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Chapter 5: Leaving Nature Out

5.1 The ‘No Nature’-principle

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

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

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

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

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

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

³⁰³ Danziger (1990, 1-6).

³⁰⁴ Pickering (1984, 406).

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

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

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

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

5.2 David Bloor’s Realism

5.2.1 Introduction

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

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

³⁰⁵ Pickering (1984, 7).

³⁰⁶ Pickering (1984, 8).

³⁰⁷ Bloor (1982, 281).

³⁰⁸ Bloor (1976, 2-5).

³⁰⁹ Bloor (1999, 93).

³¹⁰ Barnes, Henry, and Bloor (1996, 1).

³¹¹ Bloor (1976, 28).

³¹² Bloor (1976, 29).

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

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

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

5.2.2 The First Argument: Underdetermination

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

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

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

³¹³ Bloor (1976, 36).

³¹⁴ Bloor (1996, 853). Virtually the same quote appears in Bloor and Edge (2000, 159). See also Bloor (2011, 403).

³¹⁵ Bloor (1996, 841).

³¹⁶ Laudan and Leplin (1996, 63-68). See also the position of the non-relativist in Laudan (1988).

³¹⁷ Cf. Okasha (2000, 290).

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

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

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

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

³¹⁸ Okasha (2000, 293-294). For a similar confusion, see McMullin (1995, 243-245).

³¹⁹ Cf. Rorty (1979, 322); Laudan (1996, 17-19).

³²⁰ Bloor (1984, 86).

³²¹ Bloor (1988, 67).

³²² Bloor (2011, 402).

³²³ See also the distinctions between different underdetermination theses in Laudan (1996, 31-43); Douven (2008, 294-299).

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

5.2.3 The Second Argument: Nature Dropping Out

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

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

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

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

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

³²⁴ Bloor (1999, 87-91).

³²⁵ Bloor (1999, 93).

³²⁶ Bloor (1999, 88).

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

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

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

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

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

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

³²⁷ Tosh (2007, 186-191).

³²⁸ Lewens (2005, 572).

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

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

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

5.2.4 The Third Argument: The Social Determination of Classification

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

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

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

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

³³⁰ Bloor (1982, 267).

³³¹ Durkheim and Mauss (1903, 8-9).

³³² Durkheim ([1912] 1995, 14).

³³³ Bloor (1982, 269).

³³⁴ Quine (1951, 39).

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

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

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

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

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

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

³³⁵ Bloor (1982, 272).

³³⁶ Bloor (1982, 273-274).

³³⁷ Bloor (1982, 277).

³³⁸ Quine (1960, 5).

³³⁹ Bloor (1982, 278).

³⁴⁰ Bloor (1982, 278).

³⁴¹ Bloor (1982, 279-280).

³⁴² See Woolgar (1981) for a criticism of interests as resources for sociological explanations of science.

³⁴³ Durkheim (1912) 1995, 18-19.

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

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

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

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

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

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

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

³⁴⁴ Bloor (1982, 293).

³⁴⁵ Bloor (1982, 288).

³⁴⁶ See e.g. the arguments concerning the transplantation of Greek natural philosophy in different cultural contexts by Cohen (2010, 53-96).

³⁴⁷ McMullin (1984, 158).

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

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

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

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

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

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

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

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

³⁴⁸ Bloor (1997, 9-26).

³⁴⁹ Bloor (1997, 22).

³⁵⁰ Cf. Brown (1989, 54-56).

³⁵¹ Bloor (1997, 16). Cf. also *ibid.*, 36.

³⁵² Lynch (1992, 227-228).

³⁵³ Cf. Kusch (2004a, 571-572), who is critical of this argument.

³⁵⁴ Bloor (1992, 271).

³⁵⁵ Sharrock (2004).

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

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

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

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

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

³⁵⁶ Cf. also Kusch (2004b), who also notes that it is unclear precisely what kind of reduction of meaning, rule and rightness to social institutions Bloor has in mind.

³⁵⁷ Bloor (1997, 29).

³⁵⁸ Bloor (1992, 279).

³⁵⁹ Bloor (1996, 845-853).

³⁶⁰ Kemp (2005, 712-715).

³⁶¹ Bloor (2007, 216-220).

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

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

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

5.2.6 Bloor on the Hermeneutic Circle

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

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

³⁶² Bloor (1982, 281).

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

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

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

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

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

³⁶⁴ Barnes, Henry, and Bloor (1996, 18-45).

³⁶⁵ Barnes, Henry, and Bloor (1996, 25).

³⁶⁶ Barnes, Henry, and Bloor (1996, 28-30). Arguments concerning rationality slide somewhat too easily into arguments concerning rightness here.

³⁶⁷ Barnes, Henry, and Bloor (1996, 33).

5.3 Harry Collins' Relativism

5.3.1 Introduction

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

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

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

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

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

5.3.2 The Fourth Argument, Step 1: Methodological Relativism

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

³⁶⁸ Collins (1976, 437).

³⁶⁹ Collins (1981b, 218).

³⁷⁰ Collins (1993, 54).

³⁷¹ E.g. Collins (2004, 14).

³⁷² Collins (1993, 138).

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

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

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

Is this nitpicking? The controversy between SSK as represented by Bloor and Larry Laudan was precisely about the question whether it was acceptable to distinguish between rational and non-rational motives for scientific decisions, and deal with the non-rational motives in a different way than with the rational motives³⁷⁸ – Laudan would not equate this distinction with scientific versus non-scientific, of course, because he thought the demarcation problem to be a non-issue.³⁷⁹ If we take Collins’s words seriously, he needs to engage not just Laudan on the question whether it is meaningful to draw a distinction between scientific and non-scientific forces, but also Bloor on the question whether this

³⁷³ Collins (2004, 14-15).

³⁷⁴ Collins (2004, 793). Cf. also Collins (1981b).

³⁷⁵ Cf. Collins (1981a); see Collins (1983, 96-97) for an account of what the third stage would look like. Here Collins proposes that if the side in a controversy that has more resources emerges victorious, we can look at why that side came to have greater resources, which may be because the relevant scientific industry has vested interests in the preservation of current scientific understanding: in this case, if gravity waves had been discovered, “the work of the scientific sector of industry would be thrown into chaos on many fronts.” (97)

³⁷⁶ Collins (2004, 784).

³⁷⁷ Collins (2004, 785).

³⁷⁸ Laudan (1982b; 1984, 53-56) chastises Bloor for demanding that rational and irrational opinions are explained by the same kinds of causes; Bloor (1984) answers that reasons in the sense of collective standards can be reduced to forms of social organization.

³⁷⁹ Laudan ([1982a] 2009; 1984, 48-53).

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

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

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

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

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

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

³⁸¹ Collins and Yearley (1992a, 386).

³⁸² Collins and Yearley (1992b, 372), referring to Callon (1986).

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

[2] "People complained about wheat prices, then they overthrew their local government."

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

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

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

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

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

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

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

³⁸³ Cf. Goldman (1999, 9-17, esp. 14-16).

³⁸⁴ Gellner (1957, 235-236).

³⁸⁵ Needham (1960, 97).

³⁸⁶ Gellner (1960, 187).

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

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

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

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

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

³⁸⁷ Barnes (1961, 298).

³⁸⁸ Cf. Barnes (1964).

³⁸⁹ Gellner (1963, 199).

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

5.3.4 The Fourth Argument, Step 2: The Circularity Argument

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

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

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

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

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

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

³⁹⁰ Collins (1994, 791).

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

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

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

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

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

In this sense there is a hermeneutic circularity at play, a dialogical interaction between what we believe we know about science, and what we can learn from its history. But this interaction can take place only if our actual beliefs about science – for instance, that

it reliably teaches us things about the world – are part of it. As Philipp Pettit argued, the claims of the Strong Programme are much more exciting if we interpret them in a “non-conservative” way, where the findings of the sociologist *can* be subversive to the beliefs she studies.³⁹²

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

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

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

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

³⁹² Pettit (1988, 85-86).

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

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

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

5.4 Karin Knorr-Cetina's Constructivism

5.4.1 The Fifth Argument: Science Constituting Natural Facts

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

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

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

³⁹³ Cf. Forman (1991) and section 1.4; for an example of what Forman has in mind when he talks about historiography defined (and mangled) by the interests and prejudices of science, see Forman (1983).

³⁹⁴ Knorr-Cetina (1981, 2).

³⁹⁵ Knorr-Cetina (1983a, 113-115).

³⁹⁶ Knorr-Cetina (1983a, 136 [italicization removed]).

³⁹⁷ E.g. Knorr-Cetina (2000).

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

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

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

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

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

³⁹⁸ Knorr-Cetina (1983a, 122).

³⁹⁹ Knorr-Cetina (1981, 3).

5.4.2 Fabrication and Adaptation

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

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

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

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

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

⁴⁰⁰ Knorr-Cetina (1981, 2).

⁴⁰¹ Knorr-Cetina (1981, 2).

⁴⁰² Laudan (1981).

⁴⁰³ Hesse (1988).

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

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

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

5.5 Conclusions

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

- A1: *Nature underdetermines the content of scientific theories.* We have noted that the affirmation of underdetermination is not a denial of causal relevance.
- A2: *Nature is common to all of us and therefore 'drops out' of explanations.* We have noted that because different actors can have different relations to nature, this argument gives no compelling reason not to refer to nature.

⁴⁰⁴ Cf. Tosh (2006, 694-697).

⁴⁰⁵ Hacking (1999).

- A3: *Neither external reality nor individual minds provide us with the categories we employ to study nature, and therefore real explanatory power resides in society.* We have noted that social institutions are not wholly self-referential. The importance of society does not imply that nothing outside it matters to our explanations.
- A4: *If historians of science rely on knowledge of nature in their explanations of knowledge of nature rather than on their autonomous competence and expertise, their explanations are circular.* We have seen that in the end, this argument applies only where our current scientific knowledge depends on precisely the history that we study, and that in this case it also depends on inevitabilism with regard to the delineation of scientific knowledge. This is not a definitive rejection of A4, and we will return to the problem of circularity in later chapters.
- A5: *Nature is the result of social constructions, not its cause.* When we looked at an advocate of this argument, it soon turned out that it could be made to look coherent only if it meant something other than that scientists autonomously created their own environment. In a more complex alternative, then, nature cannot be regarded as just a social construction. We will deal with this in detail in the next chapter.