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PHILOSOPHY OF SCIENCE

A NEW INTRODUCTION

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To the memory of Carl Gustav (Peter) Hempel
In whose footsteps we all walk.

Preface

This book is about the philosophy of science. We know from experience that this expression sounds, to many people, almost like a contradiction in terms. What could philosophy and science have to do with one another? Philosophy seems preoccupied with profound problems that can never be resolved: the "eternal questions" of the meaning of life and the nature of knowledge and the good. Science seems precisely the opposite: cut and dried, simply concerned with concrete matters of fact. Yet science and philosophy have vitally important things to say to one another. The sciences have transformed—and continue to transform—our understanding of the world we live in and of our place in it, our history and our future; the new understanding they have given us has implications that can be felt through every branch of philosophy. On the other hand, closer scrutiny reveals that the sciences raise deep and pressing philosophical questions of their own. Scientific claims have tremendous authority in today's societies, and many of us believe that scientific inquiry is able to give us a special kind of knowledge: insight into the underlying workings of the natural world that is uniquely objective and reliable. Yet the sciences are also contested, subject to internal dispute among experts as well as to criticism from without. When public debates about any particular scientific issue become heated, the questions raised are philosophical ones about the nature, authority, and ownership of scientific knowledge. To make choices in our lives, we must each come to some conclusions about how to think about scientific controversies on issues as diverse as health risks and global climate change. At a political level, we face additional questions about how to shape public policy in response to the conflicting claims of scientists and of their critics, and about how to make choices about the direction of science itself. All of these questions require us to think philosophically about science. This book aims to show what such thinking looks like, and why it is both important and fascinating to do it.

There are many introductory books in the philosophy of science, and very high standards were set by C. G. Hempel's *Philosophy of Natural Science*, first published in 1966. For many subsequent authors, the task has seemed to be one of updating Hempel's book. We, too, see that book as a classic, but we think the time has come for a large expansion of the agenda. Developments in philosophy, in history of science, in sociology of science, in the sciences themselves, and in public reactions to them—all occurring in the past decades—have made it important to raise, and discuss, questions that Hempel and his followers put to one side. There are many books published since 1966 in which the topics of our Chapter 2 are pursued at greater depth—but, for us, that is one chapter among many.

We have had help from many other people in bringing this idea to realization. Kristin Maffei and Robert Miller have made the process as easy as anyone could hope for. Generous and perceptive reviewers helped greatly in shaping the book; we have tried to meet the challenges they set: Justin B. Biddle (Georgia Institute of Technology), Frank K. Fair (Sam Houston State University), Bruce Glymour (Kansas State University), Richard Grandy (Rice University), Michael Hoffmann (Georgia Institute of Technology), Marc Lange (UNC Chapel Hill), Nicholas Thompson (Clark University), and Michael Weisberg (University of Pennsylvania) all contributed signally. Gillian particularly thanks Jonathan Barker (for close reading and many good ideas), Nancy Barker (for unflagging support and many good dinners), and above all Dave Pearson (for everything). Philip particularly thanks Patricia Kitcher (as always an exemplary reader and the most constructive of critics).

Contents

CHAPTER 1

Science and Philosophy

Scientific Disputes and Philosophical Questions

For more than three decades now, researchers who investigate the Earth's climate have been telling the rest of the world that our planet is heating up, and that human activities are largely responsible. During the past two centuries people have released an increasing amount of carbon dioxide into the atmosphere, with the result that some of the heat that would normally dissipate is trapped. Many investigators believe that the long-term consequences for life on Earth are serious, and that the future well-being of our species is profoundly endangered. Yet although there have been periodic upsurges of interest in restricting the emission of greenhouse gases, and despite the efforts of a few nations to reduce dependence on fossil fuels, there is no coherent global strategy for responding to the supposed threat.

Why has science failed to carry the day? Why has debate about the scientific case continued? In many nations there are vocal groups who deny that the alleged facts have been established. Journalists and politicians talk of the myth of global warming; large conglomerates fund "alternative" research; apparently moderate voices point out that the specific consequences of whatever warming trend has been established are matters of dispute and that policies designed to limit emissions might plunge the world into an economic crisis having even more adverse effects on our descendants. So, it is supposed, the question should remain open.

The problem is that science alone cannot tell us how to make reasonable judgments about what is happening to the world's climates, or about how we should respond to the threat of climate change. To do this, we need good science, but we also have to face some basic questions that science can't fully answer: How exactly have the climatologists arrived at their conclusions?

What is the evidence and what does it entitle people to believe? How should we craft policies for the future when we recognize the uncertainty of our own situation? Whose interests should be taken into account and how should conflicting needs be weighed against one another? These are philosophical questions. They arise from an important problem that confronts humanity, and from the role science plays in our efforts to understand and address that problem.

Global warming is not an isolated case. Developments in the sciences often call for philosophical reflection. Consider another case, one in which scientific research is entangled with how we think about ourselves and other people. During much of recent human history people have categorized one another by race. Moreover, they have frequently operated with a view that some races are naturally—intrinsically—inferior to others. Sometimes research in the sciences has supported these claims. So, for instance, it has been asserted that intelligence is measured by scores on a particular test, that there are differences in the average scores of members of different races, and that studies of twins who have been reared apart reveal that intelligence is highly "heritable." Other scientists have disputed both the data and the interpretation offered by those who would defend deep racial differences. Some have suggested that a systematic study of the world's people reveals no basis for thinking that our species is divided into races, and that we should eliminate the concept of race entirely. Almost all would now agree that there is no evidence whatsoever for the existence of genes that have any noteworthy effect on cognitive abilities or traits of character and that are unequally distributed across the groups marked as "races." Yet recent research in molecular genetics does show that DNA sequences with no known import (bits of what is sometimes thought of as "junk DNA") are found with different frequencies in populations that have been isolated from one another for a significant period of time, so that there are "natural" divisions of *Homo sapiens* into smaller groups that share a closer kinship. Popular discussions of that research often view it as rehabilitating the notion of race.

Is that correct? What are we saying when we suppose that a particular division of the living world (or of the inorganic world) is "natural"? On what evidence are claims like this based? How should we explain the features of human psychology and behavior that fascinate us, and account for the differences across various populations?

Think about another pair of examples, not normally juxtaposed. Physicists have sometimes campaigned for public funds to build large facilities in which they hope to accelerate the weird microentities they view as the fundamental constituents of matter to speeds so high that their collisions would produce a type of particle that has been theoretically predicted but never detected. (American physicists lost in their attempt to secure government money, but their European counterparts won, and they appear now to have found their elusive target.) On a more modest scale, Freudian psychoanalysts advertise themselves as having a method, grounded in an understanding of the constituents and mechanisms of the mind, that enables them to bring relief to people with psychiatric troubles. Despite the increased popularity of drugs as remedies for psychiatric disorders, as well as the emergence of alternative forms of psychotherapy, some analysts continue to attract patients and to make a comfortable living.

In both instances, the entities that inspire various practical procedures—building huge tunnels, weekly sessions on the couch—are both strange and remote from everyday observation and detection. How can we fathom the mysteries of the Higgs boson or identify a repressed conflict between a patient and his father? Are these things part of reality, on a par with apples and oranges, rocks and radios? Or should we think of particle physics and psychoanalysis simply as practical devices, good insofar as they lead people to the goals they want to achieve, but not as making any serious claims about nature? Is there a significant difference between the two instances, and, if so, in what does it consist?

We could continue the list, but these few examples are probably enough to make the point. All over the map of contemporary science, further questions—nonscientific questions—arise. As you ponder those questions, you are led to issues that seem to lie in the province of philosophy. What is evidence, and how do we obtain it? How should people act when they can recognize that their evidence is partial? Does the world come with natural divisions, and, if so, how can they be discovered? Is it right to think of the sciences as giving a deep picture of nature, even when the things it discusses are strikingly at odds with our previous ideas about reality? Who has the authority to make scientific judgments, and why?

Overarching these questions are even more general ones. Are the natural sciences the uniquely best sources of human knowledge, setting standards that ought to be achieved in all fields of inquiry? Do they constitute just one of many ways of thinking about ourselves and the world that are good in different ways or that serve different purposes? Do they threaten our understanding of ourselves, presenting a limited or distorting vision of the world and our place in it?

The philosophy of science, as we understand it, consists in an attempt to answer—or, at least, discuss—these questions, both the more specific ones and those that are most general. This book is an introduction to it.

Modern Science: A Brief History

Let us start more slowly and more systematically. What are we talking about when we talk about "science"?

We start with examples. The natural sciences include physics, chemistry, biology, earth science, atmospheric science, oceanography, neuroscience, and at least some parts of psychology. We exclude mathematics because its ways of establishing its conclusions seem so different—most obviously, mathematicians don't appear to rely on observation and experimentation. We mostly leave out applied sciences (like metallurgy), engineering, and technology, although it is not always easy to separate these forms of inquiry from "pure" (or "basic") science. The social sciences (economics, anthropology, linguistics, etc.) pose some of the same issues as those that arise for the natural sciences, but also bring distinctive questions of their own that we do not pursue. As we shall see, defining what exactly makes a field of investigation count as a science is itself a task that raises substantial philosophical questions—these are among the main concerns of Chapter 2. It is clear nonetheless that the natural sciences on our list share features that make it reasonable to treat them together, and that they raise important and interesting common issues that philosophy can usefully address. The resulting investigation ultimately sheds some light on work in mathematics, the applied sciences, and the social sciences as well.

Current work in the natural sciences can be traced to inquiries that were pursued in ancient Greece. The word *science* is, however, a recent coinage, introduced with its present meaning only in the nineteenth century. Before that, the investigations that aimed at general and systematic explanations of nature were known as natural philosophy; more particular descriptive studies were parts of natural history. Even in the twentieth century, undergraduates at Cambridge University studying science were prepared for exams in natural philosophy. The enduring label testifies to an old intertwining of science and philosophy.

The practices of the sciences as we now know them have been profoundly shaped by the events of the seventeenth century. Galileo and Kepler, Bacon and Descartes, and Boyle and Newton were the most prominent figures in a transition that is often known as the Scientific Revolution (although some historians of science maintain that that label suggests a change more abrupt and complete than the reality). It is hard to deny that a fundamental shift occurred. To approach some of the questions we will be discussing, and to appreciate the appeal of particular answers to them, it is necessary to have some understanding of what happened.

At the beginning of the sixteenth century, most Europeans shared a picture of the cosmos drawn from the works of Aristotle and interpreted from a Christian perspective. According to this picture, the Earth was located at the center of the universe, and the moon, planets, sun, and stars revolved around it. Earth itself, and all objects found on and near its surface, were composed of four elements: earth, air, fire, and water. Each of these elements had different qualities that helped explain the properties of the objects they composed. Earthly objects were subject to change and decay. The stars, planets, and other objects in the heavens were composed of a special fifth substance, quintessence; they were eternal, flawless, and unchanging.

Events were explained in terms of causes, but among the causes that were deemed important for explanation and understanding were some of a special sort. Each kind of object was thought to have a "final cause" or *telos*, something like a goal or purpose, determining the changes and motions it naturally tends to undergo. Objects could be made to move unnaturally or "violently," but their natural motions were the basis of the regularities we observe and it was important to identify them in providing a general explanation. Earthly objects moved naturally in straight lines toward or away from the center of the universe, at the center of the Earth. The natural motion of heavenly bodies was circular, for (as Greek philosophers had supposed) the circle is the simplest and most perfect geometrical figure, and circular motion can continue eternally. Some heavenly bodies moved in simple circles around the center of the universe, but others were thought to move on paths compounded of various circular movements (this concession was made to account for the complex movements of the planets against the background of the "fixed stars"). The mathematical structure of the heavens had been worked out in detail by Ptolemy in the second century. Sometimes this geocentric universe was understood as a real physical structure of nested rotating crystalline spheres on which the various heavenly bodies were borne. Ptolemy's system required a great many compound movements (known as epicycles: see Figure 1.1), however, which made such a realistic interpretation difficult to maintain. The Ptolemaic system was often thought of as a mere calculating device for generating predictions, not as a representation of real objects and causes.

Inquiry into the natural world was guided by the writings of earlier scholars, in particular those of Aristotle and the theologians who had integrated his work with Christian doctrine. Investigation began with a careful exegesis of the views of these authorities, and sought to answer questions in

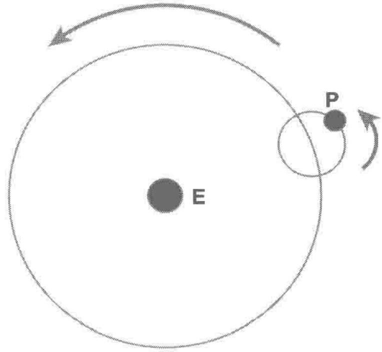

Figure 1.1 Epicycles in the Ptolemaic system.

ways that agreed with those views as properly understood. Empirical observations were recruited to assist such investigations, but qualitative observations made in everyday circumstances or reference to familiar observable phenomena were sufficient for the purposes of natural philosophy, and no special techniques or quantitative measurements were called for.

The sixteenth century began to challenge this picture of the world and the associated ideas about proper investigation. Trouble began quite accidentally. A Polish monk, Nicolaus Copernicus, was asked by the Papacy to develop a better model of the motions of the heavenly bodies—it was evident that the calendar needed reform, and improved astronomy was the key to solving the practical problem. Copernicus labored for more than thirty years, ultimately producing an astronomical system that displaced the Earth from the center of the universe. It was clear from the start that the new model was easier to work with than the traditional versions—and also evident that for any heliocentric system there was a geocentric system that would deliver exactly the same predictions for the planetary motions (although the geocentric models would be more cumbersome). Orthodox astronomy embraced Copernicus by treating the new system as a mere device for making astronomical calculations. That way embarrassing questions could be dodged: How did the birds and clouds keep up with a moving Earth? What exactly happened in the biblical event when Joshua commanded the sun to stand still? But a very few of Copernicus's successors wondered if the heterodox ideas of the new astronomy might be literally true—if perhaps, after all, the Earth did really move. Early in the seventeenth century, Galileo's observations of the moon and planets with the new telescope provided reason to think Copernicus had correctly represented the real structure of the universe.

Galileo's contemporary, Johann Kepler, another one of the rare Copernicans, struggled to find a more accurate representation of the motions of the planets, eventually replacing the perfect circles with elliptical orbits. After Galileo and Kepler, many thinkers came to view the Earth not as the center around which the universe was built, but merely as one planet among many. They also thought in novel ways about the causes of change and the nature of explanation. Aristotle's distinction between natural and violent motion was replaced with the idea that motion is the same everywhere—in Earth as it is in Heaven—and governed everywhere by the same natural laws. Eventually, toward the end of the seventeenth century, Isaac Newton's mathematical laws of motion and of gravitation revealed how the whole system worked.

At the same time, investigators in other areas were developing a new account of matter. Explanations of the properties of substances in terms of the four Aristotelian elements gave way to mechanistic theories that attributed the qualities of objects to mechanical properties (shape and motion) of their component parts, often conceived as indivisible atoms. The cosmos of the

Middle Ages, small and intimate, filled with purpose and value, centered around the uniquely important Earth with its human inhabitants, was replaced by a universe understood as analogous to a vast clock, with an order that was grand and comprehensible but wholly impersonal.

Investigators who turned to the authority of their own reason and senses challenged the reliance on tradition. Galileo and Bacon championed the importance of making observations and experiments. More radically, Descartes, troubled perhaps by the breakdown of Aristotelian ideas that had dominated inquiry for two millennia, urged that all inherited assumptions should be questioned. Investigations should begin anew on the basis of principles immune from any possible doubt. This shaking off of orthodox ideas—the "Doctrines of the Schools"—echoed the questioning of church authority that had begun in the Renaissance and Reformation. It began a division within intellectual life: With respect to theology, the traditional methods were favored and the authority of religious scholars was preserved, but for the study of nature, observation and reason were to hold sway.

The pioneers of the new sciences thought that their investigations could disclose systematic patterns underlying the diverse phenomena of the natural world. Those patterns were expressed in quantitative laws: In Galileo's famous phrase, the book of Nature is "written in the language of mathematics." How could the patterns be discovered, and the book read? A popular thought, articulated by Bacon, practiced and adapted by Boyle, and written into the key documents of the early Royal Society, recommended starting with the collection of observations, and generalizing from the connections found in the observed samples. Newton claimed to reject all conjectures, and to base his famous laws on systematic study of the phenomena and careful inferences from them: beginning with observations he saw himself as "making them general by induction" (*induction* being the approved term for inferences that generalize from a sample).

The practice of doing experiments took on a new role. In contrast to the Aristotelian tradition, which regarded ordinary circumstances as revealing natural processes most clearly and interventions as distorting the natural course of things, the new thinkers believed that the special circumstances created by experimentation played a special role in illuminating the underlying structure of the natural world. New quantitative experimental methods encouraged a focus on properties that could be readily controlled and measured. Slowly, a world of qualities and purposes gave way to a world with fundamental properties that were quantifiable and measurable.

New social and institutional organizations were created to support the emerging methods of inquiry. In seventeenth-century England, France, and Italy, societies were founded to foster the exchange of information and to support and evaluate work in the new sciences. These societies began to standardize the form of scientific publications and the professional accreditation of practitioners, creating conditions in which researchers could trust the reports of others beyond their own social circle—a process that eventually led to the institutional structure so crucial to the functioning of professional science today.

We have given a whirlwind sketch, and historians would want to add many details, some of which would recognize continuities between the "new science" and inquiries in the Middle Ages or show how older positions sometimes persisted in the thought of those who most prided themselves on having overcome the past. Yet even if the niceties were acknowledged, two important points for our future discussions would remain. First, what we now think of as science, an enterprise with a distinctive set of approaches and fundamental conceptions, was not always the principal form of inquiry. It replaced an alternative approach to knowledge, focused on different issues—questions about purpose, value, and human salvation. Second, the features of that new enterprise emerged in a particular historical context, in connection with a particular domain of investigation. The pioneers responded to particular deficiencies in the system of knowledge that was taught in the universities of the day, and they elaborated their views of a replacement by beginning with the physics of motion. It is worth pondering those points if you think that science is the best, or even the only possible, form of human knowledge, or if you believe that certain of its features (e.g., reliance on experimentation or use of mathematics) are constitutive of anything worthy of the name "science."

Images of Science

In the chapters that follow, we consider the ways in which philosophers (and sometimes thinkers from other fields: historians of science and sociologists of science, for example) have tried to elaborate general conceptions of science—"images of science" as we call them. Many of those images are popular among scientists (although scientists tend to be unworried about the details of their favorite image), and they are prominent in newspaper discussions of science, as well as in books written for the general public. You are probably familiar with most of them.

The images are often taken to describe how science actually is. When someone (historian, sociologist, or journalist) discovers that a piece of work fails to fit the preferred image, though, there is often a significant shift in perspective. The image is no longer seen as *descriptive*, but as *normative*: This is how science *should be*. Despite this shift, a connection with description usually remains. The problematic work is a deviation from the proper course of scientific activity, a course taken to be exemplified in the overwhelming majority of scientific investigations.