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Tilman Sauer
Raphael Scholl *Editors*

The Philosophy of Historical Case Studies

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The Philosophy of Historical Case Studies

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Chapter 1
Introduction

Tilman Sauer and Raphael Scholl

1.1 The Philosophy of Historical Case Studies

In her novel *Five Little Pigs*, Agatha Christie presents five different versions of the same murder. It falls to Hercule Poirot to sort out the conflicting accounts—to use "se little grey cells" in order to infer from the available facts what really happened. The famous Belgian detective thus finds himself in a similar predicament as historians and philosophers of science when they need to assess divergent reconstructions of the same historical episode. Whether explicitly or not, such reconstructions are always informed by philosophical positions about the character of science: Many episodes have been told and retold in different and often incompatible versions, none of which are manifestly correct or incorrect.

Although underdetermination is entertaining in mystery stories, it is a vexing challenge for history and philosophy of science. To illustrate, take one of our motivating instances: Semmelweis's work on the cause of childbed fever in the middle of the 19th century, long a textbook favorite not only in philosophy of science, but also in clinical research. It matters for our ongoing debates about scientific methodology whether Semmelweis proceeded by the hypothetico-deductive method, by inference to the best explanation, by experimental causal inference, or by flawed reasoning. All of these accounts have been defended in the literature, and it is surprisingly difficult to determine which of them offers the best balance between descriptive adequacy and philosophical insight.

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Similar problems present themselves in the reconstruction of many episodes in the history of science. Take as a second instance the well-known disagreement about the early history of gravity wave research between Allan Franklin and Harry Collins (discussed in this volume in Chaps. 6 and 7). Here the divergence is even more pronounced, and its implications reach even further. Franklin's account of an emerging rational assessment contrasts sharply with Collins's view of the social construction of consensus. Yet both authors are conceptually sophisticated and engage deeply with the relevant historical sources. Must we accept a stalemate, or can we develop philosophical tools and deepen our historical understanding until one of the two accounts emerges as clearly and justifiably more accurate?

The disagreements between Franklin and Collins about gravity wave research are typical for studies at the intersection of history and philosophy of science. The goal in such projects is not only to use philosophical concepts in order to make an historical episode intelligible, but also to use the historical episode in order to improve our understanding of these very same concepts. Thus, concepts from the theory of experiment, such as calibration and measurement, are crucial to how the history of gravity wave research is understood. More broadly, the interpretation of the case at once hinges on and speaks to the nature of scientific rationality, especially the role of scientific epistemology in consensus formation. It is unsurprising that authors with different views of these issues will initially interpret the historical sources differently.

However, we believe that the underdetermination problem of integrated history and philosophy of science is far from intractable. It is an invitation to think about methodology: to reflect carefully about how to relate philosophical concepts and historical cases to each other. When different philosophical concepts lead to different narratives of the same historical episode, can a close study of the empirical facts of history decide between the competing philosophical viewpoints? Is it possible to develop a new or refined philosophical account of experiment—or calibration, or measurement, or rationality—on the basis of the historical sources? Conversely, is philosophical analysis sometimes capable of adjudicating between competing historical narratives? In other words, can it be legitimate to prefer one historical reconstruction over another on philosophical grounds?

Renewed awareness of such methodological questions is relevant to many current areas of research. Case studies from contemporary and past science play a prominent role not only among historians and historically inclined philosophers of science, but also among the many current philosophers of science who engage in detail with scientific practice. Cases are routinely used to explore, illustrate, question, or test philosophical and historical points of view—they can even become the linchpins of controversies. It is thus worthwhile to engage in explicit discussion about how we can put cases to appropriate use in the study of science.

Philosophical presupposition about the essence and character of science are inextricably woven into each and every historical narrative. In some instances, these underlying assumptions are subtle. There are works where they exert their power quietly in stabilizing a traditional genre of historiography. Biographical studies are an example, or editorial projects. The very project of writing a scientist's biography, or of editing a corpus of source texts, is based on assumptions of the dynamics of science and of the quality of scientific work, even if the prefaces and introductions will not make their assumptions explicit in all cases. The different ways that biographies are written or editorial projects are organized continues to be determined by philosophical assumptions.

But there are also many instances where philosophical concepts stand out prominently and are exposed to critical evaluation, or where a historical narrative is questioned on conceptual grounds. Consider historiographical and philosophical categories with broad epistemological import that are directly applicable to the analysis of cases. Among those we find meta-level concepts like discovery, observation and observability, the reality of theoretical concepts, representation and modeling, and scientific methodology. When these meta-categories are in play, any discussion soon requires a jointly historical and philosophical analysis that is sensitive to the ways in which history and philosophy of science can and cannot be related to each other, and especially to the inferences that case studies do and do not permit. In short, a philosophy of case studies is needed.

In November 2013, we brought together a group of researchers to debate the relationship between philosophy and history of science at the University of Bern, at a workshop titled "The philosophy of historical case studies". The present volume derives from that workshop and makes the points of view, arguments, and cases developed by the contributors available to a wider audience. In addition, it presents a number of further papers that were invited after the workshop.

All the essays in this volume reflect explicitly on the relation between philosophical concepts and the way we write our historical accounts. Most of them proceed from one or more examples of actual underdetermination, that is, from an instance where different historical narratives were in fact determined by different philosophical projects. The essays all contribute in some way to one of two broad goals: to improve our understanding of the methodological challenges of case studies, and to develop a framework for meeting these challenges.

We are far from anything like a canonical understanding of the underdetermination problem of integrated history and philosophy of science, not to mention a recipe-like methodology to meet its challenges. Nevertheless, we believe that the collection of essays in this volume illuminates in a particularly transparent way key aspects of the interplay between history of science and philosophy of science.

1.2 Overview

The present volume is divided into three parts. The first part is concerned with theory: Its contributions try to describe and advance the current state of the art in relating history and philosophy of science to each other. The second part revisits controversies: Its contributions take up cases where different philosophical approaches have produced conflicting histories, or where historical studies have failed to settle philosophical disagreements. Finally, the third part strives for application: It presents contributions that use case studies both to investigate historical questions and to expand philosophical concepts.

1.2.1 The Relations Between History of Science and Philosophy of Science

Michael Bycroft's contribution focuses on the historiographical and interpretative maxim of the symmetry principle. As he points out, the symmetry principle is central to the methodology of much of today's science studies. But clear statements of its meaning are difficult to find. The standard formulation according to which true and false beliefs should be explained in the same way is deceptively simple and ambiguous. Bycroft proposes and defends a version of the symmetry principle according to which historical investigation should not assume from the beginning that true beliefs are best explained rationally and false beliefs are best explained irrationally.

Giora Hon offers a distinction relevant for the historical reconstruction of concept formation. He proposes to distinguish between two kinds of knowledge of the scientific community about a certain problem at a certain time: shared "baselines" on the one hand, and personal "snapshots" on the other hand. The baseline knowledge is the common ground for every attempt at a given time to interpret experimental data or problem settings. But each author takes a different view of the shared knowledge. Individual "snapshots" are limited and selective and, more importantly, every author emphasizes different aspects of a problem over others. His paradigm example to illustrate the distinction and to demonstrate its significance is the famous opening sentence of Einstein's 1905 paper on the "Electrodynamics of Moving Bodies." Here Einstein confronts a general feature of Maxwell's electrodynamics with a peculiar feature that he takes as motivation and point of departure for his analysis of the foundations of kinematics. Hon exemplifies his distinction by discussing three different authors, Einstein, Lorentz, and Poincaré, with respect to the relevance of Kaufmann's cathode ray experiments of the velocity dependence of the electron mass for the evaluation of their different theories of electron dynamics. He therefore goes beyond suggesting a historiographical category. He contends that the actors themselves perceived their differences in terms of the "baseline" knowledge and their individual "snapshots" of it.

Wolfgang Pietsch reminds us that the issue of case study methodology is by no means restricted to the history and philosophy of science. Rather it is a much discussed topic in the social and medical sciences, and Pietsch draws on some of that literature to introduce distinctions relevant for history and philosophy of science. He distinguishes between a predictive and a conceptual mode of reasoning with case studies. The predictive mode, which is prominent in the medical sciences (Pietsch's example is AIDS), is less relevant for the history of science. Here, the conceptual mode of reasoning is more often found. It is related to the problem of analogical reasoning, and the generic problem of both is how to justify case-based generalizations.

Finally, Raphael Scholl and Tim Räz address both the foundational issue of why a combination of history and philosophy may result in any non-trivial insights at all, as well as the more difficult question of an adequate methodology for an integrated history and philosophy of science. They propose a typology of case studies for the purposes of integrated history and philosophy of science. Their classification, which is not intended as exhaustive, includes hard cases, paradigmatic cases, big cases, and randomized cases. Scholl and Räz further discuss the confrontation of philosophical concepts and historical cases, illustrating by example how to handle agreements and disagreements between historical cases and philosophical concepts, and how to adjust philosophical categories in response.

1.2.2 Controversies Reconsidered

The very existence of the essay by Allan Franklin and Harry Collins is a statement. Both authors have engaged in extensive—and influential—historical and sociological analysis of scientific enquiry. They have done so from different viewpoints and philosophical assessments of the essence and character of science, and they happen to have looked at the same historical episode: the first phase of gravitational wave research with a bar detector by Joseph Weber in the sixties and seventies. Not surprisingly, their assessments of why Weber's research program came to be rejected by the community differ strongly. According to Collins, sociological explanations, the analysis of interaction between the scientists and their mutual perceptions play a major role. Franklin, on the other hand, argued that Weber's results were rejected as a result of rational discourse along well justified methodological principles. Famously, their different assessments led to some acidic mutual polemic. Both authors have continued to defend their claims against their strongest critics—themselves—and have found the resistance they each met to be sincere and justified. As a result they have come to accept some of each other's criticisms. In the contribution to this volume, they lay out, for the first time, an agreement about their different points of view. And not only do they agree about what they do not agree upon. They reach a new agreement by highlighting insights that only emerged in the process of their extended debate.

Katharina Kinzel also takes as an example the case study of the early gravity wave experiments. She uses this case study, along with different accounts of the demise of the phlogiston theory, as examples to reflect on the possibility and restrictions of pluralism in historiography. She looks at the various evaluation criteria for differing historical accounts and finds that they are either basic and generic, or complex and specific to the case at hand. Her account is informed by concepts of literary theory, and she proposes to look at different historical accounts as structured by different narrative templates. Applying evaluation criteria based on selectivity of sources, theory-ladenness and narrativity, she evaluates on the one hand the different accounts of the early gravity wave research by Franklin and Collins, and on the other hand the accounts by Musgrave and Chang of the chemical revolution.

Tarja Knuuttila and Andrea Loettgers compare three different accounts of the emergence and early history of the Lotka-Volterra model for population dynamics. All three accounts focus on modeling and on Vito Volterra's work, and all three case studies deliver their points by contrasting Volterra's work with that of other scientists. Michael Weisberg discussed Volterra's work as an example of a special kind of theorizing, which he calls modeling as opposed to abstract direct representation. Weisberg contrasts Volterra's work with that of Darwin on coral reef formation to illustrate his point. Scholl and Räz also compare Volterra's work and Darwin's, but they see both as modelers and distinguish them from scientists using more "direct" approaches such as methods of causal inference. According to them, a key difference between the two modelers is that Darwin proceeded much farther on the path from a 'how possibly' to a 'how actually' model than Volterra. Knuuttila and Loettgers challenge both accounts. They point out that the difference between Weisberg and Scholl and Räz can in part be explained by the fact that the commentators focus on Volterra's works from different periods. In their own work, Knuuttila and Loettgers take an even larger view of the development of Volterra's thinking, and they contrast it with the work of Alfred Lotka.

Tim Räz reflects on a case of disagreement that is both highly specialized and, at the same time, raises methodological questions of broad significance. His topic is a disagreement among five Einstein scholars who put a great deal of joint research effort into the analysis and reconstruction of Einstein's so-called Zurich notebook. It was written between summer 1912 and spring 1913 and documents Einstein's and his friends Marcel Grossmann's search for a generally covariant field equation of gravitation. The notes document a learning curve from the very first acquaintance with elements of tensor calculus to rather sophisticated calculations of properties of tentative field equations. Along the way, Einstein and Grossmann famously wrote down the correct equations already—if only in linear approximation—only to discard them again. In the line-by-line reconstruction of Einstein's notes the five scholars agree on almost all details but nevertheless differ significantly in their assessment of what it was that induced Einstein to discard the right field equations. The disagreement crystallizes in a distinction between what they call coordinate conditions and coordinate restrictions, the former concept indicating a modern understanding, the latter concept identified in their reconstruction of the notes. The disagreement concerns the question of whether Einstein at the time of doing the calculations documented in the notebook was already aware of the concept of coordinate conditions. After laying out the problem, Räz analyzes the different positions and probes possible ways of furthering the debate and resolving the disagreement.

1.2.3 Integration in Practice

Theodore Arabatzis and Kostas Gavroglu analyze the relationship between history and philosophy of science by questioning the concept of discovery as a simple historiographic category. In a naive understanding, discoveries are localizable in space and time and one can also identify the object of discovery and the subject, the discoverer. One can ask the four "w"-questions—the what, who, where, and when—and expect unambiguous answers for any genuine discovery. But as Arabatzis and Gavroglu show, in actual historical cases, we need to be prepared that neither of those question can be answered unambiguously. Discoveries, they maintain, are extended historical processes. Their example is the discovery of the inert gas argon by Lord Rayleigh and William Ramsay in the late 1890s. Closer historical analysis of the discovery reveals that the major difficulty in accepting the experimental data as indicating a new chemical element rather than as a discrepancy, was a necessary revision of the concept of chemical element itself. Prior to the discovery of Argon, the concept of “element” implied that substances that were identified as elements would be reacting with each other. But argon was chemically inert. The process of turning a discrepancy into a discovery involved the revision of the general concept of chemical element, in order to accommodate the chemical properties of argon as properties of a new element. Their case study demonstrates a philosophical point about the nature of scientific progress. Analyzing the discovery of argon with a skeptical stance toward the received meaning of the philosophical category of “discovery” reveals the inner workings of such a process and gives clues as to how the category should be used in a descriptively adequate way. The authors claim that the confrontational model of integrating history and philosophy of science should be replaced by a model where the historiographical categories should be judged by the historical narratives that they enable.

Simone de Angelis studies the interplay of *senso* (sense perception) and *discorso* (reasoning) in Galileo’s observation of lunar mountains between 1609–1611. He extends previous discussions that either considered the finding of mountains as a straightforward observational fact or focused only on Galileo’s geometrical, model-based reasoning. De Angelis argues that historical episodes should be understood as integrating a much greater range of historical and conceptual material. To get a full understanding of the episode of the lunar mountains, we must consider not only the different types of texts and forms of representation that Galileo produced, but also the critical context in which particular arguments and models were presented and received. Further, we need to take account of the various epistemic strategies that Galileo employed, including instruments, observations, theories, models, arguments and one experiment. Only if we integrate these many aspects of the episode can we understand the epistemic situation in which Galileo worked, and how he was able to conclude that the moon is mountainous.

Tilman Sauer considers the Stern-Gerlach experiment of 1922, focusing on the different perspectives that have been adopted with regard to the experiment and its outcome. In the experiment, individual silver atoms were sent through an inhomogeneous magnetic field and their deflection was observed. The historical actors considered this an *experimentum crucis*: while classical physics predicted a broadening of the beam of silver atoms, Bohr’s quantum theory predicted a splitting of the beam into two components. The experiment spoke for Bohr’s theory by demonstrating discrete deflection of silver atoms. However, the Stern-Gerlach experiment is now seen in a different light. First, it is seen as a confirmation of angular momentum projection quantization of the silver atom’s electron spin. Second, it is seen as a demonstration of the dynamical collapse of the wave function in a quantum measurement, an interpretation which Sauer traces through Einstein, Ehrenfest and Bohm. Sauer argues that an adequate understanding of the Stern-Gerlach experiment requires us not only to take account of the various perspectives on the experiment that have been adopted, but also on how one perspective gave way to the other over time.

Dana Tulodziecki examines the transition from zymotic views of disease to germ views in the 19th century. In the debate about scientific realism, the case is seen as an instance supporting the pessimistic meta-induction: even though the zymotic theory was predictively successful, it was eventually abandoned in favor of the more successful germ theory. Faced with this data, anti-realists would argue that the radical discontinuity shows success to be no indication of truth. Realists would counter that the discontinuity is only apparent: those elements of the zymotic view which were responsible for the theory's success were, in fact, retained in the germ theory. Thus, predictive success remains an argument for truth in selective realism. Tulodziecki examines the historical sources closely and concludes that neither the anti-realist emphasis on radical discontinuity nor the realist emphasis on continuity of key elements allows an adequate understanding of the transition from zymes to germs. There was never a discrete choice between two theories, each of which had arguments in favor and against. Instead, there was a gradual evolution from one theory to the other, with elements being replaced step by step. The historical sources thus show the debate about scientific realism to be based on false assumptions. A reframed question emerges from this discussion: How can we adequately describe and explain the actual, gradual transition from the zymotic to the germ view of disease, and how does this actual transition relate to the question of scientific realism?

Adrian Wüthrich takes issue with the concept of unobservability and the alleged role that the maxim of eliminating unobservables played for Heisenberg's *Umdeutung* of kinematic and mechanical relations in the foundation of matrix mechanics. Wüthrich doubts that observability is a sharp concept and is sceptical as to whether a clear distinction between observable and unobservable can be upheld. In the abstract to his seminal 1925 paper, Heisenberg gives prominence to the maxim of eliminating from the theory unobservable quantities like electron orbits. The actual relevance of this methodological maxim has been questioned, and Wüthrich agrees with Mara Beller's criticism of Heisenberg's claim. In his paper, however, he takes Beller's critical attitude toward the rhetoric strategy of Heisenberg as a challenge to interpret Heisenberg's self-proclaimed method in a way that gives it a positive turn. Rather than assuming that no explicit methodology was at play, he argues that the actual methodological strategy that the actors were using was to ask for minimal causal explanations in the sense that a theory should only posit such entities as are required for an adequate explanation.

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Part I
The Relations Between History of Science and Philosophy of Science

Chapter 2
How to Save the Symmetry Principle

Michael Bycroft

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

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[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

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