Derek Turner

Making Prehistory

Historical Science and the Scientific Realism Debate

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Scientists often make surprising claims about things that no one can observe. In physics, chemistry, and molecular biology, scientists can at least experiment on those unobservable entities, but what about researchers in fields such as paleobiology and geology who study prehistory, where no such experimentation is possible? Do scientists discover facts about the distant past or do they, in some sense, make prehistory? Derek Turner argues that this problem has surprising and important consequences for the scientific realism debate. His discussion covers some of the main positions in current philosophy of science – realism, social constructivism, empiricism, and the natural ontological attitude – and shows how they relate to issues in paleobiology and geology. His original and thought-provoking book will be of wide interest to philosophers and scientists alike.

DEREK TURNER is Assistant Professor of Philosophy at Connecticut College.

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For Michelle I. Turner

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Introduction

Much of the sound and fury in the philosophy of science over the last few decades has had to do with a view – or better, a family of views – known as *scientific realism*. Pick up any issue of *Science* magazine, and you will find reports on research dealing with microphysical entities, properties, events and processes. For example, one article in the August 19, 2005 issue includes the following claim:

> When x-ray photons pass through a liquid sample that is thin compared to its x-ray absorption depth, less than 1% of the photons are scattered (Anfinrud and Schotte 2005, p. 1192).

Oversimplifying shamelessly (I will get to the finer distinctions later), the scientific realist thinks that scientists know a great many things like this, even though no one could possibly see or smell an x-ray photon, or bump into one while wandering about at night. The realist holds that a great many scientific claims like this one are true, or nearly true.[1] Those x-ray photons are really out there, and liquids really do have x-ray absorption depths – really! And what's more, scientists did not bring any of this about; they *discovered* it all. What happens to photons when they pass through a liquid sample does not depend at all on what we think about photons, or on the concepts we use to think about them, or on the language we use to talk about them. The history of science is a tale of progress in which scientists learn more and more (or get closer and closer to the truth) about how the world really is, independently of us. That's realism.

[1] What could it mean for a claim to be nearly, or approximately true? Realists have struggled to clarify the notions of approximate truth and verisimilitude, with mixed success. Indeed, the difficulty of explaining what approximate truth could be has driven many philosophers away from realism. See Psillos (1999, ch. 11) for one helpful recent discussion of this issue from a realist perspective.

But x-ray photons are one thing; dinosaurs, shifting tectonic plates, and evolutionary processes also pose a challenge. Should we be realists about those things? Should we be realists about prehistory?

Most of the philosophers who think and write about scientific realism take their examples from the study of the microphysical world. Sadly, historical sciences, such as paleobiology and geology, have been left almost entirely out of the discussion, even though one cannot see, or smell, or bump into a living dinosaur any more than one can an x-ray photon.[2] As a result, I argue, the scientific realism debate has been skewed. I have written this book with two audiences in mind: First, I hope to show philosophers of science how our assessment of the arguments for and against scientific realism, and of some of the main positions that philosophers have staked out in the realism debate, might change when we examine them with an eye toward the scientific study of prehistory. Second, I hope to show scientists who study prehistory that the scientific realism debate, contrary to the impression one would get from perusing the philosophical literature, has relevance to their work, and may even have the potential to change the way they conceive of what they do.

One tried and true recipe for a philosophical book is to begin with one or two claims that strike everyone as boring, obvious, and uncontroversial; then show, by a series of unimpeachable logical steps, that these claims have surprising, counterintuitive consequences whose truth no one ever would have suspected. The more boring and uncontroversial the original claims, and the more surprising and counterintuitive the consequences, the better.

In this book, I begin with two boring and obvious claims about how the past differs from the microphysical world. I give these two claims high-sounding names – the *asymmetry of manipulability* and the *role asymmetry of background theories* – but there is nothing fancy or even very subtle about the ideas themselves. The first idea is that although we obviously cannot change the past, we can use technology to manipulate things and events at the microphysical level. Scientists have designed particle accelerators that make it possible for them to run experiments in which they crash subatomic particles together. For other vivid examples of technological control of microphysical events and processes, think of nuclear

[2] See, however, Wylie (2002, ch. 5) for a defense of scientific realism in the context of archaeology. Wylie just presents the case for realism in general and concludes that we should be realists about the past. She does not consider the possibility that the strength of the arguments for and against realism might vary depending on the scientific context.

weapons, or genetic engineering, or current research in nanotechnology. The second idea has to do with what philosophers of science call background theories, or theories that scientists take for granted in the course of their research. In historical science, background theories all too often tell us how historical processes destroy evidence over time, almost like a criminal removing potential clues from a crime scene. For example, the fossilization process destroys all sorts of evidence about the past, with the result that we will never know many things about the past, such as the colors of the dinosaurs. In experimental science, by contrast, background theories more often suggest ways of creating new empirical evidence. For example, one can scarcely begin to understand the development of modern physics and astronomy without appreciating how the study of optics, or the behavior of light as it passes through lenses, bounces off mirrors, and so on, contributed to (and also benefited from) the development of ever more sophisticated microscopes and telescopes. More generally, part of the point of experimentation in science is to create new evidence, and background theories about microphysical entities and processes often suggest new ways of doing that. Taking quantum theory for granted enables scientists to build particle accelerators, which in turn enable them to run new kinds of experiments. In historical science, background theories often tell scientists how the evidence has been destroyed; in experimental science, they often tell scientists how to manufacture new evidence.

Hopefully all of this sounds like common sense. In this book, I undertake to show that these fairly obvious ideas have important and surprising consequences that most philosophers of science have yet to appreciate. In the first part of the book (chapters 1 through 5), I examine the main arguments for and against scientific realism, and I show that the strength of those arguments varies in interesting and sometimes complicated ways, depending on whether we are talking about the microphysical world or about prehistory. For example, chapter 5 argues that novel predictive successes will be fewer and further between in historical than in experimental science. If that is right, it bears directly on one of the most popular current arguments for scientific realism: the argument that interpreting theories realistically is the best way to make sense of their novel predictive successes. This is one of the things I mean by saying that the scientific realism debate has been skewed by the neglect of geology and paleobiology. I also look at the consequences that the asymmetry of manipulability and the role asymmetry of background theories have for the underdetermination problem (chapter 2), the more traditional arguments for scientific realism (chapter 3), and the pessimistic induction from the history of science (chapter 4).

The title of this book, *Making Prehistory*, hints at the sort of social constructivist views that many scientists find kooky, or worse. You may be thinking: "Surely he's not going to argue that dinosaurs are social constructs, or that their extinction is something that the scientific community – somehow – brought about." Don't worry; I am not going to argue that. But I am not a scientific realist, either, at least not across the board. Instead, I defend a view, the *natural historical attitude*, which is inspired by the work of the philosopher Arthur Fine (1984, 1986, 1996). Fine, caring more about physics than about geology or paleobiology, called his own view the "natural ontological attitude." The natural historical attitude is one of agnosticism with respect to the metaphysics of the past: Maybe we have made prehistory, and maybe we haven't. But if we take our own best theories about the past seriously, they clearly imply that we will never have any historical evidence that could adjudicate the dispute between metaphysical realists and social constructivists, so we do best to suspend judgment and move on to other things. That is the take-home message of chapter 6.

Among philosophers of science, the most respectable alternatives to scientific realism are Arthur Fine's natural ontological attitude and Bas van Fraassen's constructive empiricism, a radical view which has it that our knowledge is entirely restricted to what we can observe. Van Fraassen, like Fine, has concerned himself mainly with physics, and both of these versions of non-realism look like genuine contenders so long as we restrict our attention to the microphysical world. However, I argue in chapter 7 that when we turn our attention to the scientific study of prehistory, van Fraassen's view has such repugnant consequences that it must drop out of serious contention. This, incidentally, is another way in which the realism debate has been skewed by the neglect of historical science: Van Fraassen's constructive empiricism seems at first like a viable philosophical theory of science, but only so long as we ignore geology and paleobiology.

In the concluding chapter, I take up the issue of consilience, or the idea that scientists can have some confidence that they are getting things right when they can offer a unified explanation of a variety of seemingly unrelated phenomena. What should someone who takes seriously the asymmetry of manipulability and the role asymmetry of background theories say about consilience? How might our understanding of the role of consilience in historical science be affected by adopting the natural historical attitude? I argue that while appeals to consilience do have some evidential weight, the asymmetries also mean that scientists should be moderately skeptical about such appeals.

Most scientists who work at reconstructing the past seem to take a realist view of prehistory. This consensus, or near consensus, can make it seem as though realism were the most natural or most obvious position. One potential explanation for this near consensus is that philosophers of science have not articulated any serious non-realist alternatives. Another potential explanation is that none of the great theories of historical science – evolutionary theory, plate tectonics, etc. – cause trouble for scientific realism in quite the way that quantum theory does.

During the twentieth century, the scientific realism debate evolved in step with significant changes in theoretical physics. Disagreements about how to interpret quantum theory, for example, became tangled up with disagreements about whether to adopt a realist or an instrumentalist interpretation of scientific theories. Without going into details, we can note that quantum theory has two features which, taken together, raise some pretty basic philosophical questions: First, that theory has proven itself to be wildly successful at generating accurate predictions. And second, if we take literally what quantum theory implies about the microphysical world – for example, about the superposition of states, about the collapse of the wave function, about non-locality, and much else – the theory seems wildly unfamiliar and counterintuitive. These two features have driven many philosophers and scientists towards *instrumentalism*, or the view that scientific theories are just instruments or tools for generating predictions. Instrumentalism treats scientific theories as a kind of technology. If quantum theory is merely a complex mathematical tool for generating accurate predictions, in exactly the same way that a hammer is a tool for driving nails, then there is no need even to ask whether the theory accurately represents the microphysical world. Contrary to realists, instrumentalists hold that truth and accurate representation are not what science is all about. Instead, science is all about results, and about increasing our control of the world around us. At any rate, since theories in physics often naturally and inevitably give rise to the sorts of questions that animate the realism debate, probably no one needs to explain why physicists should care about that debate. But what about geologists and paleobiologists? What might they stand to gain from an exploration of the realism debate?

Although this book is mainly an essay on scientific knowledge, many of the questions raised here also have to do with issues of status and prestige. Within biology, for example, cell and molecular biology and anything involving medical research tend to enjoy a somewhat higher status. Subfields such as ecology, and anything involving whole organisms, enjoy a somewhat lower status. At the low end of the totem pole, we find paleontologists, who study whole organisms that do not even exist anymore. In his classic, *Wonderful Life* (1989), Stephen Jay Gould makes an impassioned "plea for the high status of natural history" and laments the fact that people so often associate the experimental method with *the* scientific method. He quotes the physicist Luis Alvarez – ironically, one of the formulators of the hypothesis that an asteroid collision caused the mass extinction at the end of the Cretaceous period – as saying: "I don't like to say bad things about paleontologists, but they're really not very good scientists. They're more like stamp collectors" (1989, p. 281). Of course, it is not true that all paleontologists do is to collect specimens from the field and publish descriptions of them, but the quotation reveals something about how people have perceived the study of prehistory. Gould, for his part, argues with great passion and eloquence for a view that could be summed up by the slogan, *Different Methods, Epistemic Equality*. That is to say, historical science and experimental science necessarily employ different methods of investigation, as well as different styles of explanation, and they emphasize different things (particulars *vs*. laws and regularities). But according to Gould and some other more recent writers (such as Carol Cleland, whose work I discuss in chapter 2), these methodological differences make no significant epistemological difference: When it comes to delivering scientific knowledge, historical work is every bit as good as experimental work.

I would like to renew Gould's plea for the high status of historical science. However, Gould goes about making that plea in a counterproductive way. The asymmetry of manipulability and the role asymmetry of background theories really do place historical researchers at a relative epistemic disadvantage, so the slogan "Different Methods, Epistemic Equality," is mistaken. In its place I would propose a different slogan: *Epistemic Disadvantage, Equal Scientific Status*. I try to drive this point home in chapters 2 through 5 by examining the main arguments that philosophers of science have discussed in connection with the realism debate. In my view, rather than denying the epistemic disadvantages of historical science, we can make the best case for the high status of natural history by calling attention to those disadvantages and even celebrating them. If we were watching two distance runners, one of whom runs along a smooth track (perhaps even one that is outfitted with one of those moving walkways you find in airports), while the other runs along hilly and treacherous terrain, we should think very highly of the second runner, even if she takes longer to cover the same distance. Acknowledging that those who study the past find themselves at an epistemic disadvantage relative to those who study the microphysical world is also the key to understanding some of the most interesting developments in paleobiology and geology over the last few decades, such as the use of computer simulations to carry out numerical experiments. Numerical modelling is a strategy for coping with the asymmetry of manipulability.[3]

What else might we gain from this exploration of the consequences of the two asymmetries, and of the ways in which the scientific realism debate has been skewed toward the microphysical? For too long, discussions of historical science have been constrained by the traditional distinction between *ideographic* and *nomothetic* science.[4] We owe that terminology to the neo-Kantian philosophers, Wilhelm Windelband and Heinrich Rickert, who thought that this distinction shed some light on the differences between the natural sciences (*Naturwissenschaften*) and the human sciences (or *Geisteswissenschaften*). According to this tradition, nomothetic science is concerned with laws and regularities, or with patterns involving types of events. Ideographic science, by contrast, focuses on sequences of particular events, or on event tokens. Ideographic science, not surprisingly, is often thought to involve some sort of narrative. Kepler and Newton were doing nomothetic science. The nineteenth-century geologists who first drew the inference that much of the northern hemisphere was once covered by an ice pack were doing ideographic science. For my part, I have not found the ideographic/nomothetic distinction to be very helpful. Paleontologists have taken advantage of laws of biomechanics to infer how fast a dinosaur was walking when it made a particular set of tracks (Alexander 1976). It is also possible to use biomechanical considerations to deduce the maximum swimming speeds of extinct marine reptiles (Massare 1988). Geologists run elaborate computer simulations

[3] As will become apparent, my interpretation of these recent developments is deeply influenced by Huss (2004).

[4] For a helpful discussion of this distinction, see Tucker (2004, p. 241). I have also found Stephen Jay Gould's (1987) to be very illuminating. Much of Gould's work in the 1970s and 1980s was animated by the idea that paleobiology need not be an entirely ideographic discipline. According to Gould, paleontology "resides in the middle of a continuum stretching from idiographic to nomothetic disciplines" (1980, p. 116). Gould, Raup, Sepkoski, and Simberloff began using stochastic models of evolution during the 1970s, and they saw that as an attempt to make paleobiology into a more nomothetic discipline (see Raup et al. 1973; Raup and Gould 1974).

to test ideas about what the earth's climate might have been like 600 million years ago. Each simulation models a series of particular events, but scientists run the models over and over again, refining them and adjusting parameters as they go. Are these examples of ideographic or of nomothetic science? What could we gain by forcing these examples into one category or the other? I aim to show that we can get a much more realistic picture of historical science (in the ordinary, not the philosophical sense of "realistic") if we cut loose from this distinction between ideographic and nomothetic science and focus instead on the epistemically relevant differences between the different kinds of unobservable things that scientists study.

Finally, why should scientists care about the natural historical attitude? For I really do recommend that attitude as a good one for geologists, paleobiologists, and even archaeologists and historians to adopt. But what difference would such an attitude make to working scientists? I offer two answers to this question. First, the disconnect between philosophical discussions of the arguments for and against scientific realism, on the one hand, and historical science, on the other, has left scientific realism as the default view of the sciences of prehistory. Since no one has articulated any serious alternatives to realism with respect to geology and paleobiology, realism is the only game in town. The few philosophers who have thought deeply about non-realist views about the past – for example Michael Dummett – have had little or no interest in the details of the practice of historical science. My worry is that when a certain philosophical view seems to be the only game in town, there is not much incentive for anyone to enter into an open and critical discussion of the fine points of that view. At any rate, I will argue that certain parts of the realist view of the past – especially the part about the past having occurred independently of us – go way beyond what is justified by the historical evidence. And I recommend the natural historical attitude as a stance that is more Spartan and less burdened with philosophical theory than metaphysical realism, and one that evinces greater respect for the limitations of historical evidence.

Second, over the last few decades, scientists and philosophers alike have been caught up in what have come to be known as "the science wars" (for a wonderful discussion, see Parsons 2001). This cultural conflict has pitted scientists and a great many professional philosophers of science (on all sides of the realism debate) against a variety of social constructivists, historians, and social theorists of science. In many ways, this conflict has been a clash of disciplinary methods and standards, but it has also involved substantive philosophical claims about the world, with one