Chapter 2
How to Save the Symmetry Principle

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Abstract The symmetry principle is a central tenet of science studies, but clear statements of the principle are hard to find. A standard formulation is that *true and false beliefs should be explained in the same way*. This claim is multiply and harmfully ambiguous. The aim of this paper is to identify the main ambiguities and defend a more precise version of the symmetry principle. I argue that the principle should refer to types of cause not causes *in general*, that the relevant types are rational and irrational causes not social and non-social ones, that true and false beliefs should be explained impartially not identically, and that impartiality does not imply a ban on truth as an explanation of belief. The symmetry principle that emerges from these choices is that *historians should not assume in advance of historical inquiry that true beliefs are best explained rationally and that false beliefs are best explained irrationally*. I argue that this principle does what all symmetry principles should do: it is conducive to good historical writing, protects us from a genuine threat, makes room for the sociology of true beliefs, does not cast doubt on legitimate projects such as internal history of science, and does not commit us to controversial philosophical positions such as skepticism about present-day scientific theories.

2.1 Introduction

The symmetry principle is a central tenet of science studies—perhaps *the* central tenet of science studies—but clear statements of the principle are thin on the ground. According to a standard formulation, the principle is that *true and false beliefs should be explained in the same way*. This statement is multiply and harmfully ambiguous. Does it mean that all beliefs should be explained causally rather than acausally? Or does it mean that they should be explained using the same types of cause? If the latter, what kinds of cause do we have in mind? And what does "in the same way" mean? Should we really explain all beliefs in the same way, or should we

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simply keep an open mind about which explanations hold in any given case? Also, does the symmetry principle imply that some kinds of explanation are illegitimate? Or does it simply ask us to distribute our explanations evenly across true and false beliefs? Finally, how do we reconcile our equal treatment of past beliefs with our conviction—which most of us have—that all beliefs are *not* equal? Is it enough to treat the principle as a heuristic with no epistemological consequences, or is a more substantial response required? If the latter, what is the best response?

The aim of this paper is to distinguish between the various answers to these questions and to defend a particular answer to each. The result will be what I hope is a clearer version of the symmetry principle. To anticipate, the principle is the following: *historians should not assume in advance of empirical inquiry that true beliefs are best explained rationally and that false beliefs are best explained irrationally*. In short, historians should not use truth as a guide to rationality. I shall call this the Symmetry Principle, or the Principle for short (note the capital letters). Saving the symmetry principle means rescuing the Symmetry Principle from the many inferior maxims that go by that name.

I shall argue that the Symmetry Principle is more successful than other versions in meeting the following requirements. Firstly, it is sound. Historians who follow the Symmetry Principle will, all else being equal, give more accurate accounts of past and present science than those who routinely violate the Principle. Secondly, it is necessary in the sense that it protects us against an error that we are otherwise likely to commit. Thirdly, it performs the function for which the phrase "symmetry principle" was coined, namely to make room for sociological explanations of established scientific beliefs, as opposed to sociological explanations of scientific institutions or of discredited beliefs. Fourthly, the Symmetry Principle performs this function without prejudice to other goals that historians and sociologists of science can legitimately pursue. In particular, the Symmetry Principle says nothing against the practice of internal history of science. Finally, the Symmetry Principle does not require us to take sides in debates that are live ones in mainstream philosophy of science. I shall say more about these requirements when I invoke them in the course of my argument.

Given the number of articles and chapters that have been written on the symmetry principle, readers may wonder why another one is necessary. The short answer is that most of those articles and chapters have been written by sociologists, philosophers and scientists rather than by historians. As a result, the symmetry principle is usually discussed as part of larger debates about the promise of one or other sociological programme or about the viability of scientific realism. The principle is less often discussed as a tool for historical research, with the result that the second, fourth and fifth criteria in the previous paragraph are rarely taken into account. When historians invoke the symmetry principle, we tend to take it for granted, referring the reader to sociologists and philosophers for a more detailed defence and definition of the principle (e.g. Golinski 2005, p. x). Admittedly, there are overlaps between the historian's interest in the symmetry principle and that of the philosopher or sociologist. My debts to existing literature will be especially apparent in sections two and three below. However even in those sections I hope to give a historiographical twist to old debates.

2.2 Human Action Versus Types of Human Action

Does the symmetry principle state that all beliefs should be explained, at least partly, as the consequences of human action? Or does it state that all beliefs should be explained using the same range of human activities? Both versions can be found in the first detailed exposition of the symmetry principle, Barry Barnes' *Scientific Knowledge and Sociological Theory*. On the one hand, Barnes says that his target is the practice of "treating truth as unproblematic and falsehood as needing causal explanation" (Barnes 1974, p. 3). To treat true beliefs as "unproblematic" is to suppose that they "derive directly from awareness of reality" or that they "are the consequence of direct apprehension rather than effort and imagination" (p. 2). These statements suggest that Barnes is out to discredit sociologists who recognise no causal explanations of true beliefs, or who recognise only a trivial kind of causal explanation whereby states of affairs completely explain why people believe those states of affairs. Barnes is attacking the idea, for example, that the fact that the moon is mountainous is a sufficient explanation of Galileo's belief that the moon is mountainous.[1]

On the other hand, there are passages in which Barnes seems to say that true beliefs are routinely explained in causal terms, and moreover that these causes include human activities. Barnes devotes several pages to a survey of philosophers' accounts of "how beliefs actually can arise" through such causal processes as "sensory inputs, memory, induction and deduction" (p. 7). Barnes contrasts these causes with the ones usually invoked by sociologists to explain false beliefs, such as "inferior or impaired mentality, stupidity, prejudice, bigotry, hypocrisy, ideology, conditioning and brain-washing" (p. 2). On this showing, Barnes' complaint is not that sociologists have ignored the human activities that give rise to true beliefs. Instead it is that sociologists have explained true beliefs in terms of the former cluster of activities (sensing, deducting, and so on) rather than the latter cluster of activities (being stupid, prejudiced, and so on).

This ambiguity has not gone away in subsequent expositions of the principle. The peak of clarity came in David Bloor's 1976 account of the Strong Programme in the sociology of knowledge, where he distinguished between the principle that true and false beliefs both "require explanation" and the principle that true and false beliefs require explanation in terms of "the same types of cause." Bloor called the former the principle of "impartiality" and the latter the principle of "symmetry" (p. 7). This distinction did not last long, however. In (1981) Bloor referred to studies "in which both true and false beliefs are treated 'symmetrically,' i.e. as equally in need of explanation" (p. 392; cf. Barnes and Bloor 1982, p. 23). Harry Collins is a similar case.

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[1]Cf. Barnes (1972), esp. pp. 376, 378.

In several places he advises sociologists to assume that "the natural world in no way constrains what is believed to be" (Collins 1981a, p. 3, 1981b, p. 218, 1982, p. 140). This suggests that Collins' project is to introduce human activities into our explanations of the beliefs of scientists. In other places, however, Collins has associated the symmetry principle with the project of "showing the interpretative flexibility of experimental data." Here the targets of Collins' relativism do not appear to be historians who ignore human activities altogether, but rather those who concentrate on a particular kind of activity, namely carrying out experiments and inferring theories from the results of those experiments. According to Collins, these activities are not the "decisive" ones in the emergence of scientific consensus (Collins 1981a, pp. 3–4, 7, cf. 1987, p. 825). Even the critics of the symmetry principle have sometimes been guilty of equivocating between explanations that appeal to truth and those that appeal to human activities. For example, Jean Bricmont and Alan Sokal, in a recent paper attacking the symmetry principle, slide between two versions of the view they are attacking. Initially it is the view that the *truth* of a belief cannot explain the belief; later it is the view that the *evidence* in favour of a belief cannot explain the belief.[2]

What do historians make of all of this? Jan Golinski's *Making Natural Knowledge* is a good place to look for an answer, since Golinski is sympathetic to the Strong Programme but identifies himself as a historian rather than a sociologist (Golinski 2005, pp. x, xix–xx, 5). Golinski's overall historical approach, which he calls "constructivism," was "inaugurated by a determination to explain the formation of natural knowledge without engaging in assessment of its truth or validity." This attitude of epistemic neutrality is just what he calls the "symmetry postulate" (p. 7). His phrasing of that postulate does not reveal whether he is urging the use of human activities *tout court*, or rather a particular kind of human activity, to explain true beliefs. However his definition of constructivism suggests that he has the former in mind. The constructivist "regards science as a human product, made with locally situated cultural and material resources, rather than as simply the revelation of a pre-given order of nature" (pp. xvii, 6).

To save the symmetry principle we need to reinstate Bloor's 1976 distinction between explaining all beliefs with (human) causes and explaining them all with the same types of (human) cause. As I shall put it, we need to distinguish between the "causal" and "multicausal" readings of the symmetry principle. One reason for this is to do justice to internal history of science. Traditionally, internal history of science has concerned itself with what Barnes called "sense perception, memory, deduction and induction." One consequence of the equivocation that I have been describing is that internal historians of science are lumped together with those who believe that theories "derive directly from awareness of reality." The danger of this conflation is that the sins of the latter will be unfairly attributed to the former. Barnes, Bloor and Collins never explicitly make this attribution. However a reader of their works could be forgiven for thinking that internal historians of science are guilty of some kind of

[2]Compare Bricmont and Sokal (2001a, p. 40, 2001b, p. 245). The equivocation is partly resolved at Bricmont and Sokal (2001b, p. 246).

explanatory subterfuge, and that the only way to give genuinely *causal* accounts of past science is to become a social historian of science.

Distinguishing the causal and multicausal readings of the symmetry principle has the added advantage of enabling us to reject the former. This is necessary because the causal reading does not protect us against a genuine threat. Few historians of science, past or present, have tried to explain past theories without reference to human activities of one kind or another.[3] This generalisation may seem rash, but it becomes plausible as soon as we see what it amounts to. An example may help to illustrate the point. Consider William Whewell, the nineteenth-century polymath whose *History of the Inductive Sciences* is one of Golinski's examples of a pre-constructivist work. Consider, in particular, a randomly chosen passage in which Whewell explains Humphrey Davy's theory that chemical and electrical attractions have the same cause (Whewell 1837, vol. 3, pp. 154–162). By my count, Whewell refers to 18 separate human actions in the course of his 9-page explanation. These include such things as: Davy's acquisition of a battery of great power in 1801; Davy's conjecture that in all cases of chemical decomposition, the elements are related to each other as electrically positive and negative; William Wollaston's demonstration that the Voltaic pile is always accompanied by oxidation or other chemical changes, and his conclusion that the pile cannot be explained solely in terms of contact between different metals; and Davy's equivocations about exactly what he meant by his electro-chemical theory. Acquiring an object, making a conjecture, drawing an inference, equivocating—surely these are human activities in the same sense that pursuing a class interest or upbraiding a colleague are human activities. Histories of science have always referred to such activities. Indeed, it is hard to imagine how one could write history of science without such references.

Why then have twentieth-century authors so often claimed the contrary? One plausible answer is that the authors in question have confused the claim that scientific theories have no human causes with other, superficially similar claims. For example, Golinski points out, rightly, that Whewell believed that the natural sciences make steady progress over time, and that they do so using a single method that is common to them all (2005, pp. 3–5). These beliefs may be false, but they do not imply that Whewell believed scientific theories arise independently of human action. On the contrary: Whewell recognised at least one activity that scientists perform and that is causally responsible for their beliefs, namely the act of implementing their method. There is another confusion lurking in Golinski's claim that eighteenth- and nineteenth-century historians of science saw the mind as a "mirror of nature." Golinski names Priestley and Whewell as holders of this view. No doubt these men believed that truth consists in a correspondence between mind and nature, and that truth is something that scientists regularly attain. But both of these beliefs are compatible with the view that scientists need to *do* things—including complex, difficult and time-consuming things—in order to acquire true beliefs.

Another source of confusion is that philosophical disagreements do not always have serious historiographical consequences. I have in mind the disagreement

[3] Laudan (1981b, p. 178) makes the same point about philosophers of science.

between those who recognise a class of nonmaterial facts, namely the facts about which inferences are objectively correct, and those who think that the only facts about inferences are the psychological ones about people endorsing this or that inference. John Worrall has defended the former view, which Bloor firmly opposes (Worrall 1990, pp. 313–318; Bloor [1976] 1991, pp. 178–79). According to Worrall, nonmaterial facts not only exist but can be legitimately used by historians to explain some of the inferences that we observe in the historical record. As both Bloor and Worrall recognise, their disagreement is real and fundamental. But what difference does it make to the way they do history? A glance at Worrall's historical papers suggests that it makes little difference, at least not with regards to his willingness to explain the outcomes of scientific debates in terms of the spatio-temporal activities of the scientists involved. His papers are awash with scientists whose hands manipulate objects and whose brains organise data and draw inferences (e.g. Worrall 1976, 1990).

No doubt Bloor's account of the same episodes, if he were to write one, would be different from Worrall's. But the difference between the two accounts would probably not lie in the amount of human activity they describe. More plausibly, it would lie in the *kind* of human activities they describe and that they consider causally significant. Worrall would focus on "sensory inputs, memory, induction and deduction," to borrow Barnes' list, whereas Bloor would focus on social interests and conventions. For want of better terms, Worrall would focus on "rational" causes and Bloor on "social" causes. A symmetry principle based on a distinction such as this one—a distinction between two different types of cause—is more promising than a principle urging causal explanations of all beliefs. The latter principle is sound but unnecessary.

2.3 Social Versus Rational

But what types of causes should we focus on here? Is the distinction between social and rational causes the right one for the job? The fact that many authors fail to distinguish between the causal and the multicausal readings of the symmetry principle means that it is not easy to know how they answer this question. Nevertheless, the standard answer seems to be that the social/rational distinction is dispensable, if not illusory. As many people have pointed out, social causes and rational ones are not mutually exclusive. Social causes usually involve cognition of some kind—after all, a scientist has to identify his interests in order to act upon them, and this identification requires both reason and experience. Conversely, reason is a social phenomenon in the obvious sense that it is usually carried out by groups of individuals who interact with one another. Moreover, the way in which these groups are organised—in small teams rather than large ones, for example—can effect the methods they pursue and the theories they adopt.[4]

[4]These points are sometimes framed as a debate about the validity of the distinction between "internal" and "external" factors, e.g. Barnes (1974, Chap. 5), Shapin (1992).

These overlaps leave us with two choices.[5] Firstly, we could revert to the distinction between causes that are social and those that are not. These categories are, by definition, mutually exclusive; and we can safely assume that the latter category is not empty, since it is surely not the case that social causes are the only kind of cause at work in past science. Secondly, we could revert to the distinction between rational and non-rational causes. When they have expressed an opinion on the matter, sociologists have typically chosen the first option. That is, they usually frame the symmetry principle as the view that all beliefs should be explained in terms of "social causes," "socialisation," the "social dimension" of science, or the "socially negotiated character" of science.[6] In order to save the symmetry principle, I suggest, we need to reject the first option and adopt the second.

The reason for this is that only the rational/irrational distinction gives us a symmetry principle that protects us from a genuine threat. Critics of the sociology of science have rarely maintained that social factors, *as social factors*, cannot help to explain the formation of a true belief. Insofar as they have denied a role for social factors, they have done so not because they perceived those factors to be social but because they perceived them to be irrational. Admittedly, this is a claim about the background motives of the critics in question, and since those motives are often tacit they are not easy to analyse.[7] However we can do worse than consider the case of Larry Laudan, one of the staunchest and most persistent critics of the Strong Programme. Laudan once argued that a historian should only consider social factors as an explanation for a scientist's belief if she has been unable to find a rational explanation for the belief (Laudan 1978, pp. 201–10). On this showing, Laudan's view seems clear-cut: "sociology is only for deviants," as Newton-Smith put it (1981, p. 238).

If we read carefully, however, we find that Laudan has plenty of time for the sociology of rational beliefs:

> The flourishing of rational patterns of choice and belief depends inevitably upon the preexistence of certain social structures and social norms. (To take an extreme example, rational theory choice would be impossible in a society whose institutions effectively suppressed the open discussion of alternative theories.) … *we need further exploration into the kinds of social structures which make it possible for science to function rationally* (when it does so) (1978, pp. 209, 222, original emphasis).

Clearly Laudan is not opposed to social explanations per se. Instead he is opposed to a particular kind of social explanation, namely those that compromise the rationality of the beliefs that are so explained. Since Laudan wrote, at least four philosophers of science have echoed his call for more studies of the social dimension of rationality (Papineau 1988; Worrall 1990, p. 314; Bird 2000, p. 275; Lewens 2005, pp. 567–68).

Unlike the social/nonsocial distinction, the rational/irrational distinction gives real bite to the symmetry principle. If we plug the latter distinction into the standard

[5] Some would add a third option, which is to formulate the symmetry principle without reference to the "social", the "rational", or related concepts. Latour (1993, pp. 91–97) seems to take this option.

[6] E.g. Barnes (1974, p. 6), Bloor ([1976] 1991, p. 6), Collins (1981a, p. 4), Golinski (2005, p. xx).

[7] Of course, this caution also applies to the rival claim that the social rather than the rational has been the main bone of contention.

formula, we end up with a principle that true beliefs can be explained using irrational causes and false beliefs using rational ones, even when the true and false beliefs in question are rival beliefs. This principle has teeth because it cuts the link between truth and rationality that we all rely on when assessing beliefs. How do we decide whether climate change is man-made, whether there is life on distant planets, or whether it will rain tomorrow? We consider the evidence, weigh the arguments for and against, evaluate our sources, search for new sources, and perhaps assess the social structure of any relevant expert communities—in short, we exercise our rationality. We do all this because we think that the more diligently we do it, the more likely we are to make a correct assessment of the belief. In other words, we assume that rationality is a good guide to truth—no doubt a fallible guide, but the best guide we have, and better than no guide at all. Now, if rationality is a good guide to truth, then the reverse must be true: truth must be a good guide to rationality. Hence, when we study past science it is natural for us to assume that true beliefs have rational origins and that false beliefs have irrational ones—or at least that the overall balance of rationality lies with true beliefs. This tendency is so natural that it is worth having a principle to guard against it.

Several objections might be raised against this version of the symmetry principle. One is that it strays too far from the original purpose of the principle, which was to make room for sociological explanations of true beliefs. Admittedly, the social/nonsocial distinction does a better job of serving the purposes of Barnes and Bloor than the rational/irrational distinction. Only the former distinction gives us a principle that explicitly states that true beliefs can be explained sociologically. The term "social" does not appear in the principle I am advocating. Nevertheless, my principle certainly makes room for sociological explanations. Moreover, it makes room for precisely those sociological explanations that trouble philosophers, namely those that are irrational.

The term "rationality" may be a stumbling block for some readers. Have not historians and sociologists shown just how problematic this term is? In particular, have they not shown that rationality is context-dependent, in the sense that different problems or subject-matters call for different methods; that it is non-consensual, in the sense that different people endorse different methods when presented with the same problem or subject-matter; and perhaps even that it is relative, in the sense that no person's notion of rationality is objectively better than anyone else's? Let us suppose, for the sake of argument, that all of these claims about rationality are true. Even then, they do not cause problems for the Symmetry Principle. All that is required is that each historian, given a context and a set of belief-forming processes, is able to sort the processes into those that, in her judgement, are conducive to true beliefs in that context and those that are conducive to false beliefs in that context. It is not necessary that the historian would make the same judgement given a different context, or that her judgements are the same as any other historian's, or that they can be objectively ranked alongside those of other historians.

But isn't rationality—even rationality in the meagre sense I have just outlined—a normative notion? And is there any place for normative notions in the descriptive discipline of history? The answer to both questions is "yes." Rationality is a normative notion even if it is context-dependent and non-consensual; and even if it is relative, and even if the historian believes it to be relative, it may be sufficiently normative to raise her hackles as a historian. But there is a place for this normative notion in history, because it occupies a small and self-deprecating place when enshrined in the Symmetry Principle. That Principle does not require us to make any normative statements in our books and articles, or even in the course of our research. On the contrary: it enjoins us *not* to make normative judgements when we go about explaining past beliefs. The only reason the Symmetry Principle refers to a normative notion is to denigrate that notion as a guide to historical research. The notion of "rationality" in my principle is as innocuous as the notion of "rationality" in earlier versions of the principle, such as the one stated by Barry Barnes and David Bloor in 1982.[8]

Scientific realists might worry that my principle has *too much* bite. If truth is a poor guide to the rationality of past beliefs, as the Symmetry Principle maintains, why should rationality be a good guide to truth in the present? And if rationality is indeed a poor guide to truth in the present, then there is no reason to think that our best current scientific theories are anywhere near the truth. This conclusion is absurd, the realist might argue, so my principle must be abandoned. I agree that the conclusion is absurd, but not that it follows from the Symmetry Principle. My defense depends in part on the resolution of a third ambiguity that muddies much of the literature on the symmetry principle.

2.4 Restrictive Versus Permissive

The instruction "explain all beliefs in the same way" can be followed in two quite different ways. The historian can assume that all beliefs really can be explained in the same way. Or she can suspend judgement about how they can be explained until she has done enough historical research to make this judgement. In short, the historian can treat beliefs identically or impartially.[9] The difference between the two approaches—and it is a big difference—is that the former rules out the possibility that different beliefs are susceptible to different sorts of explanation, whereas the latter leaves this possibility open. For this reason I shall call the former the "restrictive" approach and the second the "permissive" approach.

Which of these approaches do we find in canonical statements of the symmetry principle? If we turn to the early manifestos of Barnes and Bloor, we find restrictive versions of the principle. For example, Bloor writes that a reformed sociology of knowledge "would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs" (Bloor [1976] 1991, p. 7). There is

[8]"Regardless of whether the sociologist evaluates a belief as true or rational, or as false and irrational, he must search for the causes of its credibility" (Barnes and Bloor 1982, p. 23).

[9]Here "impartially" means simply "without bias," and is unrelated to Bloor's principle of "impartiality" (on the latter see above, Sect. 2.2).

no room here for the possibility that true and false beliefs sometimes have different causes. The sociologist is assured that all beliefs have the same types of cause and is advised to seek them out. Bloor is just as strident in the 1982 paper he co-authored with Barnes (p. 23). Perhaps it would be unfair to rely too heavily on these slogans, however. To get a more nuanced view we might consider how Barnes and Bloor clarified their principle and how they and their followers have applied it to historical cases. Unfortunately, these two considerations point in opposite directions.

On the one hand, Barnes and Bloor both soften their initial statement of the principle. Barnes does so in a chapter on the role of "external" and "internal" factors in the history of science. Barnes glosses the former as "socio-economic" and the latter as "intellectual" or "technical." Barnes is refreshingly permissive about internalist historians, saying that he does not "take any a priori objection to their rejection of the significance of external or non-intellectual factors … We may proceed with an open mind to examine the [empirical] case against the externalists" (Barnes 1974, pp. 104–5). Bloor makes a similar concession. It is "surely correct," he writes, "that only some, and not all, episodes in the history of science are found to be crucially dependent on particular, social interests." The social component of knowledge is "always present" in science, but it is not necessarily "the trigger of any and every change" (Bloor [1976] 1991, pp. 166–67; cf. Ben-David 1981).

These statements are clear enough, but they are belied by the way that the symmetry principle is usually used to praise or blame a piece of historical work. When a study is praised as "symmetric," this is usually because it explains true and false beliefs in the same way. For example, Bloor praises J.B. Morrell for his "conspicuously symmetrical" account of two nineteenth-century chemical research schools led by Justus von Liebig and Thomas Thomson. Morrell sets out to explain why Liebig's school achieved international fame while Thomson's fell into obscurity. By "symmetrical" Bloor means that Morrell explains the plight of *both* schools in terms of the *same* set of factors—their interaction with the physical world in their laboratories, the personalities of Thomson and Liebig, their financial arrangements, and so on (Bloor [1976] 1991, pp. 34–36). Similarly, Barnes and Shapin congratulate Brian Wynn on his refusal to find "asymmetry" in the work of late-Victorian physicists at the University of Cambridge. By this they mean that Wynn considered both social and intellectual factors in his study, and that he found "no empirical basis for giving the one priority over the other" (Barnes and Shapin 1979, p. 95). Praise such as this gives the impression—intended or otherwise—that historians violate the symmetry principle whenever they give unequal weight to social and intellectual factors in their explanations of a belief.

Criticism sometimes conveys the same message as praise. For example, Shapin considers "profoundly asymmetrical" a paper by Charles C. Gillispie on Denis Diderot and other eighteenth-century thinkers who drew moral lessons from nature (Shapin 1980, p. 122). Some of Diderot's contemporaries, such as Voltaire, thought that nature bears no such lessons. It is clear from the paper that Gillispie sides with Voltaire on this matter. Shapin's complaint is that Gillispie explains Diderot's view as the product of a political ideology; when Gillispie explains Voltaire's view, by contrast, he appeals to the fact that Voltaire read and understood Newton's scientific works. Only those who adopt the restrictive view of the symmetry principle will find this a reasonable complaint. Those who adopt the permissive view will be open to the possibility that Voltaire was right for a good reason (consulting the opinion of an expert) whereas Diderot was wrong for a bad reason (adjusting his metaphysics to fit his politics). On the permissive view, Gillispie's account is asymmetric but need not be viciously so. On that view, what matters is the symmetry of the reasoning that led to his explanation, not the symmetry of the explanation itself.

It is hard to imagine how anyone could go about defending the restrictive view of the Symmetry Principle. Such a defence would require an a priori demonstration that, in every past scientific debate, the reasons on each side of the debate have been equally good. Perhaps an argument can be detected in the oft-repeated claim that science is "constitutively social" and that it is a "form of culture like any other." These phrases remind us that social phenomena are not optional additions to scientific life but indispensable components of it. It does not follow, however, that social and non-social factors are evenly distributed across true and false beliefs; and even if this did follow, it would not imply that rational and irrational factors are so distributed.

For the rest, the permissive reading sits well with two premises that most sociologists of science share with most historians of science. One is that empirical research is a more reliable source of data about past and present science than a priori speculation, at least if our aim is to describe rather than to evaluate the beliefs of scientists. The other premise is that historians cannot safely assume that the past resembles the present, or that a given period in the past resembles any other period in the past. These premises are hardly compatible with the restrictive reading of the symmetry principle, which rules out some phenomena a priori and treats a certain kind of symmetry as a historical constant.[10]

2.5 Equivalence Versus Exclusion

On the face of it, symmetry principles do not prohibit any explanations, whether social, non-social, rational, irrational, or whatever. They simply prohibit some ways of distributing these explanations across true and false beliefs. Nevertheless, prohibitions of the former kind are a recurring theme in literature on the symmetry principle. Indeed, symmetry principles have always made a double recommendation: all beliefs should be treated in the same way (equivalence), and certain treatments should not be applied to any beliefs (exclusion). The aim of this section is to untangle these two recommendations and to dissociate exclusions from the symmetry principle.

The exclusions in question are of two broad kinds. Some downplay the causal significance of certain human activities; others impose a total ban on certain explanatory resources that are not human activities, such as laws of nature and absolute standards of rationality. Harry Collins' writings illustrate both kinds of exclusion. As noted above, Collins downplays the role of experimentation, and especially the practice

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[10]Cf. Laudan (1981b, p. 191).

of replicating experiments, in the resolution of scientific disputes. Collins seems to think that replication makes *some* difference to the beliefs of scientists, but he insists that it is not "decisive" and that it explains less than do social circumstances such as the relations of trust between scientists. Collins engages in a different kind of exclusion when he rejects "truth, rationality, progress and success" as valid explanations for any scientific beliefs, even when they are supplemented with other sorts of explanation. Similarly, Bloor would like to excise all "teleological" explanations from science studies, and Barnes and Shapin rail against "lazy references to reality, to nature, or logic, or necessity" (Bloor [1976] 1991, pp. 11–12; Barnes and Shapin 1979, p. 10, cf. p. 187).

Some of these exclusions are, to put it mildly, controversial. Consider Collins' argument for the indecisiveness of replication. This argument is either purely empirical or partly philosophical. If the former, we should be wary of assuming that it applies outside the cases that Collins has so far considered, which amount to a tiny fraction of twentieth-century science. The argument may apply outside those cases, but this is not something that historians can assume in advance of inquiry, much less enshrine in a methodological principle. If Collins' argument is partly philosophical, it rests on a dubious interpretation of the Quine-Duhem thesis, according to which theories can always be legitimately rescued from counter-examples by suitable adjustments to other parts of the theorist's belief system.[11]

Truth is another controversial exclusion. It is not at all evident that facts about nature can play no legitimate role in explanations of scientists' beliefs. The standard arguments to the contrary are that nature is the same for all actors, so it cannot explain differences between actor's beliefs; and that truth cannot explain the outcome of scientific disputes because truth is precisely what is under dispute. Neither of these arguments is persuasive. As Nick Tosh has pointed out, similarities in conditions *can* explain differences in outcomes. To use one of Tosh's examples, the existence of the atmosphere helps to explain why a lead weight falls faster than a feather. As for the second argument, it conflates the *justification* that an actor offers for a belief with the historian's *explanation* of the actor's belief. Obviously, Galileo did not include the claim "the moon is mountainous" in his arguments for the thesis that the moon is mountainous. That would have been circular. However it does not follow that the mountains on the moon played no causal role in bringing about Galileo's belief that those mountains exist (Tosh 2006, pp. 684–92, 2007, pp. 187–91; private communication).

It is tempting to conclude from these debates that we should give up the symmetry principle as a bad job.[12] A better response is to recognise that the symmetry principle, properly understood, does not commit us to any controversial exclusions. We can fairly distribute our explanations without thereby denigrating entire classes of explanation. Why would anyone think otherwise? Collins gives an argument that is

[11] On the history of dubious interpretations of the Quine-Duhem thesis, see Zammito (2004, pp. 17–25, 148, 150, 159, 163, 173, 180).

[12] If I understand them right, this is the conclusion of Tosh (2006, 2007), Bricmont and Sokal (2001a, b).

plausible but flawed. For him, the symmetry principle states that all beliefs should be explained in the same way regardless of whether they are TRASP (Collins' acronym for "true, rational, 'successful, or progressive'). Collins posits that only beliefs that are TRASP can be explained in terms of the fact that they are TRASP. From this posit, it follows that historians are bound to violate the symmetry principle unless they omit TRASP-ness altogether from their explanations (Collins 1981b, p. 217).

Collins' posit may be partially conceded—but only partially—in the case of truth. The mountains on the moon can explain Galileo's belief that there are mountains on the moon. But the perfectly smooth surface of the moon cannot explain the belief, held by Galileo's contemporary Ludovico delle Colombe, that the moon has a perfectly smooth surface, because the moon does *not* have such a surface.[13] More generally: the truth of a true belief can explain why people hold the belief; but the truth of a *false* belief cannot explain why people hold that belief, for the simple reason that there is no such thing as the truth of a false belief.

There are two reasons why this concession to Collins is only partial. Firstly, it does not apply to the truth of all beliefs but only to the truth of whatever belief we are trying to explain. The smoothness of the moon cannot explain the belief that the moon is smooth; but *other* facts about nature, ones recognised by modern science, can plausibly explain this belief. For example, Colombe's error about the moon's surface was partly due to his conviction that the moon is more perfect than terrestrial bodies, a belief due in part to the fact that the moon revolves around the earth roughly once every twenty-seven days. The second caveat is that the truths that can explain false beliefs include the truths of *rival* beliefs. Consider another of Colombe's false beliefs, namely that the blotches on the moon's surface, as seen through Galileo's telescope, are due to the uneven density of the moon's interior. The observed blotches were due precisely to the phenomenon—the mountains on the moon—that Colombe set out to refute with his density theory. In general, there are no truths that can explain a given true belief but cannot, at least in principle, explain a rival false belief. It follows that, although appeals to truth lead to asymmetry, this is an asymmetry of a particularly mild kind. The difference between true and false beliefs does not lie in the number or kind of truths that can explain them, since the same set of truths are candidates as causes of both true and false beliefs. The difference lies rather in the *relation* between the beliefs explained and the truths that explain them. True beliefs can enjoy a relationship with the truths that explain them that is unavailable to false beliefs. The relationship is that of correspondence: true beliefs can be explained by *their own* truth, whereas false beliefs cannot be explained by their own truth.

In the case of rationality there is no asymmetry, not even of the relational kind that applies to truth. Asymmetry arises for truth because truths are tenseless: if "the moon was mountainous in the seventeenth century" is a true proposition in 2014, that means that it was also a true proposition in 114 and 1614, and that it will still be a true proposition in 10014. The same cannot be said about rationality. In 2014 it is rational to believe the statement "the moon was mountainous in the seventeenth century."

[13]On Colombe's view on the moon, see e.g. Heilbron (2010, pp. 172–73).

But it is conceivable, and is arguably the case, that this belief was *not* rational in 114 or even in 1514. Beliefs that are rational today may have been irrational in the past, for the simple reason that the available evidence has changed over time. As a result, explanations of a belief that appeal to the rationality of the belief are not doomed to asymmetry. Such explanations can be applied *both* to beliefs that are rational today *and* to beliefs that are irrational today. The symmetry principle gives us no grounds for writing off rationality as an explanatory resource.

This conclusion assumes the time-dependence of rationality, but only a very mild kind of time-dependence. No paradigm-shifts are required, no dramatic changes in the meanings of the terms of theories or the criteria used to assess those theories. All that is required is that reasonable people can change their minds. An everyday example will suffice to illustrate the kind of process I have in mind. On Saturday evening I consult a competent weather report, one that has served me well in the past, and I learn that there will be no rain in my region on Sunday morning. When I wake up on Sunday morning, I hear a constant drumming on the roof and observe through a wet window that there are large puddles on my driveway. On Saturday evening I believed, rationally, that it would not rain on Sunday morning. On Sunday morning I believed, rationally, that my earlier belief had been mistaken. My metaphysics and epistemology did not change overnight. All that changed was the state of the evidence, and my beliefs about Sunday's rain changed accordingly. This is probably not the only sense in which rationality has changed in the course of past science. But we do not need to agree on any of the more controversial kinds of time-dependent rationality in order to agree that explanations that appeal to rationality are not inherently asymmetric.

Let us consider truth again. Given that some appeals to truth are inherently asymmetric, albeit mildly so, should we ban the asymmetric ones from historical explanations? No. Firstly, there is a perfectly good symmetry principle that makes no reference to truth-based explanations. According to the Symmetry Principle, the truth-value of a belief is a poor guide to its rationality. This principle requires that we explain some false beliefs in terms of their rationality. But it does not require that we explain some false beliefs in terms of their *truth*, and that in the absence of such cases we explain *no* beliefs in terms of their truth. Secondly, and most importantly, we gain nothing of value from the latter requirement that we do not already get from the former. What, after all, is the point of the latter requirement, in the eyes of Barnes and Bloor and Collins and Shapin? Their aim is partly to prevent historians from explaining true beliefs solely in terms of their truth. But very few historians of science have done this; not even Whewell did this, as I argued in the first section of this paper. Their other aim is to promote epistemic openmindedness. This means checking our tendency to explain true beliefs in terms of reason and sense experience and false beliefs in terms of sloppiness and self-interest. But the Symmetry Principle already achieves this goal. There is no need to add an extra clause banning appeals to truth in historical explanation.

2.6 The Symmetry Principle and Scientific Realism

The observation that the rationality of a belief can change over time helps to free the symmetry principle from controversial exclusions. However the same observation appears to lead to the equally controversial conclusion that we have no reason to believe the claims of present-day scientists. To recapitulate the argument at the end of Sect. 2.3: as per the Symmetry Principle, truth-value is a poor guide to the rationality of past beliefs; therefore rationality is a poor guide to the truth-value of past beliefs; therefore rationality is a poor guide to the truth-value of *present* beliefs; therefore there are *no* good guides to the truth-value of present beliefs. This conclusion will be a *reductio ad absurdum* of the Symmetry Principle for anyone—and this is surely just about everyone—who thinks that there are good reasons to believe many of the things that present-day scientists tell us about nature. Most of us believe that the moon *is* mountainous, and moreover that this belief is justified. The Symmetry Principle is in trouble if it implies otherwise.

This worry should not be confused with two other worries that philosophers have entertained about the symmetry principle. David Papineau once wondered whether the sociology of science "discredits science." He concluded that it does not, but his reason for thinking that it *might* was that the sociology of science—and in particular the symmetry principle—seems to tell us that scientists are not rational and hence that their conclusions cannot be trusted (Papineau 1988). By contrast, my worry is that the Symmetry Principle tells us that scientists cannot be trusted even when they *are* rational. This worry should also be distinguished from Larry Laudan's thesis that the predictive and explanatory success of present-day theories is a poor guide to their truth. Laudan built his case on the historical observation that many theories that were successful in the past are now considered false, and indeed have been superceded by theories that were, at some periods in the past, less successful than they. Laudan argued that today's successful theories will suffer the same fate as their forebears (Laudan 1981a, cf. 1981b, p. 186).

This "pessimistic induction," as it is now known, is similar to the argument I outlined two paragraphs ago. The difference is partly one of scope. Laudan intended his argument to apply only to scientists' beliefs about unobservable entities, such as electrons and ethers; and only to one criterion for assessing those beliefs, namely their predictive and explanatory success. My argument is wider. Indeed it is as wide as the Symmetry Principle, which applies to all the beliefs of scientists and to all rational criteria that they use to assess those beliefs. The two arguments also differ in their conclusions. Laudan concluded that we have no reason to believe today's theories about unobservable entities. My argument assumes that such doubt is implausible if broadened to include all the beliefs of today's scientists, and concludes that the premise—the Symmetry Principle—must be at fault. I shall call this argument the "pessimistic reduction," a name that signifies its similarity to the pessimistic induction and its aim of reducing the Symmetry Principle to absurdity. The aim of the rest of this section is to defend the Symmetry Principle against the pessimistic reduction.

One option is to take a leaf from Collins' book and insist that the Symmetry Principle is merely a heuristic device and hence that it says nothing about what the world contains or about how much we know about its contents. In other words, it is methodologically sound but epistemically and metaphysically innocent. It asks not that historians become anti-realists, but merely that they suspend their realism when examining past science. This is now the orthodox interpretation of the symmetry principle, endorsed by Shapin, Bloor and Golinski as well as Collins.[14] Unfortunately, methodological advice cannot be so easily separated from substantive claims about the past, and such claims can have epistemic consequences.[15] The way to disarm the pessimistic reduction is not to deny that the Symmetry Principle has any consequences for epistemology but to formulate the Principle in such way as to limit the damage that those consequences do to our epistemic intuitions.

Plausibly, one way to soften the consequences of the Symmetry Principle is to place a constraint on the kinds of theories to which it applies. The most obvious option would be to restrict the Principle to claims about unobservable entities. This would license historians to use truth-value as a guide to their explanations of a great number of past beliefs: claims about particular objects and events, from the forms of plants to the size of earthquakes; empirical laws, such as the sine law of refraction and the value of the mechanical equivalent of heat; and, depending on how one defines "unobservable entity," claims about distant galaxies and geological events that occurred in the remote past. Much of the history of physics and chemistry would become immune to the Principle, and historians of anatomy and physiology would be almost entirely beyond its grasp.

The problem is that, despite these massive concessions from history to philosophy, the Symmetry Principle would still be controversial. After all, scientific realism is still a respectable thesis among philosophers of science. Many philosophers believe that there are grounds for confidence not just in particular observations and empirical laws but also in unobservable entities such as proteins, quarks and oxygen molecules. These philosophers would likely reject any version of the Symmetry Principle that casts doubt on scientist's claims about unobservable entities, even if it leaves the rest of science in tact. Granted, scientific realism may be false. However, historians should not adopt methodological principles that pre-empt this conclusion. In doing so they would alienate a large portion of their potential readership, forfeit the right to use their research as an independent test of scientific realism, and incur the obligation of revising all their publications should scientific realism turn out to be true. In short, limiting the Principle to unobservable entities is not going to please anyone. It is too great a constraint for the average historian, and too small a constraint for the average philosopher.

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[14]This interpretation is so orthodox that some writers now say that is has always been orthodox (Shapin 1999, p. 4; Golinski 2005, p. 8). This claim is hard to reconcile with passages in the early writings of Barnes (1974, p. 154), Barnes and Bloor (1982, p. 27), and Collins and Cox (1976).

[15]In this I agree with Bricmont and Sokal (2001a, pp. 38–43), though my reasons are not identical to theirs.

A more promising approach is to observe that only the restrictive version of the Symmetry Principle is prey to the pessimistic reduction.[16] To see this, consider the historical theses that we commit ourselves to if we adopt these two versions of the Principle. The permissive version commits us to a view about the state of our knowledge about the past. It implies that we currently have no reason to think that the balance of rationality has always lain with true beliefs. If we did have such a reason, then the permissive Principle would be unnecessary, since the inference that Principle disallows would be a legitimate inference. In such a world, the permissive Principle would not, it must be said, lead historians into error. But it would lead them to truth more slowly than necessary, since it would force them to do time-consuming empirical research to answer a question—where did the balance of rationality lie in past debates?—that they could have answered from their armchairs. When we endorse the permissive Principle we commit ourselves to the belief that we are not in such a world.

What are the epistemological consequence of this commitment? Merely that infallible scientific realism is false, i.e. that we cannot be absolutely certain that scientists' current beliefs about nature are true. This is a perfectly respectable consequence that most scientific realists would happily accept. Most realists, and perhaps all of today's realists, are fallibilists. They concede that there is a possibility that any given present-day theory is false. They simply insist that the probability of this being the case is rather small. This is consistent with the belief that there are many historical debates in which the balance of rationality did not lie with the true theory.

It might be objected that fallible scientific realism is *not* consistent with the view the balance of rationality lay with the false theory in *most* historical debates. Indeed, fallible realism seems to require that rationality lay with the true theory in the *vast majority* of historical debates. Fortunately, this realist thesis does not render the permissive Principle unnecessary. Given any historical case, the realist historian can be fairly confident that rationality lies with the true theory. However he can be *much more* confident in this judgement if he examines the historical record. And if his empirical research shows that this was one of the rare cases in which rationality lay with the false theory, his data trumps his a priori expectation. The realist historian who examines a past debate is like a policeman in a quiet neighbourhood who examines a driver for excessive drinking. The policeman believes on good evidence that most drivers in the neighbourhood are sober, and hence that this particular driver is unlikely to be drunk. Nevertheless he checks the driver's breath, as thoroughly as he can, because he knows that, in individual cases, checking is better than guessing.

This response to the pessimistic reduction is not available to those who endorse the restrictive version of the Symmetry Principle. The realist requires that rationality lay with the true belief in the vast majority of historical debates. The restrictive Principle flatly contradicts this requirement. It states that in every historical debate worthy of

[16]There are other promising approaches. One is to distinguish between live scientific debates, where many relevant experts disagree on a question, and settled debates, in which there is a wide though not universal consensus on the question. Here I focus on the restrictive/pessimistic distinction in order to show the importance of that distinction.

the name, the side that turned out to be wrong had as many good reasons for their position as the side that turned out to be right. There is no question of adopting the restrictive Principle merely as a methodological tenet. If scientific realism is true, then historians who follow the restrictive Principle are in great danger of giving false accounts of past science. The permissive Principle runs no such risk.

2.7 Conclusion

One version of the symmetry principle is that there are a great variety of beliefs and that all of them are equal. The moral of this chapter is that there are a great variety of symmetry principles and that they are *not* all equal. To save the symmetry principle we need to distinguish it from the many dubious maxims with which it has become entangled over the course of its long career. To begin with, the principle should not urge the inclusion of human activities in explanations of the beliefs of scientists. This is not because human activities have no explanatory role, but because very few historians of science, past or present, have maintained such a perverse doctrine. Similarly, the principle should distinguish between rational and irrational beliefs rather than social and nonsocial ones, since very few historians or philosophers have denied that social factors can help to explain true beliefs. The principle should be permissive rather than restrictive, at least until someone demonstrates that all scientific battles without exception were fought with equally powerful arguments on both sides. In the meantime, the permissive view is in tune with the empiricism and historicism espoused by many sociologists and historians. The principle should instruct us to distribute our explanations in an impartial manner, but it should not exclude any types of explanation from historical practice. Some such exclusions may be justified, but it is not the principle that justifies them, and in particular the principle does not exclude appeals to truth in historical explanations. Finally, the principle should not force us to take sides on controversial philosophical questions such as the truth or otherwise of scientific realism. The principle that results from these choices is what I have been calling the Symmetry Principle. It states that historians should not assume in advance of empirical inquiry that true beliefs are best explained rationally and that false beliefs are best explained irrationally. Stated briefly—perhaps too briefly to avoid further confusion—historians should not use truth as a guide to rationality.

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