

Introduction

The Science Wars in Perspective

The steadily growing influence of science and technology on all aspects of life will be a major theme in any retrospective assessment of the twentieth century. In spite of — and perhaps also because of — this unquestionable influence, the very nature of science and technology is, at the close of the century, a deeply contested issue. Indeed, so contested is the issue that the public media now regularly report on the latest battles in the so-called “science wars.” Increased publicity both reflects and reinforces increased polarization, with the result that there often seem to be only two sides. On the one side, we find what I would call “enlightenment rationalists” or “metaphysical realists,” but often derisively referred to as “reductionists” or “essentialists.” This camp includes most scientists, most members of the public, and some historians and philosophers of science. The other camp contains mostly intellectuals, some historians and philosophers of science, many sociologists of science, and many students of literature and culture more generally. To their enemies, these students of culture, scientific and otherwise, are merely “relativists” or “post-modernists.” At their most polarized, the science wars are portrayed simply as battles between humanists and scientists.¹

To be sure, one can find contemporary students of scientific culture claiming that “there is no obligation upon anyone framing a view of the world to take account of what twentieth-century science has to say” (Pickering 1984, 413). My rejection of such views will become clear very shortly. Here I wish to concentrate on the flavor of the opposition. This comes across clearly in a commentary on his spoof of cultural studies (1996a) by the physicist, Alan Sokal. “Anyone who believes that the laws of physics are mere social conventions,” Sokal writes, “is invited to try transgressing those conventions from the windows of my [twenty-first floor] apartment.” (Sokal 1996b, 62) Now, anthropologists tell us that our pre-literate ancestors were well aware that falling from great heights can be deadly. Such knowledge is grounded in quite basic human, and even animal, experience. Indeed, some early humans apparently put this knowledge to good use, securing food and hides by driving large animals over cliffs. But no student of language or culture would be so foolish as to claim that these ancestors had any knowledge of the laws of physics. Indeed, they could not even have had the *concept* of a *law of nature*. That concept, in its modern form, did not come to the fore until maybe as late as the sixteenth or seventeenth century in Western Europe. It seems not to have existed in China, which by then already had a very long tradition of philosophies of nature. Indeed, the most elementary histories of science inform us that Newton’s conception of universal gravitation was initially greeted by many of the most intelligent people of his time with considerable skepticism. How, they asked, could one body possibly act at a distance on another? Later physicists introduced the concept of a gravitational field, which at least could be thought to act where the action is. Gravity and fields of force may not be “mere social conventions,” but even to think in such terms requires fairly sophisticated concepts created relatively recently by people living in literate cultures. Part of understanding what science is all about is understanding how these humanly created concepts enable us to connect in such exquisite ways with the real world. The science wars distract us from pursuing such understanding.

Issues underlying what are now called the science wars have been debated for more than a decade within the science studies community itself. The essays collected in this volume provide one participant’s perspective on these issues. It is not a perspective from inside either of the

recently constructed warring camps. It is from between, beyond, outside, above, or below these positions — but it is not a view from nowhere. My *intellectual roots* are in the sciences, first as a student of physics, but later also as a follower of work in statistics, biology, geology, and the cognitive sciences. My *professional identification* has always been with the philosophy of science. Nevertheless, I have always been engaged, both intellectually and professionally, with neighboring disciplines, first the history of science, and later also the sociology of science and science studies more generally. My perspective is not *interdisciplinary* but, in so far as this is possible for a single person, *multidisciplinary*.²

I begin with the conviction that there exists much genuine scientific knowledge. Moreover, I firmly believe there have been dramatic increases in scientific knowledge during the twentieth century, particularly since World War II. We have learned, for example, that inheritance is carried by DNA molecules with a two-strand helical structure. And even the continents have not always been where they now are. In stating these convictions I am not merely playing games with the meaning of expressions like “scientific knowledge.” I intend such expressions in the relatively ordinary sense that scientific knowledge is knowledge *of the world* and that there is a difference between knowledge and mere opinion, even widespread opinion.

Thus understood, most scientists and knowledgeable members of the general public agree with these convictions. So what is the problem? It is that the more theoretical ways these simple convictions have generally been understood no longer square with other things that have been learned about what constitutes scientific knowledge and how it is acquired. The problem is not with current scientific theories of the world, but with current theories (or meta-theories) of what it is to acquire good scientific theories of the world. As is typically the case for individuals, our collective self-knowledge lags behind our collective knowledge of the world.

The theories *about* science prominent in Europe and North America until around 1960 derived ultimately from the Enlightenment of the eighteenth century, a period in which the achievements of the seventeenth century scientific revolution became incorporated into a more general cultural world view. A number of concepts came into prominence in particular ways during that time, including those of *laws of nature*, *scientific truth*, and *scientific rationality*. These particular concepts were connected, of course, in that it is through the power of rational inquiry that true laws of nature were thought to be discoverable at all. In spite of a general romantic reaction in the early nineteenth century, later prominent dissenters such as Nietzsche, and some disillusionment produced by World War I, the Enlightenment picture of science seems to have dominated thinking about science from the mid-nineteenth to the mid-twentieth century. Indeed, this picture of science was reinforced in the twentieth century, inspired by the new physics, quantum mechanics and relativity theory, associated above all with Einstein. Some of the philosophers who founded what became known as Logical Empiricism explicitly thought of themselves as part of a “Second Enlightenment.”³ Fundamental concepts like those of laws of nature, scientific truth, and scientific rationality were retained, but reformulated in the idiom of the formal logic first developed by Russell and Whitehead around 1910. Laws of nature came to be understood as true universal generalizations and rationality as logical inference, above all deductive, but also, programmatically at least, inductive. The spirit of Logical Empiricism was not confined to philosophers of science. Following World War II, it spread, especially in North America, to the sciences themselves, particularly the behavioral and social sciences, and somewhat into the culture at large.

The Enlightenment picture of science was explicitly challenged in the early 1960s, particularly in Thomas Kuhn's *Structure of Scientific Revolutions*. The main lesson I take from Kuhn and many other students of scientific practice is that the very categories in which the Enlightenment view of science was formulated are inadequate to capture the actual practice of science, both historically and in its contemporary forms. Concepts like that of laws of nature, truth, and rationality are not givens, but are themselves interpretive categories which have their own histories.⁴ Science need not be understood in these terms and, indeed, may be better understood in other terms.

This way of understanding the lessons of critical studies of science for the past generation explains part, maybe even a large part, of the intellectual basis of the current conflict over the status of science. Many people on both sides seem so to have internalized the Enlightenment view of science that, for them, to challenge aspects of that view is to challenge science itself, and, conversely, to defend science is to defend it in its Enlightenment form. From my point of view, neither of these reactions is correct. There are more general, or more basic, notions of scientific knowledge and progress that may be held apart from their Enlightenment presentations. The intellectual basis of the conflict, therefore, may be undercut by the realization that there are *other ways* of conceiving scientific knowledge.⁵

The underlying purpose of these essays is to develop and promote *one* other way of thinking about scientific knowledge. My deepest methodological commitment is to *naturalism*. This commitment is expressed not so much in terms of particular doctrines, but as a program — a way of approaching the subject. The primary methodological principle of naturalism, as I understand it, is not only to avoid appeals to anything supernatural, but also to avoid appeals to *a priori* claims of any sort. This principle is intended to have the consequence that any conclusions one reaches about the nature of science are subject to criticism based on theoretical, historical, psychological, or social investigations into particular scientific practices. My naturalism, therefore, rejects appeals to any presumed non-empirical philosophical method, such as logical or conceptual analysis, to reach conclusions about the nature of science.⁶

The fundamental concept in my particular understanding of scientific practice is that of a *model*. Models, for me, are the primary representational entities in science. Scientists, I claim, typically use models to represent aspects of the world. The class of scientific models includes physical scale models and diagrammatic representations, but the models of most interest are *theoretical* models. These are abstract objects, imaginary entities whose structure might or might not be *similar to* aspects of objects and processes in the real world. Scientists themselves are more likely to talk about the *fit* between their models and the world, a terminology I happily adopt.

I argue that what are called laws of nature function ambiguously in the actual practice of science. On the one hand, they may be regarded as *principles*, such as the explicitly named Principle of Covariance, The Uncertainty Principle, or The Principle of Natural Selection. As such, I claim, they are not even candidates for being truths about the world. They are not statements, but general rules for the construction of models. Incorporated into the characterization of particular models, however, they do function as true statements, but not as statements about the world. They are then truths only about an abstract model. In this context, such statements are true in the way explicit definitions are true. The empirical question — the question of realism — is how well the resulting model fits the intended aspects of the real world. And here my central claim is that fit is always partial and imperfect. There is no such thing as a

perfect model, complete in all details. That does not, however, prevent models from providing us with deep and useful insights into the workings of the natural world.⁷

Somewhat contrary to the deliberately provocative title of this book, therefore, my view is not that there are *no* roles for the concepts of laws of nature and truth in a proper understanding of science. It is, rather, that those roles have been misrepresented in the Enlightenment view of science. Science does not deliver to us universal truths underlying all natural phenomena; but it does provide models of reality possessing various degrees of scope and accuracy. That dual conclusion may not provide extremists with all they desire, but I think it provides all that anyone can reasonably ask.

Except for the fact that it would have made a terrible title, I could have added: *Judgment without Rationality*. Here I am concerned only with judgments about the fit between particular models and aspects of reality. And of course I do not claim that scientists' judgments in these matters are "irrational." It is the conception of rationality that is at issue. The original Enlightenment dream was of universal, categorical principles of rationality that could guarantee the truth of natural laws meeting their conditions. By the twentieth century, this dream was generally reduced to a desire for universal principles that yielded the *probable* truth of natural laws. My view is that there are no such principles.

Coming to hold that one model fits better than others is not a matter of pure reasoning or logical inference. Rather, it is a matter of making a *decision*. Effective decision making requires strategies for reaching desired goals. This applies to business and military decisions as well as to scientific decisions. If one wishes to talk of rationality here, it is a *conditional* or *instrumental* rationality, a matter of using effective means for reaching desired goals. In the case at issue, one goal shared by most scientists is to choose from among the available alternatives the model that best fits the real world. But other goals, such as a desire for personal or professional gain, cannot be excluded. Experimentation, however, provides an aid to making such decisions. Sometimes a fortunate combination of data and experimental design will lead all but the most stubborn of scientists to make the same judgment. That is as close as science ever gets to the Enlightenment ideal. In less ideal cases, a scientific consensus may form based more on shared values than on empirical data. It is not surprising, therefore, that both sides in the science wars can cite cases in the history of science supporting their positions. Both sides, I think, are guilty of selective attention and over-generalization.

As an aid to the reader, I have grouped the chapters more by style and intended audience than by specific topic. The essays in Part I were all written as overviews for audiences ranging from general philosophers of science to interested scholars in science studies and in the sciences themselves. They all situate my view of science among the current alternative views. These essays should be accessible also to people new to the field of science studies, perhaps even to advanced undergraduates. The essays in Part II develop various aspects of my own view for an audience that includes scholars in all the science studies disciplines. The first of these, "Naturalism and Realism," links the general program of naturalism with a version of scientific realism that is both constructive and perspectival while remaining genuinely representational. The other essays in this group develop aspects of the view that it is models, not laws in the traditional sense, that do the important representational work in science, and that judging the fit of a model to the world is a matter of decision, not logical inference. Finally, the essays in Part III are directed more specifically to my fellow philosophers of science, although even these should be accessible to members of a wider science studies community. The final paper, in particular, is

a prolegomenon to the historical study of Logical Empiricism in North America. It raises questions and suggests hypotheses that future historians of the philosophy of science from roughly 1938 to 1962 might find useful. Above all, it suggests that the philosophy of science itself exhibits the deep historical contingency that, from my perspective, is characteristic of all scientific knowledge.

References

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Notes:

¹. This polarization is clearly exhibited in the major sustained work to date from the scientific side, Gross and Levitt's *Higher Superstition* (1994). Here I provide only references applying specifically to this introduction. More extensive references are contained in the notes to individual chapters.

². The distinction between *interdisciplinary* and *multidisciplinary* is defended in Chapter 3.

³. For example, this theme appears in a number of Feigl's (1981) essays.

⁴. Historical studies of a related concept, *objectivity*, have recently appeared (Megill 1994).

⁵. My reference to the *intellectual* basis of the conflict suggests that there are other, *non-intellectual* bases as well. And, indeed, this is certainly the case. There is *some* truth to the charge that some of those who reject scientific claims of genuine knowledge of the world have been motivated by a desire to undercut the authority of science. And this desire is often connected with a belief that scientific authority supports repressive social and political forces. On the other hand, scientists and their supporters desire the prestige and power that comes from a general belief that they possess uniquely rational methods that enable them to discover universal laws governing the fundamental workings of the natural world. Few today would follow Newton in maintaining that they were uncovering God's laws for nature, but the motivation is similar. My concern here, however, is with the

intellectual bases of the conflict. If those bases can be eliminated, that may contribute to a lessening of the overall conflict — even though intellectual agreement is hardly a guarantor of social or political agreement.

⁶. Not surprisingly, naturalism is also a concept with many meanings. My understanding of naturalism derives from debates within philosophy and the philosophy of science. For many sociologists, on the other hand, naturalism is the product of projecting social arrangements into nature, particularly biological nature. Philosophical naturalists may also be, but need not be, naturalists in this latter sense.

⁷. The philosophically knowledgeable reader may recognize a similarity between these views and those expressed by Nancy Cartwright in *How the Laws of Physics Lie* (1983). Similarity there is. I hope, however, that my way of putting things is at least somewhat clearer. On my view, the kind of laws Cartwright is talking about cannot lie, but not because they tell the truth. Principles and definitions are not the sorts of thing that are even candidates for being either liars or truth-tellers about the real world.