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Introduction

1.1 Setting Out

This book is a survey of roughly one hundred years of argument about the nature of science. We'll look at a hundred years of argument about what science is, how it works, and what makes science different from other ways of investigating the world. Most of the ideas we will examine fall into the field called "philosophy of science," but we will also spend a good deal of time looking at ideas developed by historians, sociologists, psychologists, and others.

The book mostly has the form of a "grand tour" through the decades; ideas will be discussed in roughly the order in which they appeared. Note the word "roughly" in the previous sentence; there are exceptions to the historical structuring of the book, and I will point out some of them as they arise.

Why is it best to start with older ideas and work through to the present? One reason is that the historical development of general ideas about science is itself an interesting topic. Another reason is that the philosophy of science has been in a state of fermentation and uncertainty in recent years. A good way to understand the maze of options and opinions in the field at the moment is to trace the path that brought us to the state we're in now. But this book does not only aim to introduce the options. I will often take sides as we go along, trying to indicate which developments were probably wrong turns or red herrings. Other ideas will be singled out as being on the right track. Then toward the end of the book, I will start trying to put the pieces together into a picture of how science works.

Philosophy is an attempt to ask and answer some very basic questions about the universe and our place within it. These questions can sometimes seem far removed from practical concerns. But the debates covered in this book are not of that kind. Though these debates are connected to the most abstract questions about thought, knowledge, language, and reality, they have also turned out to have an importance that extends well outside of philosophy. They have made a difference to developments in many other academic fields, and some of the debates have reverberated much further, affecting discussions of education, medicine, and the proper place of science in society.

In fact, throughout the latter part of the twentieth century, all the fields concerned with the nature of science went on something of a roller-coaster ride. Some people thought that work in the history, philosophy, and sociology of science had shown that science does not deserve the dominating role it has acquired in Western cultures. They thought that a set of myths about the trustworthiness and superiority of mainstream science had been thoroughly undermined. Others disagreed, of course, and the resulting debates swirled across the intellectual scene, frequently entering political discussion as well. From time to time, scientific work itself was affected, especially in the social sciences. These debates came to be known as the "Science Wars," a phrase that conveys a sense of how heated things became.

The Science Wars eventually cooled down, but now, as I write these words, it is fair to say that there is still a great deal of disagreement about even the most basic questions concerning the nature and status of scientific knowledge. These disagreements usually do not have much influence on the day-to-day practice of science, but sometimes they do. And they have huge importance for general discussions of human knowledge, cultural change, and our overall place in the universe. This book aims to introduce you to this remarkable series of debates, and to give you an understanding of the present situation.

1.2 The Scope of the Theory

If we want to understand how science works, it seems that the first thing we need to do is work out what exactly we are trying to explain. Where does science begin and end? Which kinds of activity count as "science"?

Unfortunately this is not something we can settle in advance. There is a lot of disagreement about what counts as science, and these disagreements are connected to all the other issues discussed in this book.

There is consensus about some central cases. People often think of physics as the purest example of science. Certainly physics has had a heroic history and a central role in the development of modern science. Molecular biology, however, is probably the science that has developed most rapidly and impressively over the past fifty years or so.

These seem to be central examples of science, though even here we encounter hints of controversy. A few have suggested that theoretical physics is becoming less "scientific" than it used to be, as it is evolving into an esoteric, mathematical model–building exercise that has little contact with the real world (Horgan 1996). And molecular biology has recently been acquiring connections with business and industry that make it, in the eyes of some, a less exemplary science than it once was. Still, examples like these give us a natural starting point. The work done by physicists and molecular biologists when they test hypotheses is science. And playing a game of basketball, no matter how well one plays, is not doing science. But in the area between these clear cases, disagreement reigns.

At one time the classification of economics and psychology as sciences was controversial. Those fields have now settled into a scientific status, at least within the United States and similar countries. (Economics retains an amusing qualifier; it is often called "the dismal science," a phrase due to Thomas Carlyle.) There is still a much-debated border region, however, and at the moment this includes areas like anthropology and archaeology. At Stanford University, where I teach, this kind of debate was one element of a process in which the Department of Anthropology split into two separate departments. Is anthropology, the general study of humankind, a fully scientific discipline that should be closely linked to biology, or is it a more "interpretive" discipline that should be more closely connected to the humanities?

The existence of this gray area should not be surprising, because in contemporary society the word "science" is a loaded and rhetorically powerful one. People will often find it a useful tactic to describe work in a borderline area as "scientific" or as "unscientific." Some will call a field scientific to suggest that it uses rigorous methods and hence delivers results we should trust. Less commonly, but occasionally, a person might call an investigation scientific in order to say something negative about it—to suggest that it is dehumanizing, perhaps. (The term "scientistic" is more often used when a negative impression is to be conveyed.) Because the words "science" and "scientific" have these rhetorical uses, we should not be surprised that people constantly argue back and forth about which kinds of intellectual work count as science.

The history of the term "science" is also relevant here. The current uses of the words "science" and "scientist" developed quite recently. The word "science" is derived from the Latin word "scientia." In the ancient, medieval, and early modern world, "scientia" referred to the results of logical demonstrations that revealed general and necessary truths. Scientia could be gained in various fields, but the kind of proof involved was what we would now mostly associate with mathematics and geometry. Around the seventeenth century, when modern science began its rise, the fields that we would now call science were more usually called "natural philosophy" (physics, astronomy, and other inquiries into the causes of things) or "natural history" (botany, zoology, and other descriptions of the contents of the world). Over time, the term "science" came to be used for work with closer links to observation and experiment, and the association between science and an ideal of conclusive proof receded. The current senses of the term "science" and the associated word "scientist" are products of the nineteenth century.

Given the rhetorical load carried by the word "science," we should not expect to be able to lay down, here in chapter 1, an agreed-on list of what is included in science and what is not. For now we will have to let the gray area remain gray.

A further complication comes from the fact that philosophical (and other) theories differ a lot in how broadly they conceive of science. Some writers use terms like "science" or "scientific" for any work that assesses ideas and solves problems in a way guided by observational evidence. Science is seen as something found in all human cultures, even though the word is a Western invention. But there are also views that construe "science" more narrowly, seeing it as a cultural phenomenon that is localized in space and time. For views of this kind, it was only the Scientific Revolution of the sixteenth and seventeenth centuries in Europe that gave us science in the full sense. Before that, we find the initial "roots" or precursors of science in ancient Greece, some contributions from the Arab world and from the Scholastic tradition in the late Middle Ages, but not much else. So this is a view in which science is treated as a special social institution with a definite history. Science is something that descends from specific people and places, and especially from a key collection of Europeans, including Copernicus, Kepler, Galileo, Descartes, Boyle, and Newton, who all lived in the sixteenth and seventeenth centuries.

To set things up this way is to see science as *unlike* the kinds of investigation and knowledge that routinely go along with farming, architecture, and other kinds of technology. So a view like this need *not* claim that people in nonscientific cultures must be ignorant or stupid; the idea is that in order to understand *science,* we need to distinguish it from other kinds of investigation of the world. And we need to work out how one approach to knowledge developed by a small group of Europeans turned out to have such spectacular consequences for humanity.

As we move from theory to theory in this book, we will find some people construing science broadly, others narrowly, and others in a way that lies in between. But this does not stop us from outlining, in advance, what kind of understanding we would eventually like to have. However we choose to use the word "science," in the end we should try to develop *both*

1. a general understanding of how humans gain knowledge of the world around them *and*
2. an understanding of what makes the work descended from the Scientific Revolution *different* from other kinds of investigation of the world.

We will move back and forth between these two kinds of questions throughout the book.

Before leaving this topic, there is one other possibility that should be mentioned. How confident should we be that all the work we call "science," even in the narrower sense described above, has that much in common? One of the hazards of philosophy is the temptation to come up with theories that are too broad and sweeping. "Theories of science" need to be scrutinized with this problem in mind.

1.3 What Kind of Theory?

This book is an introduction to the philosophy of science. But most of the book focuses on one set of issues in that field. Within the philosophy of science, we can distinguish between *epistemological* issues and *metaphysical* issues (as well as issues that fall into neither category). Epistemology is the side of philosophy that is concerned with questions about knowledge, evidence, and rationality. Metaphysics, a more controversial part of philosophy, deals with general questions about the nature of reality. Philosophy of science overlaps with both of these.

Most of the issues discussed in this book are, broadly speaking, epistemological issues. For example, we will be concerned with questions about how observational evidence can justify a scientific theory. We will also ask whether we have reason to hope that science can succeed in describing the world "as it really is." But we will occasionally encounter metaphysical issues, and issues in the philosophy of language. The discussion will intersect with work in the history of science and other fields as well.

All of philosophy is plagued with discussion and anxiety about how philosophical work should be done and what a philosophical theory should try to do. So we will have to deal with disagreement about the right *form* for a philosophical theory of science, and disagreement about which questions philosophers should be asking. One obvious possibility is that we might try for an understanding of scientific *thinking*. In the twentieth century, many philosophers rejected this idea, insisting that we should seek a *logical* theory of science. That is, we should try to understand the abstract structure of scientific theories and the relationships between theories and evidence. A third option is that we should try to come up with a *methodology,* a set of rules or procedures that scientists do or should follow. In more recent years, philosophers influenced by historical work have wanted to give a general theory of scientific *change.*

A distinction that is very important here is the distinction between *descriptive* and *normative* theories. A descriptive theory is an attempt to describe what actually goes on, or what something is like, without making value judgments. A normative theory does make value judgments; it talks about what should go on, or what things should be like. Some theories about science are supposed to be descriptive only. But most of the views we will look at do have a normative element, either officially or unofficially. When assessing general claims about science, it is a good principle to constantly ask: "Is this claim intended to be descriptive or normative, or both?"

For some people, the crucial question we need to answer about science is whether or not it is "objective." But this term has become an extremely slippery one, used to mean a number of very different things. Sometimes objectivity is taken to mean the absence of bias; objectivity is impartiality or fairness. But the term "objective" is also often used to express claims about whether the *existence* of something is independent of our minds. A person might wonder whether there really is an "objective reality," that is to say, a reality that exists regardless of how people conceptualize or describe it. We might ask whether scientific theories can ever describe a reality that exists in this sense. Questions like that go far beyond any issue about the absence of bias and take us into deep philosophical waters.

Because of these ambiguities, I will often avoid the terms "objective" and "objectivity." But the questions that tend to be asked using those terms will be addressed, using different language, throughout the book. And I will return to "objectivity" in the final chapter.

Another famous phrase is "scientific method." Perhaps this is what most people have in mind when they imagine giving a general theory of science. The idea of describing a special method that scientists do or should follow is old. In the seventeenth century, Francis Bacon and René Descartes, among others, tried to give detailed specifications of how scientists should proceed. Although describing a special scientific method looks like a natural thing to try to do, during the twentieth century many philosophers and others became skeptical about the idea of giving anything like a recipe for science. Science, it was argued, is too creative and unpredictable a process for there to be a recipe that describes it—this is especially true in the case of great scientists like Newton, Darwin, and Einstein. For a long time it was common for

science textbooks to have an early section describing "the scientific method," but recently textbooks seem to have become more cautious about this.

I said that much twentieth-century philosophy of science aimed at describing the *logical* structure of science. What does this mean? The idea is that the philosopher should think of a scientific theory as an abstract structure, something like a set of interrelated sentences. The philosopher aims to give a description of the logical relations between the sentences in the theory and the relations between the theory and observational evidence. Philosophy can also try to describe the logical relations between different scientific theories in related fields.

Philosophers taking this approach tend to be enthusiastic about the tools of mathematical logic. They prize the rigor of their work. This kind of philosophy has often prompted frustration in people working on the actual history and social structure of science. The crusty old philosophers seemed to be deliberately removing their work from any contact with science as it is actually conducted, perhaps in order to hang onto a set of myths about the perfect rationality of the scientific enterprise, or in order to have nothing interfere with the endless games that can be played with imaginary theories expressed in artificial languages. This kind of logic-based philosophy of science will be discussed in the early chapters of this book. I will argue that the logical investigations were often very interesting, but ultimately my sympathy lies with those who insist that philosophy of science should have more contact with actual scientific work.

If looking for a recipe is too simplistic, and looking for a logical theory is too abstract, what might we look for instead? Here is an answer that will be gradually developed as the book goes on: we can try to describe the scientific *strategy* for investigating the world. And we can then hope to describe what sort of *connection* to the world we are likely to achieve by following that strategy. Initially, this may sound vague or impossible, or both. But by the end of the book I hope to show that it makes good sense.

Several times now I have mentioned fields that "neighbor" on philosophy of science—history of science, sociology of science, and parts of psychology, for example. What is the relation between philosophical theories of science and ideas in these neighboring fields? This question was part of the twentieth-century roller-coaster ride that I referred to earlier. Some people in these neighboring fields thought they had reason to believe that the whole idea of a philosophical theory of science is misguided. They expected that philosophy of science would be replaced by fields like sociology. This replacement never occurred. What did happen was that people in these neighboring fields constantly found themselves doing philosophy themselves, whether they realized it or not. They kept running into questions about truth, about justification, and about the connections between theories and reality. The philosophical problems refused to go away.

Philosophers themselves differ a great deal about what kind of input from these neighboring fields is relevant to philosophy. This book is written from a viewpoint that holds that philosophy of science benefits from lots of input from other fields. But the argument that philosophy of science needs that kind of input will not be given until chapter 10.

1.4 Three Answers, or Pieces of an Answer

In this section I will introduce three different answers to our general questions about how science works. In different ways, these three ideas will be recurring themes throughout the book.

The three ideas can be seen as rivals; they can be seen as alternative starting points, or paths into the problem. But they might instead be considered as pieces of a single, more complicated answer. The problem then becomes how to fit them together.

The first of the three ideas is *empiricism*. Empiricism encompasses a diverse family of philosophical views, and debates within the empiricist camp can be intense. But empiricism is often summarized using something like the following slogan:

Empiricism: The only source of real knowledge about the world is experience.

Empiricism, in this sense, is a view about where *all* knowledge comes from, not just scientific knowledge. So how does this help us with the philosophy of science? In general, the empiricist tradition has tended to see the differences between science and everyday thinking as differences of *detail and degree*. The empiricist tradition has generally, though not always, tended to construe science in a broad way, and it has tended to approach questions in the philosophy of science from the standpoint of a general theory of thought and knowledge. The empiricist tradition in philosophy has also been largely *pro*-science; science is seen as the best manifestation of our capacity to investigate and know the world.

So here is a way to use the empiricist principle above to say something about science:

Empiricism and Science: Scientific thinking and investigation have the same basic pattern as everyday thinking and investigation. In each case, the only source of real knowledge about the world is experience. But science is especially successful because it is organized, systematic, and especially responsive to experience.

So "the scientific method," insofar as there is such a thing, will be routinely found in everyday contexts as well. There was no fundamentally *new* approach to investigation discovered during the Scientific Revolution, according to this view. Instead, Europe was freed from darkness and dogmatism by a few brave and brilliant souls who enabled intellectual culture to "come to its senses."

Some readers are probably thinking that these empiricist principles are empty platitudes. *Of course* experience is the source of knowledge about the world—what *else* could be?

For those who suspect that basic empiricist principles are completely trivial, an interesting place to look is the history of medicine. The history of medicine has many examples of episodes where *huge* breakthroughs were made by people willing to make very basic empirical tests—in the face of much skepticism, condescension, and opposition from people who "knew better." Empiricist philosophers have long used these anecdotes to fire up their readers. Carl Hempel, one of the most important empiricist philosophers of the twentieth century, liked to use the sad example of Ignaz Semmelweiss (see Hempel 1966). Semmelweiss worked in a hospital in Vienna in the mid-nineteenth century; he was able to show by simple empirical tests that if doctors washed their hands before delivering babies, the risk of infection in the mothers was hugely reduced. For this radical claim he was opposed and eventually driven out of the hospital.

An even simpler example, which I will describe in some detail to provide a change from the usual case of Semmelweiss, has to do with the discovery of the role of drinking water in the transmission of cholera.

Cholera was a huge problem in cities in the eighteenth and nineteenth centuries, producing death from terrible diarrhea. Cholera is still a problem whenever there are poor people crowded together without good sanitation, as it is transmitted from the diarrhea through drinking water. In the eighteenth and nineteenth centuries, there were various theories of how cholera was caused—this was before the discovery of the role of bacteria and other microorganisms in infectious disease. Some thought the disease was caused by foul gases, called *miasmas*, exuded from the ground and swamps. In London, John Snow hypothesized that cholera was spread by drinking water. He mapped the outbreak of one epidemic in London in 1854 and found that it seemed to be centered on a particular public water pump in Broad Street. With great difficulty he persuaded the local authorities to remove the pump's handle. The outbreak immediately went away.

This was a very important event in the history of medicine. It was central to the rise of the modern emphasis on clean drinking water and sanitation, a movement that has had an immense effect on human health and well-being. This is also the kind of case that shows the attractiveness of even very simple empiricist views.

You might be thinking that we can just end the book here. Empiricism wins; looking to experience is a sure-fire guarantee of getting things right. Those who are tempted to think that no problems remain might consider a cautionary tale that follows up the Snow story. This is the tale of brave Doctor Pettenkofer.

Some decades after Snow, the theory that diseases like cholera are caused by microorganisms—the "germ theory of disease"—was developed in detail by Robert Koch and Louis Pasteur. Koch isolated the bacteria responsible for cholera quite early on. Pettenkofer, however, was unconvinced. To prove Koch wrong, he *drank* a glass of water mixed with the alleged cholera germs. Pettenkofer suffered no ill effects, and he wrote to Koch saying he had disproved Koch's theory.

It is thought that Pettenkofer might have had high stomach acid, which can protect people against cholera infection. Or perhaps the cholera germs had died in that sample. Clearly Pettenkofer was lucky; Koch was right about what causes cholera. But the case reminds us that direct empirical tests are no *guarantee* of success.

Some readers, I said, might be thinking that empiricism is true but too obvious to be interesting. Another line of criticism holds that empiricism is false, because it is committed to an absurdly simple picture of thought, belief, and justification. The empiricist slogan I gave earlier suggests that experiences pour into the mind and somehow turn into knowledge. It turns out to be very difficult to refine basic empiricist ideas in a way that makes them more psychologically realistic. Empiricists do not deny that reasoning, including very elaborate reasoning, is needed to make sense of what we observe. Still, they insist that the role of experience is somehow fundamental in understanding how we learn about the world. Many critics of empiricism hold that this is a mistake; they see it as a hangover from a simplistic and outdated picture of how belief and reasoning work. That debate will be a recurring theme in this book.

I now turn to the second of the three families of views about how science works. This view can be introduced with a quote from Galileo, one of the superheroes of the Scientific Revolution:

> Philosophy is written in this grand book the universe, which stands continually open to our gaze. But the book cannot be understood unless one first learns to comprehend the language and to read the alphabet in which it is composed. *It is written in the language of mathematics,* and its characters are triangles, circles, and other geometric figures without which it is humanly impossible to understand a single word of it; without these, one wanders about in a dark labyrinth. (Galileo [1623] 1990, 237–38, emphasis added)

Putting the point in plainer language, here is the second of the three ideas.

> *Mathematics and Science:* What makes science different from other kinds of investigation, and especially successful, is its attempt to understand the natural world using mathematical tools.

Is this idea an alternative to the empiricist approach, or something that can be combined with it? Perhaps surprisingly, an emphasis on mathematical methods has often been used to argue against empiricism. Sometimes this has been because people have thought that mathematics shows us that there must be another route to knowledge beside experience; experience is *a* source of knowledge, but not the *only* important source. Alternatively, we might claim that empiricism is trivial: of course knowledge is based on experience, but that tells us nothing about what differentiates science from other human thought. What makes science special is its attempt to quantify phenomena and detect mathematical patterns in the flow of events.

Nonetheless, it is surely sensible to see an emphasis on mathematics as something that can be combined with empiricist ideas. It might seem that Galileo would disagree; Galileo not only exalted mathematics but praised his predecessor Copernicus for making "reason conquer sense [experience]" in his belief that the earth goes around the sun. But this is a false opposition. In suggesting that the earth goes round the sun, Copernicus was not ignoring experience but dealing with apparent conflicts between different aspects of experience, in the light of his background beliefs. And there is no question that Galileo was a very empirically minded person; an emphasis on observations made using the telescope was central to his work, for example. So avoiding the false oppositions, we might argue that mathematics used *as a tool within an empiricist outlook* is what makes science special.

In this book the role of mathematics will be a significant theme but not a central one. This is partly because of the history of the debates surveyed in the book, and partly because mathematical tools are not quite as essential to science as Galileo thought. Although mathematics is clearly of huge importance in the development of physics, one of the greatest achievements in all of science—Darwin's achievement in *On the Origin of Species* ([1859] 1964)—makes no real use of mathematics. Darwin was not confined to the "dark labyrinth" that Galileo predicted as the fate of nonmathematical investigators. In fact, most (though not all) of the huge leaps in biology that occurred in the nineteenth century occurred without much of a role for mathematics. Biology *now* contains many mathematical parts, including modern formulations of Darwin's theory of evolution, but this is a more recent development.

So not all of science—and not all of the greatest science—makes much use of mathematics to understand the world.

The third of the three families of ideas is newer. Maybe the unique features of science are only visible when we look at scientific *communities*.

Social Structure and Science: What makes science different from other kinds of investigation, and especially successful, is its unique social structure.

Some of the most important recent work in philosophy of science has had to do with exploring this idea, but it took the input of historians and sociologists of science to bring philosophical attention to bear on it.

In the hands of historians and sociologists, an emphasis on social structure has often been developed in a way that is strongly critical of the empiricist tradition. Steven Shapin argues that mainstream empiricism often operates within the fantasy that each individual can observationally test hypotheses for himself (Shapin 1994). Empiricism is supposed to urge that people be distrustful of authority and go out to look directly at the world. But of course this is a fantasy. It is a fantasy in the case of everyday knowledge, and it is an even greater fantasy in the case of science. Almost every move that a scientist makes depends on elaborate networks of cooperation and trust. If each individual insisted on testing everything himself, science would never advance beyond the most rudimentary ideas. Cooperation and lineages of transmitted results are essential to science. The case of John Snow and cholera, discussed earlier in this section, is very unusual. Snow looks like a "lone ranger" striding up to the Broad Street water pump (with crowds of empiricists cheering in the background). And even Snow must have been dependent on the testimony of others in his assessment of the state of the cholera epidemic before and after his intervention at the pump.

So trust and cooperation are essential to science. But who can be trusted? Who is a reliable source of data? Shapin argues that when we look closely, a great deal of what went on in the Scientific Revolution had to do with working out new ways of policing, controlling, and coordinating the actions of groups of people in the activity of research. Experience is everywhere. The hard thing is working out which *kinds* of experience are relevant to the testing of hypotheses, and working out who can be trusted as a source of reliable and relevant reports.

So Shapin argues that a good theory of the social organization of science will be a better *theory of science* than empiricist fantasies. But philosophers have begun to develop theories of how science works that emphasize social organization but are also intended to fit in with a form of empiricism (Hull 1988; Kitcher 1993). These accounts of science stress the special balance of cooperation and competition found in scientific communities. People sometimes imagine that seeking individual credit and competition for status and recognition are recent developments in science. But these issues have been important since the time of the Scientific Revolution. The great scientific societies, like the Royal Society of London, came into being quite early—1660 in the case of the Royal Society. A key part of their role was to handle the allocation of credit in an efficient way—making sure the right people were rewarded, without hindering the free spread of ideas. These societies also functioned to create a community of people who could trust each other as reliable co-workers and sources of data. The empiricist can argue that this social organization made scientific *communities* uniquely responsive to experience.

In this section I have sketched three families of ideas about how science works and what makes it distinctive. Each idea has sometimes been seen as *the* starting point for an understanding of science, exclusive of the other two. But it is more likely that they should be seen as pieces of a more complete answer. The first and third ideas—empiricism and social structure—are especially important. These we will return to over and over again. Part of the challenge for philosophy of science in the years to come lies in integrating the insights of the empiricist tradition with the role for social organization in understanding science. That does require significant changes to traditional empiricist ideas.

1.5 Historical Interlude: A Sketch of the Scientific Revolution

Before diving into the philosophical theories, we will take a brief break. Several times already I have mentioned the Scientific Revolution. People, events, and theories from this period carry special weight in discussions of the nature of science. So in this section I will give a historical sketch of the main landmarks, many of which will appear from time to time in later chapters. Before setting out, I should note that there is a good deal of controversy about how to understand this period of history; for example, some historians think that the whole idea of christening this period "The Scientific Revolution" is a mistake, as this phrase makes it sound like there are sharp boundaries between one totally unique period and the rest of history (Shapin 1996). But I will use the phrase in the traditional way.

The Scientific Revolution occurred roughly between 1550 and 1700. These events are positioned at the end of a series of dramatic changes in

Europe, and the Scientific Revolution itself fed into further processes of change. In religion, the Catholic Church had been challenged by Protestantism. The Renaissance of the fifteenth and sixteenth centuries had included a partial opening of intellectual culture. Populations were growing (recovering from the Black Death), and there was increased activity in commerce and trade. Traditional hierarchies, including intellectual hierarchies, were beginning to show strain. As recent writers have stressed, this was a time in which many new, unorthodox ideas were floating around.

The worldview that had been inherited from the Middle Ages was a combination of Christianity with the ideas of the ancient Greek philosopher Aristotle. The combination is often called the Scholastic worldview, after the universities or "Schools" that developed and defended it. The earth was seen as a sphere positioned at the center of the universe, with the moon, sun, planets, and stars revolving around it. A detailed model of the motions of these celestial bodies had been developed by Ptolemy around 150 A.D. (the sun was placed between Venus and Mars).

Aristotle's physical theory distinguished "natural" from "violent" or unnatural motion. The theory of natural motions was part of a more general theory of change in which biological development (from acorn to oak, for example) was a central guiding case, and many events were explained using the idea of *purpose.*

Everything on earth was considered to be made up of mixtures of four basic elements (earth, air, fire, and water), each of which had natural tendencies. Objects containing a lot of earth, for example, naturally fall toward the center of the universe, while fire makes things rise. Unnatural motions, such as the motions of projectiles, have an entirely different kind of explanation. Objects in the heavens are made of a fifth element, which is "incorruptible," or unchanging. The natural motion for objects made of this fifth element is circular.

Some versions of this picture included a mechanism (using the term loosely) for the motions of sun, planets, and stars. For example, each body orbiting the earth might be positioned on a crystalline sphere that revolved around the earth. Ptolemy's own model was harder to interpret in these terms; Ptolemy is sometimes thought to be most interested in giving a tool for astronomical prediction (though interpreters differ on this).

In 1543 the Polish astronomer Nicolaus Copernicus (1473–1543) published a work outlining an alternative picture of the universe. Others had speculated in ancient times that the earth might move around the sun instead of vice versa, but Copernicus was the first to give a detailed theory of this kind. In his theory the earth has two motions, revolving on its axis once a day and orbiting the sun once a year. Copernicus's theory had the same basic placement of the sun, moon, earth, and the known planets that modern astronomy has. But the theory was made more complicated by his insistence, following Aristotle and Ptolemy, that heavenly motions must be circular. Both the Ptolemaic system and Copernicus's system saw most orbits as complex compounds of circles, not single circles. Ptolemy's and Copernicus's systems were about equally complicated, in fact. Writers seem to differ on whether Copernicus's theory was much more accurate as a predictive tool. But there were some famous phenomena that Copernicus's theory explained far better than Ptolemy's. One was the "retrograde motion" of the planets, an apparently erratic motion in which planets seem to stop and backtrack in their motions through the stars.

Copernicus's work aroused interest, but there seemed to be compelling arguments against taking it to be a literally true description of the universe. Some problems were astronomical, and others had to do with obvious facts about motion. Why does an object dropped from a tower fall at the foot of the tower, if the earth has moved a considerable distance while the object is in flight? Copernicus's 1543 book had an extra preface written by a clergyman, Andreas Osiander, who had been entrusted with the publication, urging that the theory be treated just as a calculating tool. This became a historically important statement of a view about the role of scientific theories known as *instrumentalism,* which holds that we should think of theories only as predictive tools rather than as attempts to describe the hidden structure of nature.

The situation was changed dramatically by Galileo Galilei (1564–1642), working in Italy in the early years of the seventeenth century. Galileo vigorously made the case for the literal truth of the Copernican system, as opposed to its mere usefulness. Galileo used telescopes (which he did not invent but did improve) to look at the heavens, and he found a multitude of phenomena that contradicted Aristotle and the Scholastic view of the world. He also used a combination of mathematics and experiment to begin the formulation of a new science of motion that would make sense of the idea of a moving earth and explain familiar facts about dropped and thrown objects. Galileo's work eventually aroused the ire of the pope; he was forced to recant his Copernican beliefs by the Inquisition and spent his last years under house arrest. (Galileo was treated lightly in comparison with Giordano Bruno, whose refusal to disown his unorthodox speculations about the place of the earth in the universe led to his being burned at the stake in Rome, for heresy, in 1600.)

Galileo remained wedded to circular motion as astronomically fundamental. The move away from circular motion was taken by Johannes Kepler (1571–1630), a mystical thinker who combined Copernicanism with an obsession with finding mathematical harmony (including musical tunes) in the structure of the heavens. Kepler's model of the universe, also developed around the start of the seventeenth century, had the earth and other planets moving in *ellipses,* rather than circles, around the sun. This led to massive simplification and better predictive accuracy.

So far I have mentioned only changes in astronomy and related areas of physics, and I have taken the discussion only to the early part of the seventeenth century. Part of what makes this initial period so dramatic is the removal of the earth from the center of the universe, an event laden with symbolism. Another field that changed in the same period is anatomy. In Padua, Andreas Vesalius (publishing, like Copernicus, in 1543) began to free anatomy from dependence on ancient authority (especially Galen's conclusions) and set it on a more empirical path. Influenced by Vesalius's school, William Harvey achieved the most famous breakthrough in this period, establishing in 1628 the circulation of blood and the role of the heart as a pump.

The mid-seventeenth century saw the rise of a general and ambitious new theory about matter: *mechanism.* The mechanical view of the world combined ideas about the composition of things with ideas about causation and explanation. According to mechanism, the world is made up of tiny "corpuscles" of matter, which interact only by local physical contact. Ultimately, good explanations of physical phenomena should only be given in terms of mechanical interactions. The universe was to be understood as operating like a mechanical clock.

Some, like René Descartes (1596–1650), thought that an immaterial soul and a traditional God must be posited as well as physical corpuscles. Though many figures in the Scientific Revolution held religious views that were at least somewhat unorthodox, most were definitely not looking for a showdown with mainstream religion. Most of the "mechanical philosophers" retained a role for a Christian God in their overall pictures of the world. (If the world is a clock, who set it in motion, for example?) However, the idea of dropping souls, God, or both from the picture was sometimes considered.

In England, Robert Boyle (1627–91) and others embedded a version of mechanism into an organized and well-publicized program of research that urged systematic experiment and the avoidance of unempirical speculation. In the mid-seventeenth century we also see the rise of scientific societies in London, Paris, and Florence. These societies were intended to organize the new research and break the institutional monopoly of the (often conservative) universities.

The period ends with the work of Isaac Newton (1642–1727). In 1687 Newton published his *Principia,* which gave a unified mathematical treatment of motion both on earth and in the heavens. Newton showed why Kepler's elliptical orbits were the inevitable outcome of the force of gravity operating between heavenly bodies, and he vastly improved the ideas about motion on earth that Galileo (and others) had pioneered. So impressive was this work that for hundreds of years Newton was seen as having essentially completed those parts of physics. Newton also did immensely influential work in mathematics and optics, and he suggested the way to move forward in fields like chemistry. In some ways Newton's physics was the culmination of the mechanical worldview, but in some ways it was "post-mechanical," since it posited some forces (gravity, most importantly) that were hard to interpret in mechanical terms.

So by the end of the seventeenth century, the Scholastic worldview had been replaced by a combination of Copernicanism and a form of mechanism. As far as method is concerned, a combination of experiment and mathematical analysis had triumphed (though people disagreed about the nature of the triumphant combination). This ends the period usually referred to as the Scientific Revolution. But the changes described above fed into further changes, both intellectual and political. Chemistry began a period of rapid development in the middle to late eighteenth century, a period sometimes called the Chemical Revolution. The work of Lavoisier, especially his description of oxygen and its role in combustion, is often taken to initiate this "revolution," though it was in the nineteenth century, with the work of Dalton, Mendeleyev, and others, that the basic features of modern chemistry, like the periodic table of elements, were established.

Linnaeus had systematized biological classification in the eighteenth century, but it was the nineteenth century that saw dramatic developments in biology. These developments include the theory that organisms are comprised of cells, Darwin's theory of evolution, the germ theory of disease, and the work by Mendel on inheritance that laid the foundation for genetics.

The Scientific Revolution also fed into more general cultural and political changes. In the eighteenth century the philosophers of the French Enlightenment hoped to use science and reason to sweep away ignorance and superstition, along with oppressive religious and political institutions. The intellectual movements leading to the American and French Revolutions in the late eighteenth century were much influenced by currents of thought in science and philosophy. These included empiricism, mechanism, the inspiration of Newton, and a general desire to understand mankind and society in a way modeled on the understanding of the physical world achieved during the Scientific Revolution.

Further Reading

The topics in this chapter will be discussed in detail later, and references will be given then. Two other introductory books are worth mentioning, though. Hempel's *Philosophy of Natural Science* (1966) was for many years the standard introductory textbook in this area. It opens with the story of Semmelweiss and is a clear and reasonable statement of mainstream twentieth-century empiricism. Alan Chalmers's *What Is This Thing Called Science?* (1999) is also very clear; it presents a different view from Hempel's and the one defended here.

For all the topics in this book, there are also reference works that readers may find helpful. Simon Blackburn's *Oxford Dictionary of Philosophy* is a remarkably useful book and is fun to browse through. The *Routledge Encyclopedia of Philosophy* is also of high quality. *The Blackwell Companion to the Philosophy of Science* has many short papers on key topics (though many of these papers are quite advanced). The *Stanford Online Encyclopedia of Philosophy* is still in progress but will be a very useful (and free) resource.

There are many good books on the Scientific Revolution, each with a different emphasis. Cohen, *The Birth of a New Physics* (1985), is a classic and very good on the physics. Henry, *The Scientific Revolution and the Origins of Modern Science* (1997), is both concise and thorough. It has an excellent chapter on mechanism and contains a large annotated bibliography. Schuster 1990 is also a useful quick summary, and Dear's *Revolutionizing the Sciences* (2001) is a concise and up-to-date book with a good reputation. But Toulmin and Goodfield's *Fabric of the Heavens* (1962), an old book recently reprinted, is my favorite. It focuses on the conceptual foundations underlying the development of scientific ideas. (It is the first of three books by Toulmin and Goodfield on the history of science; the second, *The Architecture of Matter* is also relevant here.)

Kuhn's *Copernican Revolution* (1957), is another classic, focused on the early stages, as the title suggests. Shapin's *Scientific Revolution* (1996), is not a good introduction to the Scientific Revolution but is a very interesting book anyway. There are several good books that focus on particular personalities. Koestler, *The Sleepwalkers* (1968), is fascinating on Kepler, and Sobel, *Galileo's Daughter* (1999), is also good on Galileo (and his daughter, a nun leading a tough life). The standard biography of the amazingly strange Isaac Newton, by Robert Westfall, comes in both long (1980) and short (1993) versions.

For a history of medicine, covering the whole world, see Porter, *The Greatest Benefit to Mankind* (1998).

2

Logic Plus Empiricism

2.1 The Empiricist Tradition

The first approach to science that we will examine is a revolutionary form of empiricism that appeared in the early part of the twentieth century, flourished for a time, was transformed and moderated under the pressure of objections, and then slowly became extinct. The earlier version of the view is called "logical positivism," and the later, moderate form is more usually called "logical empiricism." There is variation in terminology here; "logical empiricism" is sometimes used for the whole movement, early and late. Although we will be looking at fossils in this chapter, these remnants of the past are of great importance in understanding where we are now.

Before discussing logical positivism, it will be helpful to go even further back and say something about the empiricist tradition in general. In the first chapter I said that empiricism is often summarized with the claim that the only source of knowledge is experience. This idea goes back a long way, but the most famous stage of empiricist thought was in the seventeenth and eighteenth centuries, with the work of John Locke, George Berkeley, and David Hume. These "classical" forms of empiricism were based upon theories about the mind and how it works. Their view of the mind is often called "sensationalist." Sensations, like patches of color and sounds, appear in the mind and are all the mind has access to. The role of thought is to track and respond to patterns in these sensations. This view of the mind is not implied by the more basic empiricist idea that experience is the source of knowledge, but for many years such a view was common within empiricism.

Both during these classical discussions and more recently, a problem for empiricism has been a tendency to lapse into *skepticism,* the idea that we cannot know anything about the world. This problem has two aspects. One aspect we can call *external world skepticism:* how can we ever know anything about the real world that lies behind the flow of sensations? The second aspect, made vivid by David Hume, is *inductive skepticism:* why do we have reason to think that the patterns in past experience will also hold in the future?

Empiricism has often shown a surprising willingness to throw in the towel on the issue of external world skepticism. (Hume threw in the towel on both kinds of skepticism, but that is unusual.) Many empiricists have been willing to say that they don't *care* about the possibility that there might be real things lying behind the flow of sensations. It's only the sensations that we have any dealings with. Maybe it makes no sense even to try to *think* about objects lying behind sensations. Perhaps our concept of the world is just a concept of a patterned collection of sensations. This view is sometimes called "phenomenalism." During the nineteenth century, phenomenalist views were quite popular within empiricism, and their oddity was treated with nonchalance. John Stuart Mill, an English philosopher and political theorist, once said that matter may be defined as "a Permanent Possibility of Sensation" (1865, 183). Ernst Mach, an Austrian physicist and philosopher, illustrated his phenomenalist view by drawing a picture of the world as it appeared through his left eye (see fig. 2.1; the shape in the lower right part of the image is his elegant mustache). All that exists is a collection of observer-relative sensory phenomena like these.

I hope phenomenalism looks strange to you, despite its eminent proponents. It *is* a strange idea. But empiricists have often found themselves backing into views like this. This is partly because they have often tended to think of the mind as *confined* behind a "veil of ideas" or sensations. The mind has no "access" to anything outside the veil. Many philosophers, including me, agree that this picture of the mind is a mistake. But it is not so easy to set up an empiricist view that entirely avoids the bad influence of this picture.

In discussions of the history of philosophy, it is common to talk of a showdown in the seventeenth and eighteenth centuries between "the rationalists" and "the empiricists." Rationalists like Descartes and Leibniz believed that pure reasoning can be a route to knowledge that does not depend on experience. Mathematics seemed to be a compelling example of this kind of knowledge. Empiricists like Locke and Hume insisted that experience is our only way of finding out what the world is like. In the late eighteenth century, a sophisticated intermediate position was developed by the German philosopher Immanuel Kant. Kant argued that all our thinking involves a subtle *interaction* between experience and preexisting mental structures that we use to *make sense* of experience. Key concepts like space, time, and causation cannot be derived from experience, because a person must *already* have these concepts in order to use experience to learn

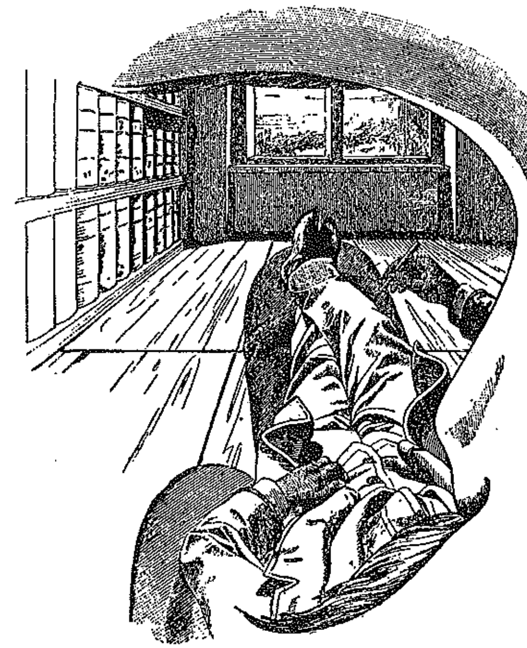

Fig. 2.1
"The assertion, then, is correct that the world consists only of our sensations" (Mach 1897, 10).

about the world. Kant also held that mathematics gives us real knowledge of the world but does not require experience for its justification.

Empiricists must indeed avoid overly simple pictures of how experience affects belief. The mind does not passively receive the imprint of facts. The active and creative role of the mind must be recognized. The trick is to avoid this problem while still remaining true to basic empiricist principles.

As I said above, in the history of philosophy the term "rationalism" is often used for a view that opposes empiricism. In the more recent discussions of science that we are concerned with here, however, the term is generally not used in that way. (This can be a source of confusion; see the glossary.) The views called "rationalist" in the twentieth century were often forms of empiricism; the term was often used in a broad way, to indicate a confidence in the power of human reason.

So much for the long history of debate. Despite various problems, empiricism has been a very attractive set of ideas for many philosophers. Empiricism has often also had a particular kind of impact on discussions outside of philosophy. Making a sweeping generalization, it is fair to say that the empiricist tradition has tended to be (1) pro-science, (2) worldly rather than religious, and (3) politically moderate or liberal (though these political labels can be hard to apply across times). David Hume, John Stuart Mill, and Bertrand Russell are examples of this tendency. Of the three elements of my generalization, religion is the one that has the most counterexamples. Berkeley was a bishop, for example, and Bas van Fraassen, one of the most influential living empiricist philosophers, is also religious. But on the whole it is fair to say that empiricist ideas have tended to be the allies of a practical, scientific, down-to-earth outlook on life. The logical positivists definitely fit this pattern.

2.2 The Vienna Circle

Logical positivism was a form of empiricism developed in Europe after World War I. The movement was established by a group of people who were scientifically oriented and who disliked much of what was happening in philosophy. This group has become known as the *Vienna Circle.*

The Vienna Circle was established by Moritz Schlick and Otto Neurath. It was based, as you might expect, in Vienna, Austria. From the early days through to the end, a central intellectual figure was Rudolf Carnap. Carnap seems to have been the kind of person whose presence inspired awe even in other highly successful philosophers.

Logical positivism was an extreme, swashbuckling form of empiricism. The term "positivism" derives from the nineteenth-century scientific philosophy of Auguste Comte. In the 1930s Carnap suggested that they change the name of their movement from "logical positivism" to "logical empiricism." This change should not be taken to suggest that the later stages in the movement were "more empiricist" than the earlier stages. The opposite is true. In my discussion I will use the term "logical positivism" for the intense, earlier version of their ideas, and "logical empiricism" for the later, more moderate version. Although Carnap suggested the name change in the mid-1930s, the time during which logical positivist ideas changed most markedly was after World War II. I will spend some time in this section describing the unusual intellectual and historical context in which logical positivism developed. In particular, it is easier to understand logical positivism if we pay attention to what the logical positivists were *against.*

The logical positivists were inspired by developments in science in the early years of the twentieth century, especially the work of Einstein. They also thought that developments in logic, mathematics, and the philosophy of language had shown a way to put together a new kind of empiricist philosophy that would settle, once and for all, the problems that philosophy had been concerned with. Some problems would be solved, and other problems would be rejected as meaningless. Logical positivist views about language were influenced by the early ideas of Ludwig Wittgenstein ([1922] 1988). Wittgenstein was an enigmatic, charismatic, and eccentric philosopher of logic and language who was not an empiricist at all. Some would say that the positivists adapted Wittgenstein's ideas, others that they misinterpreted him.

Though they did admire some philosophers, the logical positivists were distressed with much of what had been going on in philosophy. In the years after Kant's death in 1804, philosophy had seen the rise of a number of systems of thought that the logical positivists found pretentious, obscure, dogmatic, and politically harmful. One key villain was G. W. F. Hegel, who worked in the early nineteenth century and had a huge influence on nineteenth-century thought. Hegel was famous for his work on the relation between philosophy and history. He thought that human history as a whole was a process in which a "world spirit" gradually reached consciousness of itself. For Hegel, individuals are less important than the state as a whole, especially the role of the state in the grand march of historical progress. These ideas were taken to support strong forms of nationalism. Hegel's was an "idealist" philosophy, since it held that reality is in some sense spiritual or mental. But this is not a view in which each person's reality is made up in some way by that person's ideas. Rather, a single reality *as a whole* is said to have a spiritual or rational character. This view is sometimes called "absolute idealism."

Hegel's influence bloomed and then receded in continental Europe. As it receded in continental Europe, in the later nineteenth century, it bloomed in England and America. Absolute idealism is a good example of what logical positivism was against. Sometimes the positivists would disparagingly dissect especially obscure passages from this literature. Hans Reichenbach (who was not part of the original Vienna Circle but who was a close ally) began his book *The Rise of Scientific Philosophy* (1951) with a quote from Hegel's most famous work on philosophy and history: "Reason is substance, as well as infinite power, its own infinite material underlying all the natural and spiritual life; as also the infinite form, that which sets the material in motion." Reichenbach lamented that a philosophy student, on first reading this passage, would usually think that it was *his* fault—the student's fault—that he did not understand it. The student would then work away until it finally seemed obvious that Reason was substance, as well as infinite power. . . . For Reichenbach, it is entirely *Hegel's* fault that the passage seems to make no sense. It seems to make no sense because whatever factual meaning the claim might be intended to convey has been smothered with misused language.

People sometimes describe the history of this period as if it was a pitched battle between logical positivism and absolute idealism. That is not how things went. In the early twentieth century, there were many kinds of philosophy jostling and wrangling in Europe. There was a "back to Kant" movement going on (as there seems to be now; perhaps this will happen every hundred years). Another philosopher who came to seem an especially important rival to logical positivism was Martin Heidegger.

Earlier I gave a quick summary of Hegel's ideas. It is much harder to do that for Heidegger. Heidegger is sometimes categorized as an existentialist. Perhaps he is the most famously difficult and obscure philosopher who has ever lived. I will borrow the summary reluctantly given by Thomas Sheehan in the entry for Heidegger in the *Routledge Encyclopedia of Philosophy* (1998): "He argues that mortality is our defining moment, that we are thrown into limited worlds of sense shaped by our being-towards-death, and that finite meaning is all the reality we get." Simplifying even more, Heidegger held that we must understand our lives as based, first and foremost, upon practical coping with the world rather than knowledge of it. All our experience is affected by the awareness that we are traveling toward death. And the best thing we can do in this situation is stare it in the face and live an "authentic" life.

This picture of life might seem to make some sense (especially on a bad day). But Heidegger combined his descriptions of how it feels to live in the world with abstract metaphysical speculation; especially notorious are his discussions of the nature of "Nothing." Heidegger also had one point in common with some (though not all) absolute idealists: his opposition to liberal democratic political ideas.

Heidegger was seen as a key rival by the logical positivists. Carnap gave humorous logical dissections of Heidegger's discussions of Nothing in his lectures. Interestingly, recent work has shown that Carnap and Heidegger understood each other better than was once supposed (Friedman 2000).

Logical positivism was a plea for Enlightenment values, in opposition to mysticism, romanticism, and nationalism. The positivists championed reason over the obscure, the logical over the intuitive. The logical positivists were also internationalists, and they liked the idea of a universal and precise language that everyone could use to communicate clearly. Otto Neurath was the member of the group with the strongest political and social interests. He and various others in the group could be described as democratic socialists. They had a keen interest in some movements in art and architecture at the time, such as the Bauhaus movement. They saw this

work as assisting the development of a scientific, internationalist, and practical outlook on society (Galison 1990).

The Vienna Circle flourished from the mid-1920s to the mid-1930s. Logical positivist ideas were imported into England by A. J. Ayer in *Language, Truth, and Logic* (1936), a vivid and readable book that conveys the excitement of the time. Under the influence of logical positivism, and the philosophy of G. E. Moore and Bertrand Russell, English philosophy abandoned absolute idealism and returned to its traditional empiricist emphasis, an emphasis it has retained (more or less) ever since.

In continental Europe the story turned out differently. For we have now, remember, reached the 1930s. The development of logical positivism ran straight into the rise of Adolf Hitler.

Many of the Vienna Circle had socialist leanings, some were Jewish, and there were certainly no Nazis. So the logical positivists were persecuted by the Nazis, to varying degrees. The Nazis encouraged and made use of pro-German, anti-liberal philosophers, who also tended to be obscure and mystical. In contrast to the logical positivists, Martin Heidegger joined the Nazi party in 1933 and remained a member throughout the war.

Many logical positivists fled Europe, especially to the United States. Schlick, unfortunately, did not. He was murdered by a deranged former student in 1936. The logical positivists who did make it to the United States were responsible for a great flowering of American philosophy in the years after World War II. These include Rudolf Carnap, Hans Reichenbach, Carl Hempel, and Herbert Feigl. In the United States the strident voice of logical positivists was moderated. Partly this was because of criticisms of their ideas—criticisms from the side of those who shared their general outlook. But the moderation was no doubt partly due to the different intellectual and political climate in the United States. Austria and Germany in the 1930s had been an unusually intense environment for doing philosophy.

2.3 Central Ideas of Logical Positivism

Logical positivist views about science and knowledge were based on a general theory of language; we need to start here, before moving to the views about science. This theory of language featured two main ideas, the *analytic-synthetic distinction* and the *verifiability theory of meaning*.

The analytic-synthetic distinction will probably strike you as bland and obvious, at least at first. Some sentences are true or false simply in virtue of their meaning, regardless of how the world happens to be; these are analytic. A synthetic sentence is true or false in virtue of both the meaning of the sentence *and* how the world actually is. "All bachelors are unmarried" is the standard example of an analytically true sentence. "All bachelors are bald" is an example of a synthetic sentence, in this case a false one. Analytic truths are, in a sense, empty truths, with no factual content. Their truth has a kind of necessity, but only because they are empty.

This distinction had been around, in various forms, since at least the eighteenth century. The terminology "analytic-synthetic" was introduced by Kant. Although the distinction itself looks uncontroversial, it can be made to do real philosophical work. Here is one crucial piece of work the logical positivists saw for it: they claimed that all of mathematics and logic is analytic. This made it possible for them to deal with mathematical knowledge within an empiricist framework. For logical positivism, mathematical propositions do not describe the world; they merely record our conventional decision to use symbols in a particular way. Synthetic claims about the world can be expressed using mathematical language, such as when it is claimed that there are nine planets in the solar system. But proofs and investigations within mathematics itself are analytic. This might seem strange because some proofs in mathematics are very surprising. The logical positivists insisted that once we break down such a proof into small steps, each step will be trivial and unsurprising.

Earlier philosophers in the rationalist tradition had claimed that some things can be known a priori; this means known *independently of experience.* Logical positivism held that the only things that seem to be knowable a priori are analytic and hence empty of factual content.

A remarkable episode in the history of science is important here. For many centuries, the geometry of the ancient Greek mathematician Euclid was regarded as a shining example of real and certain knowledge. Immanuel Kant, inspired by the immensely successful application of Euclidean geometry to nature in Newtonian physics, even claimed that Euclid's geometry (along with the rest of mathematics) is both synthetic and knowable a priori. In the nineteenth century, mathematicians did work out alternative geometrical systems to Euclid's, but they did so as a mathematical exercise, not as an attempt to describe how lines, angles, and shapes work in the actual world. Early in the twentieth century, however, Einstein's revolutionary work in physics showed that a non-Euclidean geometry *is* true of our world. The logical positivists were enormously impressed by this development, and it guided their analysis of mathematical knowledge. The positivists insisted that pure mathematics is analytic, and they broke geometry into two parts. One part is purely mathematical, analytic, and says nothing about the world. It merely describes possible geometrical systems. The other part of geometry is a set of synthetic claims about which geometrical system applies to our world.

I turn now to the other main idea in the logical positivist theory of language, the *verifiability theory of meaning.* This theory applies only to sentences that are not analytic, and it involves a specific kind of "meaning," the kind involved when someone is trying to say something about the world. Here is how the theory was often put: *the meaning of a sentence consists in its method of verification.* That formulation might sound strange (it always has to me). Here is a formulation that sounds more natural: knowing the meaning of a sentence is knowing how to verify it. And here is a key application of the principle: if a sentence has no possible method of verification, it has no meaning.

By "verification" here, the positivists meant verification *by means of observation.* Observation in all these discussions is construed broadly, to include all kinds of sensory experience. And "verifiability" is not the best word for what they meant. A better word would be "testability." This is because testing is an attempt to work out whether something is true *or* false, and that is what the positivists had in mind. The term "verifiable" generally only applies when you are able to show that something is true. It would have been better to call the theory "the testability theory of meaning." Sometimes the logical positivists did use that phrase, but the more standard name is "verifiability theory," or just "verificationism."

Verificationism is a strong empiricist principle; experience is the only source of meaning, as well as the only source of knowledge. Note that verifiability here refers to verifiability in *principle,* not in practice. There was some dispute about which hard-to-verify claims are really verifiable in principle. It is also important that *conclusive* verification or testing was not required. There just had to be the possibility of finding observational evidence that would count for or against the proposition in question.

In the early days of logical positivism, the idea was that in principle one could *translate* all sentences with factual meaning into sentences that referred only to sensations and the patterns connecting them. This program of translation was fairly quickly abandoned as too extreme. But the verifiability theory was retained after the program of translation had been dropped.

The verifiability principle was used by the logical positivists as a philosophical weapon. Scientific discussion, and most everyday discussion, consists of verifiable and hence meaningful claims. Some other parts of language are clearly not intended to have factual meaning, so they fail the verifiability test but do so in a harmless way. Included are poetic language, expressions of emotion, and so on. But there are also parts of language that are *supposed* to have factual meaning—are supposed to say something about the world—but which *fail* to do so. For the logical positivists, this includes most traditional philosophy, much of ethics, and theology as well!

This analysis of language provided the framework for the logical positivist philosophy of science. Science itself was seen as just a more complex and sophisticated version of the sort of thinking, reasoning, and problem-solving that we find in everyday life—and completely *un*like the meaningless blather of traditional philosophy.

So let us now look at the logical positivists' picture of science and of the role of philosophy in a scientific worldview. Next we should turn to another distinction they made, between "observational" language and "theoretical" language. There was uncertainty about how exactly to set this distinction up. Usually it was seen as a distinction applied to individual terms. "Red" is in the observational part of language, and "electron" is in the theoretical part. There was also a related distinction at the level of sentences. "The rod is glowing red" is observational, while "Helium atoms each contain two electrons" is theoretical. A more important question was where to draw the line. Schlick thought that only terms referring to sensations were observational; everything else was theoretical. Here Schlick stayed close to traditional empiricism. Neurath thought this was a mistake and argued that terms referring to many ordinary physical objects are in the observational part of language. For Neurath, scientific testing must not be understood in a way that makes it private to the individual. Only observation statements about physical objects can be the basis of public or "intersubjective" testing.

The issue became a constant topic of discussion. In time, Carnap came to think that there are lots of acceptable ways of marking out a distinction between the observational and theoretical parts of language; one could use whichever is convenient for the purposes at hand. This was the start of a more general move that Carnap made toward a view based on the "tolerance" of alternative linguistic frameworks.

We now need to look at logical positivist views about logic. For logical positivism, *logic is the main tool for philosophy*, including philosophical discussion of science. In fact, just about the only useful thing that philosophers can do is give logical analyses of how language, mathematics, and science work.

Here we should distinguish two kinds of logic (this discussion will be continued in chapter 3). Logic in general is the attempt to give an abstract theory of what makes some arguments compelling and reliable. Deductive logic is the most familiar kind of logic, and it describes patterns of argument that transmit truth with certainty. These are arguments with the feature that if the premises of the argument are true, the conclusion must be true. Impressive developments in deductive logic had been under way since the late nineteenth century and were still going on at the time of the Vienna Circle.

The logical positivists also believed in a second kind of logic, a kind that was (and is) much more controversial. This is *inductive* logic. Inductive logic was supposed to be a theory of arguments that provide support for their conclusions but do not give the kind of guarantee found in deductive logic.

From the logical positivist point of view, developing an inductive logic was of great importance. Hardly any of the arguments and evidence that we confront in everyday life and science carry the kind of guarantees found in deductive logic. Even the best kind of evidence we can find for a scientific theory is not completely decisive. There is always the possibility of error, but that does not stop some claims in science from being supported by evidence. The logical positivists accepted and embraced the fact that error is always possible. Although some critics have misinterpreted them on this point, the logical positivists did *not* think that science ever reaches absolute certainty.

The logical positivists saw the task of logically analyzing science as sharply distinct from any attempt to understand science in terms of its history or psychology. Those are empirical disciplines, and they involve a different set of questions from those of philosophy.

A terminology standardly used to express the separations between different approaches here was introduced by Hans Reichenbach. Reichenbach distinguished between the "context of discovery" and the "context of justification." That terminology is not helpful, because it suggests that the distinction has to do with "before and after." It might seem that the point being made is that discovery comes first and justification comes afterward. That is not the point being made (though the logical positivists were not completely clear on this). The key distinction is between the study of the logical structure of science and the study of historical and psychological aspects of science.

So logical positivism tended to dismiss the relevance of fields like history and psychology to the philosophy of science. In time this came to be regarded as a big mistake.

Let us put all these ideas together and look at the picture of science that results. Logical positivism was a revolutionary, uncompromising version of empiricism, based largely on a theory of language. The aim of science—and the aim of everyday thought and problem-solving as well—is to track and anticipate patterns in experience. As Schlick once put it, "what every scientist seeks, and seeks alone, are . . . the rules which govern the connection of experiences, and by which alone they can be predicted" (1932–33, 44). We can make rational predictions about future experiences by attending to patterns in past experience, but we never get a guarantee. We could always be wrong. There is no alternative route to knowledge besides experience;

when traditional philosophy has tried to find such a route, it has lapsed into meaninglessness.

The interpretation of logical positivism I have just given is a standard one. There is controversy about how to interpret the aims and doctrines of the movement, however. Some recent writers have argued that there is less of a link between logical positivism and traditional empiricism than the standard interpretation claims (Friedman 1999). But in the sense of empiricism used in this book, there is definitely a strong link. We see that in the Schlick quote given in the previous paragraph.

During the early twentieth century, there were various other strong versions of empiricism being developed as well. One was *operationalism,* which was developed by a physicist, Percy Bridgman (1927). Operationalism held that scientists should use language in such a way that all theoretical terms are tied closely to direct observational tests. This is akin to logical positivism, but it was expressed more as a proposed *tightening up* of scientific language (motivated especially by the lessons of Einstein's theory of relativity) than as an analysis of how all science already works.

In the latter part of the twentieth century, an image of the logical positivists developed in which they were seen as stodgy, conservative, unimaginative science-worshipers. Their strongly pro-science stance has even been seen as antidemocratic, or aligned with repressive political ideas. This is very unfair, given their actual political interests and activities. Later we will see how ideas about the relation between science and politics changed through the twentieth century in a way that made this interpretation possible. The accusation of stodginess is another matter; the logical positivists' writings were often extremely dry and technical. Still, even the driest of their ideas were part of a remarkable program that aimed at a massive, transdisciplinary, intellectual housecleaning. And their version of empiricism was organized around an ideal of intellectual flexibility as a mark of science and rationality. We see this in a famous metaphor used by Neurath (who exemplifies these themes especially well). Neurath said that in our attempts to learn about the world and improve our ideas, we are "like sailors who have to rebuild their ship on the open sea." The sailors replace pieces of their ship plank by plank, in a way that eventually results in major changes but which is constrained by the need to keep the ship afloat during the process.

2.4 Problems and Changes

Logical positivist ideas were always in a state of flux, and they were subject to many challenges. One set of problems was internal to the program. For example, there was considerable difficulty in getting a good formulation of

the verifiability principle. It turned out to be hard to formulate the principle in a way that would exclude all the obscure traditional philosophy but include all of science. Some of these problems were almost comically simple. For example, if "Metals expand when heated" is testable, then "Metals expand when heated and the Absolute Spirit is perfect" is also testable. If we could empirically show the first part of the claim to be false, then the whole claim would be shown false, because of the logic of statements containing "and." (If *A* is false then *A*&*B* must be false too.) Patching this hole led to new problems elsewhere; the whole project was quite frustrating (Hempel 1965, chap. 4). The attempt to develop an inductive logic also ran into serious trouble. That topic will be covered in the next chapter.

Other criticisms were directed not at the details but at the most basic ideas of the movement. The criticism that I will focus on here is one of these, and its most famous presentation is in a paper sometimes regarded as the most important in all of twentieth-century philosophy: W. V. Quine's "Two Dogmas of Empiricism" (1953).

Quine argued for a *holistic* theory of testing, and he used this to motivate a holistic theory of meaning as well. In describing the view, first I should say something about holism in general. Many areas of philosophy contain views that are described using the term "holism." A holist argues that you cannot understand a particular thing without looking at its place in a larger whole. In the case we are concerned with here, holism about testing says that we cannot test a single hypothesis or sentence in isolation. Instead, we can only test complex networks of claims and assumptions. This is because only a complex network of claims and assumptions makes definite predictions about what we should observe.

Let us look more closely at the idea that individual claims about the world cannot be tested in isolation. The idea is that in order to test one claim, you need to make assumptions about many other things. Often these will be assumptions about measuring instruments, the circumstances of observation, the reliability of records and of other observers, and so on. So whenever you think of yourself as testing a single idea, what you are really testing is a long, complicated *conjunction* of statements; it is the whole conjunction that gives you a definite prediction. If a test has an unexpected result, then something in that conjunction is false, but the failure of the test itself does not tell you *where* the error is.

For example, suppose you want to test the hypothesis that high air pressure is associated with fair, stable weather. You make a series of observations, and what you seem to find is that high pressure is instead associated with unstable weather. It is natural to suspect that your original hypothesis was wrong, but there are other possibilities as well. It might be that your barometer does not give reliable measurements of air pressure. There might also be something wrong with the observations made (by you or others) of the weather conditions themselves. The unexpected observations are telling you that *something* is wrong, but the problem might lie with one of your background assumptions, not with the hypothesis you were trying to test.

Some parts of this argument are convincing. It is true that only a network of claims and assumptions, not a single hypothesis alone, tells us what we should expect to observe. The failure of a prediction will always have a range of possible explanations. In that sense, testing is indeed holistic. But this leaves open the possibility that we might often have good reasons to lay the blame for a failed prediction at one place rather than another. In practice, science seems to have some effective ways of working out where to lay the blame. Giving a philosophical theory of these decisions is a difficult task, but the mere fact that failed predictions always have a range of possible explanations does not settle the holism debate.

Holist arguments had a huge effect on the philosophy of science in the middle of the twentieth century. Quine, who sprinkled his writings with deft analogies and dry humor, argued that mainstream empiricism had been committed to a badly simplistic view of testing. We must accept, as Quine said in a famous metaphor, that our theories "face the tribunal of sense-experience . . . as a corporate body" (1953, 41). Logical positivism must be replaced with a holistic version of empiricism.

But there is a puzzle here. The logical positivists *already accepted* that testing is holistic in the sense described above. Here is Herbert Feigl, writing in 1943: "No scientific assumption is testable in complete isolation. Only whole complexes of inter-related hypotheses can be put to the test" (1943, 16). Carnap had been saying the same thing (1937, 318). We can even find statements like this in Ayer's *Language, Truth, and Logic* (1936).

Quine did recognize Pierre Duhem, a much earlier French physicist and philosopher, as someone who had argued for holism about testing. (Holism about testing is often called "the Duhem-Quine thesis.") But how could it be argued that logical positivists had dogmatically missed this important fact, when they repeatedly expressed it in print? Regardless of this, many philosophers agreed with Quine that logical positivism had made a bad mistake about testing in science.

Though the history of the issue is strange, it might be fair to say this: although the logical positivists officially accepted a holistic view about testing, they did not appreciate the significance of the point. The verifiability principle *seems* to suggest that you can test sentences one at a time. It seems to attach a set of observable outcomes of tests to each sentence in isolation.

Strictly, the positivists generally held that these observations are only associated with a specific hypothesis *against a background of other assumptions.* But then it seems questionable to associate the test results solely with the hypothesis itself. Quine, in contrast, made the consequences of holism about testing very clear. He also drew conclusions about language and meaning; given the link between testing and meaning asserted by logical positivism, holism about testing leads to holism about meaning. And holism about meaning causes problems for many logical positivist ideas.

The version of holism that Quine defended in "Two Dogmas" was an extreme one. It included an attack on the one idea in the previous section that you might have thought was completely safe: the analytic-synthetic distinction. Quine argued that this distinction *does not exist;* this is another unjustified "dogma" of empiricism.

Here again, some of Quine's arguments were directed at a version of the analytic-synthetic distinction that the logical positivists no longer held. Quine said that the idea of analyticity was intended to treat some claims as *immune to revision,* and he argued that in fact no statement is immune to revision. But Carnap had already decided that analytic statements can be revised, though they are revised in a special way. A person or community can decide to drop one whole linguistic and logical framework and adopt another. Against the background provided by a given linguistic and logical framework, some statements will be analytic and hence not susceptible to empirical test. But we can always change frameworks. By the time that Quine was writing, Carnap's philosophy was based on a distinction between changes made *within* a linguistic and logical framework, and changes *between* these frameworks.

In another (more convincing) part of his paper, Quine argued that there is no way to make *scientific sense* of a sharp analytic-synthetic distinction. He connected this point to his holism about testing. For Quine, all our ideas and hypotheses form a single "web of belief," which has contact with experience only as whole. An unexpected observation can prompt us to make a great variety of possible changes to the web. Even sentences that might look analytic can be revised in response to experience in some circumstances. Quine noted that strange results in quantum physics had suggested to some that revisions in logic might be needed.

In this discussion of problems for logical positivism, I have included some discussions that started early and some that took place after World War II, when the movement had begun its U.S.-based transformation. Let us now look at some central ideas of logical empiricism, the later, less aggressive stage of the movement.

2.5 Logical Empiricism

Let's see how things looked in the years after World War II. Schlick is dead, and other remnants of the Vienna Circle are safely housed in American universities—Carnap at Chicago, Hempel at Pittsburgh and then Princeton, Reichenbach at UCLA (via Turkey), Feigl at Minnesota. Many of the same people are involved, but the work is different. The revolutionary attempt to destroy traditional philosophy has been replaced by a program of careful logical analysis of language and science. Discussion of the contributions that could be made by the scientific worldview to a democratic socialist future have been dropped or greatly muted. (Despite this, the FBI collected a file on Carnap as a possible Communist sympathizer.)

As before, ideas about language guided logical empiricist ideas about science. The analytic-synthetic distinction had not been rejected, but it was regarded as questionable. The logical empiricists felt the pressure of Quine's arguments. The verifiability theory, which had been so scythe-like in its early forms, was replaced by a *holistic empiricist theory of meaning.* Theories were seen as abstract structures that connect many hypotheses together. These structures are connected, as wholes, to the observable realm, but each *bit* of a theory—each claim or hypothesis or concept—does not have some specific set of observations associated with it. A theoretical term (like "electron" or "gene") derives its meaning from its place in the whole structure and from the structure's connection to the realm of observation.

Late in the logical empiricist era, in 1970, Herbert Feigl gave a pictorial representation of what he called "the orthodox view" of theories (see fig. 2.2). A network of theoretical hypotheses ("postulates") is connected by stages to what Feigl calls the "soil" of experience. This anchoring is the source of the network's meaning. Feigl used this picture to describe a single scientific theory. For the more extreme holism of Quine, a person's *total* set of beliefs form a *single* network.

The logical positivist distinction between observational and theoretical parts of language was kept roughly intact. But the idea that observational language describes private sensations had been dropped. The observational base of science was seen as made up of descriptions of observable physical objects (though Carnap thought it might occasionally be useful to work with a language referring to sensations).

Logical positivist views about the role of logic in philosophy and about the sharp separation between the logic of science and the historical-psychological side of science were basically unchanged. A good example of the kind of work done by logical empiricists is provided by their work on

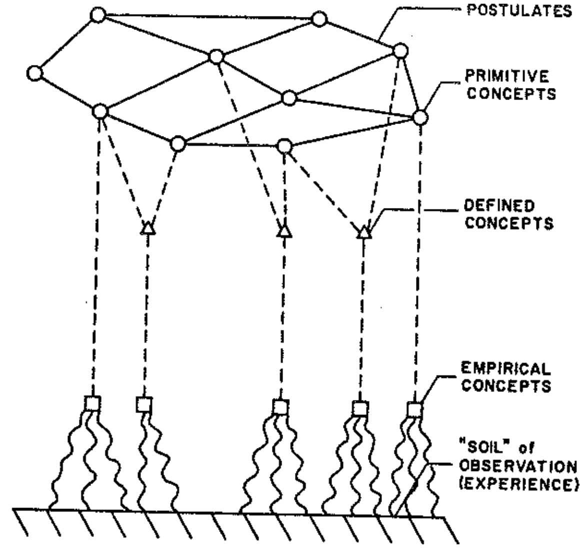

Fig. 2.2
Feigl's picture of the logical empiricist view of theories
(From Feigl 1970; reproduced courtesy of University of Minnesota Press)

explanation in science (see especially Hempel and Oppenheim 1948; Hempel 1965). For Hempel, to explain something is to show how to *infer* it using a logical argument, where the premises of the argument include at least one statement of a natural law (see chapter 13 below). This illustrates the idea, common to logical positivism and logical empiricism, that logic is the main tool of philosophy of science.

We saw that logical positivism held that the sole aim of science is to track patterns in experience. For logical positivism, when a scientist seems to be trying to describe unobservable structures in the world that give rise to what we see, the scientist must instead be seen as describing the *observable* world in a special, abstract way. Scientific language is only meaningful insofar as it picks out patterns in the flow of experience. Now, does logical *empiricism* make the same claim? Does logical empiricism claim that scientific language ultimately only describes patterns in observables?

The answer is that logical empiricists agonized over this. In their hearts their answer was *yes,* but this answer seemed to get harder and harder to defend. Carl Hempel wrote a paper in 1958 called "The Theoretician's Dilemma," which was the height of logical empiricist agony over the issue. As a fairly traditional empiricist, Hempel was attracted to the idea that the *only possible role* for those parts of language that seem to refer to unobservable entities is to help us pick out patterns in the observable realm. And if the parts of theories that appear to posit unobservable things are really any good, this "goodness" has to show up in advantages the theory has in its handling of observables. So there is no justification for seeing these parts of scientific language as describing real objects lying beyond experience. But Hempel and the logical empiricists found themselves forced to concede that this view does not make much sense of actual scientific work. When scientists use terms like "electron" or "gene," they act as if they are doing more than tracking complex patterns in the observable realm. But the idea that the logical empiricists were being pushed toward—the idea that scientific theories are aimed at describing unobservable real structures—was hard to put on the table and defend. Empiricist philosophy of language seemed implacably opposed to it.

Empiricists were familiar with bad versions of the idea that behind the ordinary world of observables there is a special and superior realm, pure and perfect. This "layered" view of reality seemed to empiricists a source of endless trouble, right from the time of the ancient Greek philosopher Plato, who distinguished the illusory, unstable world of "appearances" from the more perfect and real world of "forms." Empiricists have rightly been determined to avoid this kind of picture. But much of science does appear to be a process in which people hypothesize hidden structures that give rise to observable phenomena. These hidden structures are not "pure and perfect" or "more real" than the observable parts of the world, but they do lie behind or beneath observable phenomena. Of course, unobservable structures posited by a theory at one time might well turn out to be observable at a later time. In science, there is no telling what kinds of new access to the hidden parts of the world we might eventually achieve. But still, much of science does seem to proceed by positing entities that are, at the time of the research in question, truly hidden. For the traditional empiricist philosopher, understanding scientific theorizing in a way that posits a layer of observable phenomena and a layer of hidden structure responsible for the phenomena takes us *far too close* to bad old philosophical views like Plato's. We are too close for comfort, so we must give a different kind of description of how science works.

The result is the traditional empiricist insistence that, ultimately, the only thing scientific language can do is describe patterns in the observable realm. In the first published paper that introduced logical positivism, Carnap, Hahn, and Neurath said: "In science there are no 'depths'; there is surface everywhere" ([1929] 1973, 306). This is a vivid expression of the empiricist aversion to a view in which the aim of theorizing is to describe hidden

levels of structure. Science uses unusual theoretical concepts (which *look* initially like attempts to refer to hidden things) as a way of discovering and describing subtle patterns in the observable realm. So the logical positivists and the logical empiricists talked constantly about *prediction* as the goal of science. Prediction was a substitute for the more obvious-looking—but ultimately forbidden—goal of describing the real hidden structure of the world.

Twentieth-century empiricism made an important mistake here. We can make sense of science only by treating much of it as an attempt to describe hidden structures that give rise to observable phenomena. This is a version of *scientific realism,* an idea that will be discussed later in this book. In science there *are* depths. There is not a simple and fixed distinction between two "layers" in nature—the empiricists were right to distrust this idea. Instead there are *many* layers, or rather a *continuum* between structures that are more accessible to us and structures that are less accessible. Genes are hidden from us in some ways, but not as hidden as electrons, which in turn are not as hidden as quarks. Although there are "depths" in science, what is deep at one time can come to the surface at later times, and there may be lots of ways of interacting with what is presently deep.

2.6 On the Fall of Logical Empiricism

Logical empiricist ideas dominated much American philosophy, and they were very influential elsewhere in the English-speaking world and in some parts of Europe, in the middle of the twentieth century. But by the mid-1960s the view was definitely under threat; and by the middle or late 1970s, logical empiricism was near to extinction. The fall of logical empiricism was due to several factors, all of which I have either introduced in this chapter or will discuss in later chapters. One is the breakdown of the view of language that formed the basis of many logical positivist and logical empiricist ideas. Another is pressure from holist arguments. A third is the frustrating history of attempts to develop an inductive logic (chapter 3). A fourth is the development of a new role for fields like history and psychology in the philosophy of science (chapters 5–7). And eventually there was pressure from scientific realism. But this was only possible after logical empiricism had begun to decline.

Further Reading

For much more on the empiricist tradition in general, see Garrett and Barbanell, *Encyclopedia of Empiricism* (1997).

Schlick's "Positivism and Realism" (1932–33) and Feigl's "Logical Empiricism" (1943) are good statements of logical positivism by original members of the Vienna Circle. (Feigl uses the term "logical empiricism," but his paper describes a fairly strong, undiluted version of the view.) Ayer's *Language, Truth, and Logic* (1936) is readable, vivid, and exciting. Some see it as a distortion of logical positivist ideas.

The *Routledge Encyclopedia of Philosophy* (1998) has an interesting collection of articles, especially in the light of new debates about the history of logical positivism. The article on logical positivism is by Friedman and reflects his somewhat unorthodox reading (de-emphasizing the empiricist tradition). Stadler's entry on the Vienna Circle gives a more traditional view. See also Creath's entry on Carnap. On all these issues, see also the essays in Giere and Richardson 1997.

Peter Galison's "Aufbau/Bauhaus" (1990) is a wonderful account of the artistic, social, and political interests of the logical positivists and the links between these interests and their philosophical ideas. Passmore 1966 is a good and accessible survey of philosophical movements and trends in the late nineteenth and early twentieth centuries, including absolute idealism.

Hempel, *Aspects of Scientific Explanation* (1965), is the definitive statement of logical empiricism. His *Philosophy of Natural Science* (1966) is the easy version. Carnap's later lectures have been published as *Introduction to the Philosophy of Science* (1995).

An attempt to revive some logical positivist ideas has recently begun; see, for example, Elliott Sober's forthcoming book *Learning from Logical Positivism.*

3

Induction and Confirmation

3.1 The Mother of All Problems

In this chapter we begin looking at a very important and difficult problem, the problem of understanding how observations can *confirm* a scientific theory. What connection between an observation and a theory makes that observation *evidence for* the theory? In some ways, this has been *the* fundamental problem in the last hundred years of philosophy of science. This problem was central to the projects of logical positivism and logical empiricism, and it was a source of constant frustration for them. And although some might be tempted to think so, this problem does not disappear once we give up on logical empiricism. The problem, in some form or other, arises for nearly everyone.

The aim of the logical empiricists was to develop a *logical* theory of evidence and confirmation, a theory treating confirmation as an abstract relation between sentences. It has become fairly clear that their approach to the problem is doomed. The way to analyze testing and evidence in science is to develop a different kind of theory. But it will take a lot of discussion, in this and later chapters, before the differences between approaches that will and will not work in this area can emerge. The present chapter will mostly look at how the problem of confirmation was tackled in the middle of the twentieth century. And that is a tale of woe.

Before looking at twentieth-century work on these issues, we must again look further into the past. The confirmation of theories is closely connected to another classic issue in philosophy: *the problem of induction.* What reason do we have for expecting patterns observed in our past experience to hold also in the future? What justification do we have for using past observations as a basis for generalization about things we have not yet observed?

The most famous discussions of induction were written by the eighteenth-century Scottish empiricist David Hume ([1739] 1978). Hume asked, What reason do we have for thinking that the future will resemble the past? There