Chapter 13

Vindicating Science—By Bringing It Down

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Abstract

Science, in the classical view, is the epitome of a rational endeavor, untrammeled by social and cultural influences. It strives to reflect the way the world really is, and is elevated above our petty human lives. Social explanations come into view only when science goes astray—when it stops being science. In recent decades, radical sociologists and other science bashers have tried to wrestle away science from the upholders of the classical view, bringing it down to the level of other human endeavors. Science, they maintain, is social to the bone, and what passes for scientific knowledge is nothing but a fabric of social constructions and power relations. In turn, this radicalism has fueled suspicions among science advocates about any naturalized conception of science: the edifice of science should be free from the contamination of social influences. Both parties in the dispute, as we argue in this chapter, buy into an intuitive view that characterizes much of our everyday reasoning about the causes of belief: a stark opposition between the rational and the social. Wherever social influences hold sway, reason takes the hindmost. And wherever reason reigns, no need is felt for social explanations. This opposition harks back to an even more basic intuition: true and justified beliefs don't require a causal explanation. They are just self-evident. We grapple for causal explanations (social or otherwise) only when rationality fails. This assumption, handy though it is as a heuristic and first approximation, does not survive careful scrutiny, and needs to be abandoned. A rich causal account of

> science, including the constitutive role of the social, in no way detracts from its epistemic credentials. Science, after all, is the concerted effort of many human brains. If we want a nonmiraculous explanation of science's successes, we had better be able to account for them in social terms.

What is the role of the social in science? If one consults science textbooks, one will find that the social dimension of scientific knowledge is conspicuously absent. Science is supposed to reflect the way the world really is, independent of our petty human lives. It is, in the classical view, the epitome of a rational endeavor, ~~untrammeled by outside influences~~. Of course, science is carried out by human beings, but their individual backgrounds and social lives are simply taken to be irrelevant. Individual scientists are effaced from the fruits of their intellectual labors, or relegated to historical footnotes. What matters are the intellectual merits of a theory, not who conceived it. What matters is the evidence, not who gathered it. This stark contrast between the social and the rational can be found in philosophical accounts of science as well. Because social factors are rendered invisible in the end products of science, many philosophers have underestimated their constructive role in the acquisition of scientific knowledge.

In recent decades, sociologists and historians have tried to bring science back to earth, but many of them have unwittingly bought into the same simplistic opposition. Social influences on science have been relished by its cynical critics and resisted by its admirers, and for the same reason: the fear (or hope) that it would destroy the credentials of science. In what follows, we discuss the historical roots of this opposition, culminating in the sorry spectacle of the science wars. We also point to a deeper cognitive explanation for this battle over the social nature of science: our basic intuition that rationally justified beliefs are not in need of any explanation, and that only false and foolish ones are.

Explaining Rational Belief

When do we feel the need to explain why someone believes something? Not all beliefs held by our fellow human beings appear to produce an epistemic itch. People believe that dolphins are mammals, that the earth orbits the sun, and that World War II ended in 1945, but we rarely wonder how they arrived at such homely truths. Beliefs such as these are just obvious, and no sane person would dispute them. Not only are we not interested in how other people came to hold these beliefs, we are also oblivious to how we did so ourselves. Who

told you when the World War II ended? Where did you acquire the belief that dolphins are mammals, or that the earth goes around the sun? Your sources for these convictions, though you surely must have had them, are hard to track down.

Psychologists distinguish between episodic memories and semantic memories (Squire and Zola, "Episodic Memory"). Episodic memories carry a mental source tag, containing the time, place, and situation where we acquired them. Semantic memories, by contrast, are floating ~~unfettered~~ in our mental space: we can no longer retrieve the moment in our lives when we first learned that dolphins are mammals, although surely there must have been such a moment. Knowledge about biological taxonomy is not innate, and in fact, as in the case of dolphins, it is often surprising and counterintuitive.

There are good reasons why our brains don't bother to keep a source tag for semantic memories: doing so would just clog our memory, and be a waste of brain resources. Take the belief that coal is black. People may have acquired this knowledge in any number of ways: some may have learned about soil deposits and compression of organic matter in elementary school, others had first-hand experience with the substance as a child, before learning about its origins. Still others may have learned about coal from the accounts of parents or friends. None of this is consequential for the end result: the culturally shared knowledge of a black, solid, combustible material called "coal."

If we ask you what your reasons are for believing that coal is black, you would probably be puzzled. The first answer that comes to mind is: "Why, because it *is* black, of course!" It doesn't matter how I came to know that. I could have learned it in any number of ways. Anyone in doubt about the color of coal can quickly retrieve the answer through any number of sources.

Because the truth of such beliefs is obvious, we rarely question how other people acquired them, or how they can justify them. It seems as if such beliefs just drop out of thin air, without much in the way of a causal history. People have many beliefs about the world, or so we think, simply by virtue of these beliefs corresponding to the way the world really is. If we are pressed to come up with a causal history of such beliefs, we say that people must have been exposed, one way or another, to the right kind of evidence or testimony. If such evidence is available in abundance, there is no need to worry about the details. In traditional accounts of knowledge, the explanation for true belief is pretty straightforward, as Paul Boghossian writes: "Under the appropriate circumstances, our exposure to the evidence alone is capable of explaining why we believe what we believe" (22).

Explaining other Beliefs

So how do we account for other kinds of beliefs (as held by others, of course)? Beliefs that are false, quirky, idiosyncratic, or plainly irrational produce an epistemic itch. We want to explain how people end up embracing them. Confronted with someone who believes that coal is red, that ostriches are mammals, that cellphones cause cancer, or that Elvis is still alive, we grapple for explanations. Who told him such nonsense? Is he the victim of some sort of prank? Did he fall for one of those conspiracy theories circulating on the Internet? Some beliefs are so blatantly false that we begin to question someone's sanity, or we assume that there must have been some sort of misunderstanding (Davidson).

In terms of accounting for why people believe certain things, we resort to special explanations only when something goes wrong. True beliefs that are part of common knowledge are taken at face value, but false and foolish beliefs cry out for an explanation. Some distorting influence is needed to explain why someone has arrived at a belief that no rational person would hold. This is where social and cultural explanations come in. We refer to someone's upbringing or social milieu, his allegiances and prejudices, and we invoke concepts such as peer pressure, indoctrination, misinformation, or ideological posturing. We say that someone is being contrarian, gullible, prejudiced, or trying to save face.

Such explanations, however, are not invoked when we account for true and justified beliefs, especially the countless mundane beliefs that all of us share. Only when rationality breaks down, it seems, a space is opened up for psychosocial explanations to fill. This association between the irrational (or arational) and the social works also in the opposite direction: if X comes up with an explanation of Y's belief in terms of social factors, we assume that X is dismissing Y's belief, or at least not taking it at face value. In folk psychology, rationality is the default of belief formation (Dennett; Bortolotti), but reasoning can be contaminated by social and psychosocial factors. Wherever the social holds sway, we should be suspicious.

This assumption that true beliefs don't need a causal pedigree, unlike false ones, has even dominated much of academic psychology. For a long time psychologists have investigated deviations from the canons of rationality (Kahneman et al.), documenting all the biases and errors that human reason is liable to, but they have been much less interested in the cognitive mechanisms of rational belief updating (Krueger and Funder). True beliefs, it seems, are formed out of their own accord.

Explaining Science

In the classical view, science is (supposed to be) the epitome of reason. It is objective and impartial. It is ruthless in its indifference to what we fear or fancy. When it comes to the content of science, nature has the final say in the matter. Social, political, and ideological influences on science are anathema. If they have any discernible effect on science, then something must have gone wrong, and we are no longer talking about science. As the physicist Steven Weinberg put it: "Whatever cultural influences went into the discovery of Maxwell's Equations and other laws of nature have been refined away, like slag from ore" (qtd. in Hacking 86). Indeed, the contamination by the social is one of the diagnostic criteria to distinguish science from pseudoscience. In many pseudoscientific theories, we see that ideological fashions and cultural sensibilities, rather than evidence, are the prime movers of theory development (Boudry and Buekens; Pigliucci and Boudry).

When writing science textbooks, and for many other purposes, the social influences on the development of scientific theories can be safely ignored, just like with many of our mundane beliefs about the world. Sure, there is a story to be told about how scientists pooled their efforts to acquire this or that piece of knowledge, who published it first, who convinced whom, and so on. At some point, so we assume, scientists must have been exposed to the relevant evidence. But the details of this story make no difference: an alternative history of science would ultimately have led to the same result. Not surprisingly, many textbooks of science have markedly little interest in the history of the laws and theories being discussed, or even in their evidential support. Scientific theories are timeless and universal, transcending the particular historical context in which they emerged. Especially in the natural sciences, students are simply taught scientific theories as if they descended down from some Platonic heaven. The vagaries of scientific history, the false starts, wrong turns, and dead ends, the protracted controversies between rival views, the forerunners and pioneers of scientific ideas—all of this is rendered invisible.

There are of course eminently good reasons for this neglect in scientific textbooks. Once a scientific theory has been firmly established, and its last reputable dissenters have died out or given up, the history of its eventual rise and triumph becomes a matter for historians and sociologists. There is no need for students of physics to linger over the priority dispute between Newton and Leibniz about the invention of calculus, or the chronological development of special and

general relativity, at least not when it comes to understanding these theories and putting them into practice.

For long, philosophers of science have also treated science in splendid isolation from the social world. Philosophy of science, under the influence of logical positivism, and in particular Gottlob Frege's attack on psychologism (Friedman), was mainly concerned with the logical structure of scientific theories, the relationship between theoretical propositions and observations, and the procedure or method for accumulating scientific knowledge. The philosopher Hans Reichenbach, one of the major proponents of logical positivism, taught us to strictly separate the *context of discovery* from the *context of justification*. The first deals with the historical conception of a scientific hypothesis, or the circumstances in which some observation has first been made, and is of little interest to philosophers trying to understand the logic of science. Brilliant ideas and findings may be arrived at in any number of ways, by anyone, under whatever circumstances. No rhyme or reason to be found there.

Philosophers of science, according to Reichenbach's famous stricture, should be solely concerned with how a scientific hypothesis, once it appears on the scene, relates to observations, whether it is internally consistent, whether it is falsifiable, and so on. The latter issue, which Reichenbach called the context of justification, deals exclusively with the logical relation between scientific hypotheses and the world, and is unaffected by the context of discovery (Schickore). There is no need to belabor the shortcomings of this highly idealized conception of science, as those have been amply exposed elsewhere (Creath). Perhaps more important is to see that, despite the obvious problems with this exclusive focus on the logic of justification, there is a sensible rationale behind the distinction. The ultimate goal of science is indeed to cancel out any influence of the social and to retroactively erase its own history: scientific theories have to stand or fall on their own merits, independent from their originators.

Down to Earth

In this idealized conception of science, which focuses on the successful end result of scientific activity, there is no place for any influence of the social, or indeed, for any of the actors involved in the scientific endeavor. All of that is swept under the carpet. But the fact that the eventual goal of science is to eliminate the social does not imply that social factors have no important role to play in the process. Science, after all, is nothing but the concerted effort of (sometimes not

so) humble human brains, none of which was designed to unravel the mysteries of the world on its own.

In the past couple of decades, science has been brought down to earth again by sociologists, cognitive scientists, evolutionary psychologists, and historians. Unfortunately, the opposition between the rational and the social is still besetting the naturalization of science. The backlash against the traditional conception of science, epitomized by the logical positivists and their intellectual heirs, has swung the pendulum in the opposite direction. Still under the spell of the dichotomy between rational and social (Galison), many science naturalizers have assumed that, as they bring science down to earth, its pretensions will start to unravel.

In *The Structure of Scientific Revolutions,* Thomas Kuhn famously argued that the history of science can be divided into periods of normal science, punctuated by episodes of revolution. During times of normal science, all scientists work within a certain paradigm, sharing background knowledge, methodologies, experimental procedures, and rules of inference. Nobody questions the validity of the reigning paradigm. The period of normal science ends when a critical level of "anomalies" has accumulated, that is, empirical and conceptual problems that the ruling paradigm has trouble dealing with. This crisis eventually leads to a revolution and a paradigm shift, after which normal science resumes again.

In periods of normal science, uncritical acceptance of the reigning paradigm is ensured through social conformity and transferred from the old generation to the new. During the revolutionary period, in Kuhn's picture, the social dynamics of science are even more important. This is because the old and the new paradigm, according to Kuhn, are "incommensurable," meaning that the choice of one paradigm over the other cannot be settled by rational means. It is akin to a gestalt switch, where two different conceptual frameworks offer a completely different perspective on a given phenomenon.

Many philosophers of science dismissed Kuhn's notion of paradigm shifts and incommensurability as a form of "mob psychology." In describing this gestalt switch between the old and the new, however, Kuhn opened up a space for social influences on science, which some sociologists have enthusiastically exploited and, to Kuhn's own dismay, pushed beyond what he himself thought reasonable. In the end, "whether a revolution occurs or the anomalies are simply ignored," as Golinski summarized, the approach of the radical sociologists "would depend on the social configuration of the community" (25). Sociologists such as Harry

Collins came to the rather surprising conclusion (to most scientists at any rate) that "the natural world has a small or non-existent role in the construction of scientific knowledge" (3).

There is a continuing debate about the legacy of Kuhn's work, and the correct interpretation of such ambiguous terms as "incommensurability" and "paradigm." In any case, as sociologists were following up on (what they claimed to be) Kuhn's lead, philosophers of science tried to reinstate the distinction between the rational and the social, carving out a restricted niche for social explanations. The proper place for the social was mainly defined in a negative fashion. Imre Lakatos, who was attempting to incorporate Kuhn's insights into the falsificationist philosophy of his mentor Karl Popper, used the notion of "research program" as a unit of analysis of the history of science, a less encompassing concept than Kuhn's "paradigms." According to Lakatos, good science proceeds in a rational way, unless or until a scientific research program starts to degenerate. When science shows signs of such degeneration, we can no longer explain what happens in a purely rational fashion, and we must look for additional social and psychological accounts. In other words: when rationality breaks down, the sociologists are allowed to jump in the fray. Larry Laudan, another important philosopher influenced by Kuhn, explicitly defended what he called the "arationality assumption": "The sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits" (202).

Both the strictures of Lakatos and Laudan, as well as the sociological relativism that they were battling against (Koertge), rest on the false opposition which we outlined above: when it comes to understanding why people believe certain things, we only look for psychological and social explanations when something goes wrong. From a pragmatic point of view, in other words, the social becomes salient only when rationality fails us. The truth of the matter, however, is that all beliefs, the true and the false ones alike, have a causal history, involving cognitive and social factors (in varying combinations). If we want to understand how people come to believe stuff, even simple and obvious propositions (e.g., dolphins are mammals) are in need of an explanation. Likewise, if we want to understand how scientists have been able to unearth all sorts of true beliefs about the world, we need to understand what kinds of people scientists are, what kind of cognitive strategies they bring to bear on their research questions, what the social organization of science is, and how hypotheses are tested and evaluated within a scientific community (Longino, *Science as Social Knowledge*; *The Social Dimensions of Scientific Knowledge*).

Opposing the Rational and the Social

The development of a cognitively and socially rich account of science has been delayed by the widespread misconception that such an account would compromise the epistemic standing of science (Haack). Because of our habit of pitting social and rational explanations against each other, we assume that the intrusion of sociology and psychology into the citadel of science will eat away at its foundations. As Philip Kitcher writes: "Much thinking about the growth of science is permeated by the thought that once scientists are shown to be motivated by various types of social concerns, something epistemically dreadful has been established" (305).

Or, as the radical sociologists would have it, something exquisite: this would finally bring down science as one worldview among many, with its own power structures, social dominance relations, and coalitions (Bloor). If we succeed in encroaching on the domain of science, so the sociologists seemed to think, surely we are debunking its epistemic pretensions. Why else were the traditional guardians of science trying to keep us out? Despite its lofty epistemic ambitions, science is shown to be nothing more than a social construction, and can be treated accordingly, in just the same way that sociologists treat religion and politics. At the heart of this opposition between the social and the rational, according to Mercier and Heintz, lies an individualist conception of reason, shared by both camps in the science wars: "On the science war front, both camps see reason as the ultimate place that is safe from sociological analysis. Defenders of the rationality of science against relativism could put rationality just there: in scientists' reasoning capacities. Protagonists on the other side of the front have seen in reasoning another attempt to resist naturalistic inquiries" (Mercier and Heintz 515). Mercier and Heintz are defending a deeply social account of human reason, according to which its prime function, from an evolutionary point of view, is to argue with other people. This, not coincidentally, is also the ecological setting in which human reason is most successful. Mercier and Heintz's view goes against the traditional concept of reason, in which others are seen as potential confounders or distorters, and human reason is regarded as a cognitive faculty that works best in isolation, free from external influences.

This idea of the social as a contaminant of the rational, to which even social constructivists seem to subscribe, is more indebted to logical positivism than the latter would like to admit. Radical sociologists were led astray by the very same intuition that made the logical positivists allergic to social explanations—only now they were welcoming the opposite conclusion. David Hull expressed their

line of reasoning as follows: "Because science did not possess the ideal characteristics that the 'positivists' insisted that it should, knowledge-claims made by scientists had no more warrant than those of magicians, faith healers, and politicians" (Hull xi).

It is not surprising that such iconoclasm has further entrenched the conviction that sociologists should get their dirty hands off the edifice of science. If the content of scientific theories were determined by social factors, by ideological fashions, or by the psychological quirks of scientists, how do we explain the impressive technological prowess of science? Science is a way of finding out objective truths about the universe. We don't need sociologists to explain the triumph of the germ theory of disease. Microscopic organisms really make us ill, social constructivism be damned.

Naturalizing Science

In our view, both camps are wrong (although, arguably, the relativist science bashers more so). The simple opposition between the rational and the social-psychological explanations goes against the grain of naturalism. Scientific knowledge does not drop out of thin air: it is embodied in real human beings. If our best scientific theories in some way reflect the world out there, this must have come about through the usual perceptual capacities and cognitive operations, with available technological equipment, and in a complex network of social interactions. How else could it have come about?

Science itself, after all, tells us that the human brain is a product of evolution by natural selection, and science the product of cultural evolution (Heintz, this volume). Humans did not evolve to unravel the structure of the cosmos. Indeed, evolution has equipped us with a host of biases and intuitions that served our ancestors well in the environment in which they had to survive and reproduce, but that often get in the way of our modern quest to uncover the nature of the universe (Blancke, Tanghe, and Braeckman, this volume). If humans succeed in overcoming these intuitions regardless, developing scientific theories that violate their intuitive worldview at every turn, then we need some nonmiraculous, bottom-up, naturalistic account of this achievement. The sociologists are right that science is a deeply social endeavor, and that all scientific knowledge is in this sense "socially constructed." No single individual marooned on a desert island, no matter how brilliant, would be capable of finding out any of the significant truths about the universe that we currently possess. Though the history of science has known some solitary geniuses, working in relative isolation from their

peers, even they were still engaged in a collective enterprise, in the sense that they were building on the work of numerous predecessors. Isaac Newton was standing on the shoulders of giants (and it's giants and lesser giants all the way down). If we want to understand anything at all about the accomplishments of science, we need to solicit the help of sociologists.

The realization that science is a deeply social enterprise, and that scientific consensus is reached through coalition forming and competition, should not surprise us. The question is what particular social organization is exemplified by science, and whether this is conducive to its epistemic aspirations. Scientists are human beings, warts and all. If scientists collectively succeed in finding out significant truths about the universe, while other endeavors have failed in this regard, this must have come about through the particular social dynamics of science.

Luckily, this research is now well underway. The discipline of social epistemology is investigating the particular social arrangements that are successful in producing knowledge (Goldman). Philip Kitcher, in his seminal *The Advancement of Science,* has developed a model of the microstructure of scientific change, paying attention to the division of cognitive labor and the social organization of science. Ronald Giere has investigated the phenomenon of "distributed cognition" in science ("Scientific Cognition as Distributed Cognition"). Hugo Mercier and Christophe Heintz have described scientific reasoning as inherently social and argumentative ("Scientists' Argumentative Reasoning"). To dissolve the opposition between the rational and the social, let us briefly consider some social influences on science, which, though initially seeming to threaten its epistemic ambitions, are actually enlisted in the interest of scientific progress.

Positive Roles for the Social

Many scientists believe that being objective and impartial are the cardinal virtues of science, and that bias and prejudice make one unsuitable for scientific work. Although the culture of science rightly encourages these virtues, they are by no means necessary for the success of science. Indeed, a certain modicum of bias in this or that direction may actually facilitate the progress of science.

It is not a problem that an individual scientist is biased, or emotionally attached to a particular hypothesis. The social organization of science makes sure that these biases will be balanced by others tilting in different directions. Helen Longino, for example, has put forth an account of the importance of epistemic diversity in the workings of science, arguing that (near) objectivity in scientific

endeavors emerges from two sources: on the one hand, science constantly confronts itself with the reality of the world, as assessed by our best empirical methods (*Science as Social Knowledge*). This leaves comparatively little room for (reasonable) alternative views: it is a matter of fact whether light is bent by gravitational fields, as the general theory of relativity predicts. On the other hand, the more cultural, gender, and ideological diversity there is within the scientific community itself, the more likely it is that culture-, gender-, or ideology-specific biases will be corrected. A standard example of this is the reorientation of aspects of medical research as a result of feminist epistemological critiques: it is now increasingly acknowledged that, for example, we cannot conduct drug tests solely on a population of (mostly white, middle aged) men and simply assume that the results can be extrapolated to other human biological populations (Gesensway).

In general terms, a good social arrangement for finding out the truth of some matter is to have two or more competing groups pursue different hypotheses, trying their utmost to garner evidence for their own view and to prove competitors wrong. As David Hull writes, with regard to the ideal of objectivity in science: "The objectivity that matters so much in science is not primarily a characteristic of individual scientists but of scientific communities. Scientists rarely refute their own pet hypotheses, especially after they have appeared in print, but that is all right. Their fellow scientists will be happy to expose these hypotheses to severe testing" (3–4). In other words, it is best to let a thousand flowers bloom in science. Even if you think some hypothesis is unlikely and far-fetched, it might still be worthwhile for some scientist to pursue it. The reward structure of science ensures that even implausible hypotheses will be explored by someone: there is a high premium on being able to show that a certain orthodox or received view is wrong. The chances of succeeding are dim, but then again, the reward is huge. This social arrangement attenuates the risk that science, as a whole, misses out on an apparently unlikely alternative that might be borne out after all.

Many controversies in science can be viewed as a battle between the opposing biases of conservatism and rebelliousness. According to Philip Kitcher, cognitive variation among scientists on this dimension is conducive to progress in the long run (*The Advancement of Science*). Some scientists are mavericks, quick to challenge established views and pursue new avenues, while others are traditionalists, suspicious of radical ideas and inclined to defend the orthodoxy as long as possible. There is no single strategy that is always successful: the mavericks take more risks and will often turn out to be wrong, but may sometimes strike gold and thus prevent the ossification of scientific orthodoxy. Traditionalists are often right in sticking with the old ways, and are not the ones to waste time and effort on wild and improbable ideas. But sometimes they will be proven wrong too. There is no

single ideal way to strike a balance between caution and innovation, between persistence and flexibility. A social arrangement that allows for different cognitive strategies to flourish, according to Kitcher, will produce better results than one that attempts to enforce one single "rational" policy.

A desire for fame and success is often viewed as unworthy of a real scientist. The goal of science is truth for its own sake. Although such base motives may indeed compromise one's scientific work, if allowed to be unchecked, there is no convincing reason why they would stand in the way of significant discoveries. Even spite, jealousy, and the desire to humiliate a rival can result in excellent scientific work, if the competing parties know that they have to abide by certain rules, and will be called out whenever they violate them. In any case, a desire for fame and success does not compromise the collective goals of truth and objectivity. Institutional arrangements provide selfish motives for honesty and truthfulness in reporting and sharing results with others (Campbell; Goldman; Haack).

Indeed, social competition may be more effective as an incentive to do science than the pure and noble goal of discovery, especially when it comes to the laborious and repetitious work that science often demands. As Susan Haack puts it, competition is "an aid to our limited energy and fragile intellectual integrity" (108). Scooping a rival may be more thrilling than laying another brick in the edifice of knowledge, but that's no problem, as both may be accomplished at the same time. Fraud is ruthlessly punished in the world of science, not just because it undermines the relationship of trust on which science is based, but also because it is an unfair shortcut to the success and professional achievement that many scientists are striving for. What goes for individual rivalry also applies to competition between research groups, as David Hull writes: "As unseemly as factionalism in science may be, it does serve a positive function. It enlists baser human motives for higher causes" (349).

In all these cases, social influences are not an impediment to the epistemic ambition of science, but rather a facilitator of scientific progress. Science harnesses some of the baser motives of human behavior in the service of truth, making sure that the interplay of scientists' individual interests and biases mostly align with epistemic progress. Social constructivists are right that, in the battle between competing paradigms (or research programs), the social configuration of the research community plays an important role. This is especially true in the early stages of scientific research, when evidence is still ambiguous and incomplete, conceptual problems abound, and social factors are given free rein. Even the final vindication of the correct scientific theory, however, is also accomplished through social means: forming alliances, maintaining a good reputation, showing courage to challenge received views, and exercising restraint in attacking rivals.

Charles Darwin may have been right from the start about the fact of common ancestry, but his theory would not have carried the day as swiftly as it did without Darwin's indefatigable efforts to enlist allies to the cause and to engage and negotiate with his critics. All the parties in the dispute were trying to enlist nature as their ally, but Darwin of course had one big advantage: nature really was on his side all along. In the long run, therefore, as evidence accumulates and factions wax and wane, the social influences on science will be filtered out, and rightly so.

Conclusion

The development of a thoroughly naturalistic account of science has been delayed by the widespread misconception that this would compromise its epistemic standing. Sociological accounts of science have been met with distrust by lovers of science, and have been relished by its cynical critics, giving them ammunition to undermine science's lofty pretensions. Beneath the surface, these apparently rival views are committed to the same assumption: that social intrusions into science would undermine its epistemic ambitions. The only difference is that one party welcomed this prospect, while the other loathed it.

But the assumption is false. Science is social to the bone. No single human brain would be capable of accomplishing any of science's successes in isolation from others. Science is nothing but the concerted effort of fallible human brains to understand nature. As science itself tells us, those biological organs evolved for other activities than unraveling the structure of the cosmos. Science contains many safeguards against the contamination of social factors (e.g., double blind procedures), but in many respects, the social structure of science, along with the social relations between its actors, are conducive to its successes. Even the baser motives of the human mind—pride, jealousy, revenge—can be enlisted for the benefit of science.

It is true that the eventual goal of science, as a collective human endeavor, is to efface human actors and their social lives from view. If we ever find out that the theoretical content of our best scientific theories reflects the ideology of its originators rather that an approximate understanding of the world as it is, or if it carried the day for purely sociological reasons, then something would be seriously wrong. That would be a reason to start all over again. Science, then, is the sustained social effort to create something from which, eventually, the social will be eradicated.

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