and the kinds of entities that serve in explanations in history of science. Section 2 looks at the role of rationality. Section 3 looks at possible arguments to remove nature from the history of science for the benefit of an exclusive focus on social factors, and rejects most of those arguments. Section 4 analyzes the possibility that, since nature is *itself* the result of historical development rather than one of the causal influences on it, it has no explanatory value. This ends with the conclusion that in spite of the arguments discussed in the preceding section, the historical relation between science and nature remains a problem. Section 5, finally, draws inspiration from hermeneutic theory to shed light upon this relation and finish the argument.

1 Contingentism and Inevitabilism

The debate between contingentism and inevitabilism is, essentially, a debate about the question whether science 'could have' developed differently. In its traditional formulation by Hacking, the question is whether science could have developed differently *and* been as successful as actual science.[2] This, however, is a question for philosophy of science, not for history of science – it presupposes a yardstick for success, whereas in history of science we can dodge this nasty issue and ask whether science could have developed differently *simpliciter*.

Of course, counterfactuals are never simple. What do we even mean by this 'could'? Do we mean that the history of science is indeterministic? Conventional usage suggests this: contingency is contrasted with necessity, and necessity and determinacy seem closely related. But in the context of history, they are, in fact, better separated, and contingency is best understood in terms of sensitivity to initial conditions.[3] That is: the extent to which later

2 I. Hacking(2000), 'How inevitable are the results of successful science?', *Philosophy of science* 67 (proceedings): 58–61; L. Soler, 'Are the results of our science contingent or inevitable?', *Studies in history and philosophy of science* 39 (2008) 221–229; L. Soler, 'Revealing the analytical structure and some intrinsic major difficulties of the contingentist/inevitabilist issue', *Studies in history and philosophy of science* 39 (2008) 230–241: 230–231.

3 Y. Ben-Menahem, 'Historical necessity and contingency' in: A. Tucker ed., *A companion to the philosophy of history and historiography* (Malden, Oxford) 120–130: 120–121. *Cf.* also J. Beatty, I. Carrera, 'When what had to happen was not bound to happen: history, chance, narrative, evolution', *Journal of the philosophy of history* 5 (2011) 471–495. I also find attractive the idea that contingency may be defined in relation to path-dependency; however, this does require a certain amount of indeterminacy, though it is still not identical to it. (*Cf.* S.E. Page, 'Path dependence', *Quarterly journal of political science* 1 (2006) 87–115.)

states can differ more or less significantly dependent on differences between possible earlier states. If determinism is the belief that any state of a possible world at a given time is compatible with only one state at any later time,[4] then determinism does not decide this question.

And in practice, this is the question we are interested in when we debate contingency or inevitability in the history of science. Not: whether events are determined or not. But: whether there are alternatives conceivable that share some specific circumstances with ours, but that lead to significantly different outcomes. In this case, affirming contingency has the rhetorical function of showing that certain crude explanations are insufficient and making room for more detailed explanation. Thus, when Bruno Latour seeks to understand why Pasteur and the Pasteurians were believed, he says that we first need to "convince ourselves that this was not necessary."[5] Latour does not seem to mean that it was not determined or that it was inexplicable, since he goes on to explain it (by giving an account of the groups that Pasteur was able to recruit thanks to his various movements). The point is rather that Pasteur's success was not *inevitably* linked to the content of his science.

In general, there is no contradiction between the attempt to increase the explicability of historical events (under the implicit regulative assumption of determinism)[6] on the one hand, and the claim that from a given perspective (or from a certain level of abstraction) different outcomes were possible on the other. Usually, a historian will seek to show that under a certain conceptualization, there are possible timelines leading to significantly different outcomes, but that her own proposed explanation shows how the actual history largely determined the outcome.

Thus, when Steven Shapin and Simon Schaffer say that "we want to show that there was nothing self-evident or inevitable about the series of historical judgments [in the context of the English natural philosophical community] which yielded a philosophical consensus in favour of the experimental programme",[7] and then continue to provide a lengthy account that in the end

4 This definition is less general than that of J. Earman, *A primer on determinism* (Dordrecht etc. 1986) 12–14, who says that those worlds are deterministic which, if identical at *any* time, are identical at *all* times.

5 B. Latour, *The Pasteurization of France.* A. Sheridan, J. Law transl. (Cambridge (Mass), London 1993) 61.

6 E. Nagel, 'Determinism in history', *Philosophy and phenomenological research* 20 (1960) 291–317.

7 S. Shapin, S. Schaffer, *Leviathan and the air-pump. Hobbes, Boyle and the experimental life* (Princeton 1985) 13.

leaves the reader with the satisfied feeling that the victory of experimental science falls neatly within the limited range of outcomes consistent with the social and political situation of Restoration England, they do not contradict themselves. If they actually wanted to show that the way the social and political cards were dealt could have led equally well to Hobbesian science, they did a rather bad job, since most of the forces in their book seem to favour Boyle instead of Hobbes – summed up in succinct and rather deterministic language in the final chapter: "he who has the most, and the most powerful, allies wins."[8] Rather, what Shapin and Schaffer seek to make clear is that Boyle won not simply because he was right: given different social and political circumstances in England, Hobbes' philosophy might have carried the day.

The negation of inevitability – i.e. the affirmation of contingency – seems to have a certain affinity with a focus on social, cultural and political factors, then: room is made for these factors by suggesting that 'being right' is irrelevant or at the very least not enough, or perhaps in itself relative. Being an inevitabilist to Latour's and Shapin-and-Schaffer's contingentism (for you will always need to be a contingentist or inevitabilist with regard to the relation between a certain set of given conditions and a certain set of outcomes) would seem to mean: saying that the state of science is dependent not on culture but only on entities whose stability we take for granted, like nature and scientific rationality. Steven Weinberg, for example, says that the laws of nature as we know them 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."[9]

Indeed, Weinberg's affirmation of the culture-independence of scientific knowledge goes together with an inevitabilist view of scientific knowledge in the sense explained above, of high insensitivity to initial conditions: "the kind of physics I have done for most of my life [. . .] is moving toward a fixed point. [When this is reached,] our work as elementary particle physicists is done, and will become nothing but history."[10]

These examples suggest that a belief that science is 'nothing but' a description of nature goes together with inevitabilism about its content, while contingentism about the content of science entails saying that more historically

8 Ibid., 342.

9 S. Weinberg., 'Physics and history' (1996) in: S. Weinberg, *Facing up. Science and its cultural adversaries* (Cambridge (Mass), London 2001) 123–137: 136.

10 Ibid., 137.

variable factors have influenced this content. It is understandable, then, that in their aim to make room for genuinely 'historical' factors, historians of science have needed to make the case that 'the way nature *is*' does not fix the way science is.

2 The Role of Rationality

How *could* nature fix scientific beliefs, anyway? Assuming that nature is the same for everyone, it clearly does not fix any beliefs, for history contains many different actually held beliefs, none of which is, apparently, excluded by the way nature *is*.

Inevitabilists will need to add something, then: they will need to say that the relation between science and nature is special, because it is governed by context-invariant attitudes towards nature: that it is highly independent of those other activities that do depend on cultural, social and other historically variable factors. When we think about a normatively defined independence of science from other institutionalized activities, the name of Robert Merton comes to mind, who famously formulated a set of 'institutional imperatives' that characterized the ethos of science (as a cultural institution).[11] In this case, the role of society is restricted to its possibility either to allow science to exist by refraining from intruding on it, or to intrude on science and derail it with its temporally and locally restricted considerations.[12] Science, as it is defined, is 'governed' by its own values.

But can we talk about epistemic values, virtues or norms that 'govern' the relation of scientific theories to nature, without declaring science to be outside the reach of historical study? One response is to dodge this question by simply forbidding references to rationality – as has been advocated by the Strong Programme in the Sociology of Scientific Knowledge (SSK). But is there an alternative view, which does more justice to the scientific self-image without sacrificing historical and contextual sensitivity?

Evidence that belief in scientific rationality can be combined with historical sensitivity with regard to this rationality can be found, I suggest, in the methodological essays of Max Weber. Weber, it needs no explanation, clearly dichotomizes what is subjective and what is objective, and rationality belongs to the second domain – to the things about which people *ought* to agree; about

11 R.K. Merton, 'Science and technology in a democratic order', *Journal of legal and political sociology* 1 (1942) 115–126.

12 R.K. Merton, 'Science and the social order', *Philosophy of science* 5 (1938) 321–337.

which they cannot legitimately differ.[13] Nonetheless, when Weber argues that the possibility of a science about ethical judgments does not mean that this science could ever decide an ethical judgment to be right, the analogy he chooses is one in history of science: giving an account of Chinese astronomical beliefs can never aim at proving these beliefs to be right.[14] The analogy spans subjective desirability and objective validity.

The point is that this kind of research can never go beyond "*verstehend* [...] *erklären.* Das ist nichts Geringes."[15] Weber makes a connection between understanding and explanation that may require elucidation for those who are used to separating these: it is only by articulating our own experiences in terms of values that we can construct meaningful wholes in reality and see how these cohere. (In their most pure forms, these are the famous ideal types.) This is true for what we call the humanities and the natural sciences alike, but the understanding of the actions of other people is a special case because it is itself also oriented towards meaningful values (though, mind you, with Weber we only have indirect access to the values of others).[16]

For Weber, rational action is understood differently from – better, more easily than – irrational action. This, of course, goes against SSK teaching. The crucial question is why Weber believes this: do we understand rational action because it is *valid* or because it *resembles* our attitudes more? That is, is rational action explained *by* its own validity? In my interpretation, this is not the case: I think that Weber considers the judgment that a given course of action was rational to add nothing to its causal explanation. "[W]enn das normativ Gültige Objekt *empirischer* Untersuchung wird, so verliert es, als Objekt, dem Norm-Charakter: es wird als 'seiend', nicht als 'gültig' behandelt."[17]

An example. When we say that some accountant in the past has made a 'mistake' in multiplication, we use our own knowledge of the multiplication table in two rather different ways. We suppose its *normative* validity in our own accounting work, and we suppose its *conventional* application in the situation of this past accountant. Its validity there is not a concern of our study; it is

13 M. Weber, 'Die Objektivität sozialwissenschaftlicher und sozialpolitischer Erkenntnis' (1904) in: Max Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*. J.C.B. Mohr ed. (Tübingen 1922) 146–214: 155.

14 M. Weber, 'Der Sinn der "Wertfreiheit" der soziologischen und ökonomischen Wissenschaften' (1917) in: Weber, *Aufsätze*, 451–502: 464.

15 Ibid., 465.

16 M. Weber, 'Roscher und Knies und die logischen Probleme der historischen Nationalökonomie' (1903–1906) in: Weber, *Aufsätze*, 1–145: 115–131; M. Weber, 'Ueber einige Kategorien der verstehenden Soziologie' (1913) in: Weber, *Aufsätze*, 403–450: 404–407.

17 Ibid., 493.

simply a set of social rules that we notice he has failed to conform to. However, this does not mean that we can escape or ignore our current familiarity with the multiplication table, for without it, we would not even have been able to understand what the accountant was doing.[18]

In this view, our own norms of rationality are important not because of the transcendental validity we attribute to them, but because of the hermeneutic role they play: they provide a way through our own normative beliefs to an effective construction of the object of our historical interest, the 'historical individual'.[19] This is a most useful alternative both to the idea that 'neutral' historical research forbids us to distinguish between rational and irrational activity in the past, and to the idea that whatever is 'right', 'rational' or otherwise in tune with universal values somehow explains itself.

Even the strongest belief that our own ways of scientific reasoning are the only valid ones does not take away the slightest bit from our duty to explain causally why any individual or school in the history of science believed what they did. The corollary is that relativizing our own ways of reasoning does not equip us any better for a contextual study of past science. Both metaphysical positions are, it turns out, equally open to the insight that people in the past are not 'governed' by *our* reasoning habits, by what happens in *our* heads. This is one blow against the inevitabilists: even if they could make the case that their rationality is transcendentally valid, they are hardly any closer to making the case that it is omnipresent.

3 Leaving Nature Out?

At the other end of the spectrum, opposite to the first extreme according to which the relation between nature and science is so strict that all other factors drop out of the explanation, is another extreme, which I will abbreviate to 'NN' (for 'No Nature'), according to which we should not refer to nature at all as an *explanans* in the history of science.

In this section, we will take a look at possible arguments in favor of this position, which have in common that they seek the explanatory factors in society rather than in nature. Not all of these arguments imply social constructivism, it may be worth remarking. We will briefly discuss four of these arguments:

18 Ibid.

19 *Cf.* also A. Kedar, 'Ideal types as hermeneutic concepts', *Journal of the philosophy of history* 1 (2007) 318–345.

3.1 *Nature Underdetermines the Content of Scientific Theories*

Underdetermination is a well-known issue in philosophy of science, which David Bloor has used to bolster the SSK programme.[20] Now, 'underdetermination of theory by data' is an ambiguous concept. No one will deny that reality purely on its own underdetermines what is said about reality: even the staunchest rationalist will likely admit that it is possible to say anything in response to whatever evidence. She will certainly agree that it would have been possible for me to *fail* to come up with the Newtonian theory of universal gravitation even with all the data that Newton had. It is vital, then, that some kind of reasoning is agreed upon that connects the data and the theories. Some people will say that reality in combination with some plausible model of rationality greatly reduces or completely annihilates this underdetermination.

The case for underdetermination then depends on the permissiveness of this model of rationality. As Laudan has argued, it is relatively trivial to show that multiple theories are *compatible* with the same data or that they *entail* the same data (are empirically equivalent); but it does not follow that there cannot be a rational ampliative logic according to which some theories are better *supported* by the data than others.[21] It is one thing to say that creationism with divinely planted fossils is empirically equivalent to Darwinian evolution, and another to say that creationism and Darwinism have equal support.[22]

Ironically, the dependence of the case for underdetermination upon specific models of rationality sits rather uneasy with the famous symmetry thesis, which prescribes (among else) that explanatory work in the history and sociology of science should be done regardless of whether the studied schools, persons or decisions conform to some model of rationality.[23] But I take it that Bloor intends to argue something like this: that *any* model of rationality fails to solve or avoid the problem of underdetermination (i.e. that he says that reality in combination with any model of rationality underdetermines what scientists say or think about reality). Given the discussion in the previous section, I think that he is on strong ground if he claims that "[t]he empirical evidence suggests

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20 E.g. D. Bloor, 'Idealism and the sociology of knowledge', *Social studies of science* 26 (1996) 839–856.

21 L. Laudan, J. Leplin, 'empirical equivalence and underdetermination' in: L. Laudan, *Beyond positivism and relativism. Theory, method, and evidence* (Boulder, Oxford 1996) 55–73: 63–68.

22 S. Okasha, 'The underdetermination of theory by data and the "strong programme" in the sociology of knowledge', *International studies in the philosophy of science* 14.3 (2000) 284–297: 290.

23 Okasha, 'Underdetermination', 293–294; cf. E. McMullin, 'Underdetermination', *Journal of medicine and philosophy* 20 (1995) 233–252: 243–245.

that all institutionalized systems of belief are compatible with plausible models of natural rationality".[24] Or elsewhere: "[t]he historical literature on scientific controversy typically shows neither side compromising on what we may assume to be their natural reasoning propensities."[25]

However, I am not convinced that this needs to excite us very much. If underdetermination means nothing more than – and this is a lot – that there will always be theories that fit (evidence from) nature equally well (given some standard of rationality), then it is logically too weak to support NN. Only if we held a theory of 'non-determination', where different theories fit any possible structure in nature equally well, would NN be defensible on this basis. I know of no defenses of this thesis, and I can think of no plausible defense for an idea that implies that we would have believed the same as we do now whatever the evidence. We do not need to be inevitabilists about the content of science, then, in order to believe that nature exercises a genuine causal influence upon our beliefs about nature.

3.2 *Nature is Precisely What is Common to All Beliefs, so it 'Drops Out' of the Explanation*

David Bloor has made this point elegantly:

> 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.[26]

Bloor's argument has been attacked independently by Nick Tosh and T. Lewens. Tosh gives the example of bacteria which are exposed to heat, some of which

24 D. Bloor, 'The strengths of the strong programme' in: J.R. Brown ed., *Scientific rationality: the sociological turn* (Dordrecht, Boston, Lancaster 1984) 75–94: 86.

25 D. Bloor, 'Rationalism, supernaturalism, and the sociology of knowledge' in: I. Hronszky, M. Fehér ed., *Scientific knowledge socialized. Selected proceedings from the 5th joint international conference on the history and philosophy of science organized by the IUHPS, Vesprém 1984* (Dordrecht, Boston, London 1988) 59–74: 67.

26 D. Bloor, 'Anti-Latour', *Studies in history and philosophy of science* 30 (1999) 81–112: 93.

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.[27] 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: "[i]n 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 [...]."[28]

I consider this to be a strong argument against the point of Bloor's electron example. Bloor himself has not responded to Tosh and Lewens, but J. Kochan has brought against this line of reasoning that it fails to take account of the contrastive nature of explanation.[29] The question is not, for example, why Millikan believed in electrons *simpliciter*, but why he believed in electrons rather than sub-electrons. Kochan uses Lewens' Bigfoot analogy: 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.

The point about the contrastive nature of explanation is the strongest in Kochan's argument, and is, for the sake of Bloor's position, best taken to apply to the relation between common external factors and different social factors: if I want to explain why a certain belief was held in one society rather than another, the answer cannot be only a factor that is common to both. This argument is valid, but it is applicable to history of science only if we assume that all interesting questions about the history of science are contrastive, with the foils being *actually* held alternative beliefs. Once we allow the foils to be possible but non-actualized alternative beliefs that Millikan might have held, for example ("why did Millikan believe in electrons rather than in anything else"), there is no compelling reason why we should hold nature stable in thought. In that case, Tosh's and Lewens' objections to Bloor's point hold.

27 N. Tosh, 'Science, truth and history II: Metaphysical bolt-holes for the Sociology of Scientific Knowledge?', *Studies in history and philosophy of science* 38 (2007) 185–209: 186–191.

28 T. Lewens, 'Realism and the Strong Program', *British journal for the philosophy of science* 56 (2005) 559–577: 572.

29 J. Kochan, 'Contrastive explanation and the "Strong Programme" in the Sociology of Scientific Knowledge', *Social studies of science* 40 (2010) 127–144.

3.3 *Historians of Science Should Restrict Themselves to Factors that Lie within their Own Field of Expertise, and that Means Society, not Nature*

Harry Collins has advised "deliberately averting the gaze from scientific arguments so as to investigate the social relations of the science more assiduously."[30] Again, this is not a radical social constructivism, but a methodological relativism, making the point that what we can *see* as historians (or in Collins' case, sociologists) is limited by the kinds of sources we have and the kinds of methods we can apply to them: "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."[31]

This is a very interesting point, which raises the pressing question who can say what about what, and why. But surely, the issue of disciplinary competence, on its own, is not enough to support NN and write nature out of the picture. Social historians can ask economic historians about wheat prices if they are relevant to the social-historical developments; everyone is allowed to ask other experts about things outside their own expertise relevant to their own questions. The fact that *we* as historians cannot *see* nature does not mean that we cannot speak about it; the difference is that we get our knowledge about nature from authority – from the authority of natural scientists.

But perhaps, then, there is an important peculiarity to the predicament of the historian of science, namely that invoking beliefs about nature in explaining those same beliefs about nature is circular. We will leave this issue undecided here, and return to it in the last section.

3.4 *Nature is the* Result *of Social Action, Not the Cause*

This is the social constructivist argument for NN, suggested for instance by Karin Knorr-Cetina when she emphasizes the "active constitution of facticity through science."[32] Importantly, she does so not on the basis of some kind of philosophical idealism, but based on "direct observation of the *actual site of scientific work* (frequently the laboratory)."[33] The point is that all decisions we

30 H.M. Collins, *Gravity's shadow. The search for gravitational waves* (Chicago, London 2004) 793.

31 H.M. Collins, S. Yearley, 'Journey into space' in: A. Pickering ed., *Science as practice and culture* (Chicago, London 992) 372.

32 K.D. Knorr-Cetina, *The manufacture of knowledge. An essay on the constructivist and contextual nature of science* (Oxford etc. 1981) 2.

33 K.D. Knorr-Cetina, 'The ethnographic study of scientific work: towards a constructivist interpretation of science' in: K.D. Knorr-Cetina, M. Mulkay ed., *Science observed.*

see scientists make are locally situated, and do not revolve around the finding of truth about nature but around making things *work*. Other than constructivist accounts need to appeal to factors that fail to manifest themselves in the empirical world of the observer of science.

The question is, of course, whether Knorr-Cetina can give an account of how scientific facts and theories get to be produced without even implicitly referring to 'nature' or 'reality' in ways other than as something the production of which is also subsumed under her explanatory account. In that regard, her analogies – psychiatrists who do not need to have descriptively adequate explanations about their patients' disorders in order to treat them effectively, and a mouse that does not need to have adequate representations of cats in its mind in order to flee from them – are rather disheartening. "Like the progress of evolution itself", Knorr-Cetina says, "the progress of science can be linked to mechanisms which do not assume that knowledge mimics nature."[34]

This is, effectively, an adaptationist account of the relation between science and nature. But adaptation hardly makes sense unless there is something adapted *to* – the mouse may be adapted to a cat-holding environment without needing to mimic this environment in his head; but surely then, its fleeing behavior is explicable to us partially because there we believe an actual cat to be present, not just a fabrication of the mouse. An analogy between the fabrication of scientific theories and biological evolution, then, works against constructivism.[35]

4 The Co-Fabrication of Nature and Science

But perhaps all the arguments – and their supposed rebuttals – in the previous section share the mistaken assumption that we can usefully distinguish between society and nature, as if such a distinction is part of a necessary order. Why would we classify explanatory factors into 'natural' and 'social' and then link them again? Bruno Latour has made a strong and consistent case that

Perspectives on the social study of science (London, Beverly Hills, New Delhi 1983) 113–140: 113–115; cf. Also ibid., 136, where Knorr-Cetina characterizes her program as not subjectivist or relativist, but "the working out of an *empirical, constructivist epistemology* which conceives of the order generated by science as a (material) process of embodiment and incorporation of objects in our languages and practices."

34 Knorr-Cetina, *Manufacture of knowledge*, 2.

35 *Cf.* S. Cole, *Making science. Between nature and society* (Cambridge (Mass), London 1992) 33–60.

this "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'."[36]

The point is clear: interactions between a huge diversity of entities are manifold, and dual classifications and the corresponding expectations and vocabularies are the result of history, not its immovable movers. This means that what we have seen Knorr-Cetina say about nature – that it is the result of 'fabrication' – Latour says about both nature and society: neither is a natural class that we can fall back on or build bridges from.

> 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.
> 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.[37]

Wait, though. Doesn't Latour seem to slide from 'Nature's representation' to 'Nature' here? Yes, and this is not just a slip of the tongue: rather, Latour holds the counterintuitive position that these two should be regarded as interchangeable. There is not one stable, natural world against many different historically developed representations of that world – a multiculturalism that "acquires its right to multiplicity only because it is solidly propped up by *mononaturalism*";[38] no, there is actually a 'pluriverse', of worlds that are not 'just there' but that are the results of the diverse actions of actors.

To support this point, Latour attacks the distinction between the independence of reality and what is done to create that reality. It is not the case, he argues, that because something is constructed, it becomes *ipso facto* less real (because grounded to a lesser extent in the way the world really is); rather,

36 B. Latour, *Reassembling the social. An introduction to Actor-Network Theory* (Oxford 2005) 75–76.

37 B. Latour, *Science in action. How to follow scientists and engineers through society* (Cambridge (Mass), 1987) 258.

38 B. Latour, *Politics of nature. How to bring the sciences into democracy* (Cambridge (Mass), London 2004) 33.

real things can be constructed – we already believe this about buildings,[39] or about engines;[40] so why not about 'natural' entities, since they are not a fundamentally different class of things? This is an interesting point to historians: it suggests that history can produce solid and independent things whose solidity does not derive from their supposed history-transcendence.

Indeed, Latour argues for a 'generalized historicity'. Entities, and classes of entities, *pass* from nonexistence to existence through fabrication – and this is the result of work being done: "in his laboratory in Lille Pasteur is *designing* an actor."[41] This does not mean autonomous construction *ex nihilo*; the adding of a new actor (the microbes that Pasteur 'discovered') is a result of what happens in the entire network in which Pasteur moves.

Why is this redescription important? Because it means that there is, fundamentally, no difference between the microbe as a 19th-century scientific discovery and the microbe as an actor in the 19th-century Pasteurian network. This, in turn, means that the relation between science and nature *ceases to be a problem*. It used to be that the 'fit' between scientific theories and the world was a miracle (especially if you believed, as SSK suggests, that the former are to be explained independently of the latter, as in some pre-established harmony). But if we follow Latour in saying that theories and the world are constructed in the same movement, we see that there is no distinction between the inside world of science and the outside world to which science is applied; "it is still interesting, extraordinarily clever and ingenious, but it is *not* a miracle."[42] Or elsewhere: "[m]iracle indeed to see a clover-leaf intersection fitting *precisely* with the freeways whose flow it redistributes!"[43]

If. Because here we need to stand firm in spite of Latour's elegant efforts to patch things together and break down all kinds of dichotomies, and note that what we now believe about microbes in the 19th century may just be something different from what we now believe that 19th-century people believed about microbes. It is hard to contradict Latour's philosophy without begging the principle you are defending against him, but I believe a wedge can be placed here: there is no need to contradict Latour's point that our distinctions

39 B. Latour, *Pandora's hope. Essays on the reality of science studies* (Cambridge (Mass), London 1999) 113–173.

40 B. Latour, *Aramis or the love of technology* (Cambridge (Mass), London 1996) 23.

41 Latour, *Pandora's hope*, 122.

42 B. Latour, 'Give me a laboratory and I will raise the world' in: Karin Knorr-Cetina, Michael Mulkay ed., *Science observed. Perspectives on the social study of science* (London, Beverly Hills, New Delhi 1983) 141–169: 151.

43 Latour, *Science in action*, 242.

between nature and society are historically contingent artifacts, and not even (for current purposes, that is) his point that our whole outside world, or any world, has only an existence relative to local historical networks, if only we insist that our network may be such that it has come to contain both (representations of) microbes and (representations of) 19th-century scientists talking about microbes, and that we want to see whether and how these connect.

There are two points I want to make here, then. First, the affirmation of the radical history-dependence of the boundaries we draw cannot be used as a step to escape our history-dependence: we cannot jump out of our own 21st-century locality and see the 19th-century Pasteurian network for what it 'really' was independently of us. Second, given the boundaries we (or some of us) draw in practice, there may be instances where the relation between natural entities (like microbes) and scientific entities (like beliefs about microbes) again becomes an issue about which meaningful questions can be asked. It may be in Latour's power to change the metaphysical status we give to some of our own categories and beliefs; it is not in his power to change them all at once.

5 Towards a Hermeneutic Philosophy of History of Science

Is an alternative possible to Latour's attempt to maintain a complete independence from the scientific tradition? Emphasis in reflections upon the history of science has usually been on the importance of independence of history from the science whose history it writes.[44] Thus, Shapin and Schaffer talk about the importance of 'playing the stranger' with respect to the scientific culture with which we are familiar.[45]

The idea that it is more useful to think of ourselves in relation to science as *part* of a tradition and in dialogue with that tradition has been advocated by, among others, Martin Eger. Scientists, Eger says, are interacting not just with nature, but (largely) with the collective body of interpretations of nature; they are reading the book of science at least as much as the book of nature.[46] Rather than seeing this as an insight that 'un-makes' science by showing its circular aspects (as Collins does when he sees a vicious circle in his notion of

44 Forman, 'Independence'.

45 S. Shapin, S. Schaffer, *Leviathan and the air-pump. Hobbes, Boyle, and the experimental life* (Princeton 1985) 6.

46 Martin Eger, 'Hermeneutics as an approach to science', *Science and education* 2 (1993) 1–29, 303–328.

the experimenter's regress; a circle that needs to be broken),[47] Eger proposes to see this circularity as a feature of genuine science: for hermeneutic thinkers, it does not come as a shock that a dialogue takes place between scientists and their tradition.

Robert Crease has defined the role of hermeneutical philosophy in the context of natural science as asserting the priority of meaning over technique, of the practical over the theoretical, and of situation over abstract formalization.[48] This supports a view of science as a thoroughly cultural activity.[49]

What I want to propose here, is to extend these claims to the writing of *history* of science as well: for historians of science no less than for scientists, the weight of the entire preceding scientific tradition is inescapable. Hermeneutic theorists have tended to overlook this point, probably because their theories partially served to underline the autonomy of the humanities with respect to the natural sciences and were thus best served by assuming the correctness of conventional views about natural science.[50] Thus, in 1960 even Gadamer presented the historicity of natural science as an almost accidental feature (though in a later edition, he added in footnotes that this had turned out to be too simplistic in the face of Kuhn's and other work in history of science).[51]

What would affirming the hermeneutic nature of history of science imply? The emphasis in a hermeneutic philosophy of history of science is on *understanding* science in history, rather than on criticizing or trivializing it. As such, it is a different business that does not contradict the *possibility* of 'playing the stranger' (that is, of artificially ignoring certain *aspects* of the tradition we have inherited), but provides a coherent alternative to it: an alternative that, however, cannot take place in complete independence of current scientific knowledge (and, relatedly, practice).[52] The crucial idea is that we understand past

47 Eger, 'Hermeneutics', 98–99, about H.M. Collins, Changing order: replication and induction in scientific practice (London, Beverly Hills, New Delhi 1985).

48 R.P. Crease, 'Hermeneutics and the natural sciences: introduction' in: ibid. ed., *Hermeneutics and the natural sciences* (Dordrecht, Boston, London) 1–12.

49 Patrick A. Heelan, 'Why a hermeneutical philosophy of the natural sciences?' in: Crease, *Hermeneutics*, 13–40; J.J. Kockelmans, 'On the hermeneutical nature of modern natural science' in: Crease, *Hermeneutics*, 41–55.

50 *Cf.* G. Freudenthal, 'the hermeneutical status of the history of science: the views of Hélène Metzger' in: E. Ullmann-Margalit ed., *Science in reflection. The Israel colloquium: studies in history, philosophy and sociology of science* III (Dordrecht, Boston, London 1988) 123–144.

51 H.-G. Gadamer, *Wahrheit und Methode. Grundzüge einer philosophischen Hermeneutik* (Tübingen 2010 [1960]) 286–290.

52 G. Markus, 'Why is there no hermeneutics of natural sciences? Some preliminary theses', *Science in context* 1 (1987) 5–51: 21–22.

dealings with natural phenomena because *we* are ourselves familiar with similar phenomena.

As the historian of ancient science Daryn Lehoux puts it, in a brilliant introduction to his book *What did the Romans know*, "[t]he ancients talk about rocks, and I think I can understand them. But I think I can understand them not just because I have read their writings on rocks carefully, but also because I think I know something about rocks from my own experience, and that experience both helps and hinders my attempts to understand their ideas about the world."[53] This does not apply just to accidental characteristics; it is hard to see how we would even recognize past dealings with the external world as being *about* that, if we did not have our own knowledge of this world. Our own beliefs about nature, which we bring to the reading of past scientific texts, are crucial to our understanding of those texts.

But – and this is the second crucial idea – our own beliefs are also at least partially (to a greater or lesser extent, depending on your position in the contingentism-inevitabilism debate) the result *of* a historical path of which the texts we read are part. Is this a problem?

Contingency may appear as a problem proceeding from an ideal of radical independence of history writing from history; an ideal in which our interpretation of history is not colored by *Vorurteile* about what science is and what it is about. But in this case, we expect of the historian of science a degree of history-transcendence that we have just denied to scientists when we historicized their work, declaring it to depend on a previous scientific tradition and in dialogue with that tradition. Historical contingency is a problem if we believe that it implies unreliability; that the only things on which we ought to rely are those that are not dependent on history. This is an understandable intuition, but one that has no place in an ontologically hermeneutic philosophy.

Is the *circularity* a problem, then? Are we 'assuming what we want to explain' when we assume in our historical work the reliability of scientific beliefs which we also believe to depend on the history that we have yet to describe? Let it be noted first that a circularity of this kind is a problem only when studying our own scientific tradition. Second, in many cases a specific historical episode may be causally relevant to our current beliefs, but not in the sense that it was a necessary condition. For example, even if the Galileo controversy is a causally relevant part of the history of our beliefs about the solar system, our own rejection of geocentrism has so many grounds of which so many are independent of the 17th-century controversy that we can assume it without fear of circularity.

53 D. Lehoux, *What did the Romans know? An inquiry into science and world making* (Chicago, London 2012) 16.

Genuine circularity arises only when the events studied are completely responsible for the beliefs that we want to explain. In individual case studies, this situation may present itself when we study small communities in the recent past doing cutting-edge science (like the sociological studies of Harry Collins), and as such, it is not typical of historical study. It may also be argued, however, that history of science 'as a whole' is in this predicament: we seek historical explanations of the history of science 'as a whole', and we need to assume in our descriptions and explanations the validity of some of the results of this 'whole'.

But if this is a problem, it is not a problem peculiar to history of science; all intellectual history (writing) assumes some of the results of (developments in) intellectual history. On an even broader scale, our ability to engage in historical writing depends on a contingent history, and this dependence never seems to bother us when we proceed to write about this same history.

The history-dependence of history of science may be a source of confusion, and of apparent vicious circularities and presentisms, but in the end, there are no clear arguments against employing the best knowledge and the best conceptual resources that we possess when studying our own history, even if this knowledge and these resources depend on this history.

Conclusion

Embracing the predicate 'hermeneutic', then, signifies that there is a coherent attitude alternative to the various existing options that we have assessed – an alternative that has some characteristics in common with all these other options, but that acquires its coherence from its affirmation and acceptance of both the contingency and the inescapability of current scientific opinion.

To the rationalists, we concede the acceptability of presentism, while defending against them the initial otherness of the past. To the social reductionists, we concede the pervasive influence on scientists' opinions of things that they do not study, while maintaining against them that science is not understandable without taking into account what it is *about*. To the actor-network theorists, we concede the causal intertwining of nature and society and the locality of scientific practice, but refuse to collapse the distinction between things in the world and opinions about those things.

To the inevitabilists, we say that we understand their intuition that a science radically different from ours is inconceivable, but we also point out that if the history of science is causally relevant, it must be considered to have made a difference, and that therefore, it is a constitutive assumption of history of science that there is a significant degree of contingency to our current situation. More importantly, we have learned that no one class of factors can be isolated that alone determines the outcome of scientific development, so inevitabilism is hard to substantiate. To the contingentists, therefore, we say that they are right that our current views on nature and science are not the only possible ones, that they were not historically inevitable; however, we add that this in no way diminishes their *hermeneutic* inevitability, i.e. the inescapability of their status as the point of departure of our historical interpretations.

We cannot understand science without assuming things about the world, and what we can assume about the world is handed to us by the scientific tradition we have inherited. This does not mean that those assumptions are unchangeable; our meta-beliefs about what science is and what it is about, and even our substantive beliefs about the world, *could* in principle be altered as a result of confrontation with the historical material. The crudest example would be the discovery of deliberate fabrication of data, undermining the scientific status of the belief that we initially wanted to explain. The affirmation of our history-dependence does not preclude the possibility of criticism, then, we can find out things about science that do not fit current prejudices and interests.

One thing, however, is clear: when we seek to understand thinking about nature, neither history nor nature can be simply ignored.