Introduction

Science is highly esteemed. Apparently it is a widely held belief that there is something special about science and its methods. The naming of some claim or line of reasoning or piece of research "scientific" is done in a way that is intended to imply some kind of merit or special kind of reliability. But what, if anything, is so special about science? What is this "scientific method" that allegedly leads to especially meritorious or reliable results? This book is an attempt to elucidate and answer questions of that kind.

There is an abundance of evidence from everyday life that science is held in high regard, in spite of some disenchantment with science because of consequences for which some hold it responsible, such as hydrogen bombs and pollution. Advertisements frequently assert that a particular product has been scientifically shown to be whiter, more potent, more sexually appealing or in some way superior to rival products. This is intended to imply that the claims are particularly well-founded and perhaps beyond dispute. A recent newspaper advertisement advocating Christian Science was headed "Science speaks and says the Christian Bible is provedly true" and went on to tell us that "even the scientists themselves believe it these days". Here we have a direct appeal to the authority of science and scientists. We might well ask what the basis for such authority is. The high regard for science is not restricted to everyday life and the popular media. It is evident in the scholarly and academic world too. Many areas of study are now described as sciences by their supporters, presumably in an effort to imply that the methods used are as firmly based and as potentially fruitful as in a traditional science such as physics or biology. Political science and social science are by now commonplace. Many Marxists are keen to insist that historical materialism is a science. In addition, Library Science, Administrative Science, Speech

Science, Forest Science, Dairy Science, Meat and Animal Science and Mortuary Science have all made their appearance on university syllabuses.[1] The debate about the status of "creation science" is still active. It is noteworthy in this context that participants on both sides of the debate assume that there is some special category "science" . What they disagree about is whether creation science qualifies as a science or not.

Many in the so-called social or human sciences subscribe to a line of argument that runs roughly as follows. "The undoubted success of physics over the last three hundred years, it is assumed, is to be attributed to the application of a special method, 'the scientific method'. Therefore, if the social and human sciences are to emulate the success of physics then that is to be achieved by first understanding and formulating this method and then applying it to the social and human sciences." Two fundamental questions are raised by this line of argument, namely, "what is this scientific method that is alleged to be the key to the success of physics?" and "is it legitimate to transfer that method from physics and apply it elsewhere?".

All this highlights the fact that questions concerning the distinctiveness of scientific knowledge, as opposed to other kinds of knowledge, and the exact identification of the scientific method are seen as fundamentally important and consequential. As we shall see, however, answering these questions is by no means straightforward. A fair attempt to capture widespread intuitions about the answers to them is encapsulated, perhaps, in the idea that what is so special about science is that it is derived from the facts, rather than being based on personal opinion. This maybe captures the idea that, whereas personal opinions may differ over the relative merits of the novels of Charles Dickens and D. H. Lawrence, there is no room for such variation of opinions on the relative merits of Galileo's and Einstein's theories of relativity. It is the facts that are presumed to determine the superiority of Einstein's

innovations over previous views on relativity, and anyone who fails to appreciate this is simply wrong.

As we shall see, the idea that the distinctive feature of scientific knowledge is that it is derived from the facts of experience can only be sanctioned in a carefully and highly qualified form, if it is to be sanctioned at all. We will encounter reasons for doubting that facts acquired by observation and experiment are as straightforward and secure as has traditionally been assumed. We will also find that a strong case can be made for the claim that scientific knowledge can neither be conclusively proved nor conclusively disproved by reference to the facts, even if the availability of those facts is assumed. Some of the arguments to support this skepticism are based on an analysis of the nature of observation and on the nature of logical reasoning and its capabilities. Others stem from a close look at the history of science and contemporary scientific practice. It has been a feature of modern developments in theories of science and scientific method that increasing attention has been paid to the history of science. One of the embarrassing results of this for many philosophers of science is that those episodes in the history of science that are commonly regarded as most characteristic of major advances, whether they be the innovations of Galileo, Newton, Darwin or Einstein, do not match what standard philosophical accounts of science say they should be like.

One reaction to the realisation that scientific theories cannot be conclusively proved or disproved and that the reconstructions of philosophers bear little resemblance to what actually goes on in science is to give up altogether the idea that science is a rational activity operating according to some special method. It is a reaction somewhat like this that led the philosopher Paul Feyerabend (1975) to write a book with the title *Against Method: Outline of an Anarchistic Theory of Knowledge*. According to the most extreme view that has been read into Feyerabend's later writings, science has no special features that render it intrinsically superior to other kinds of knowledge such as ancient myths or voodoo. A high regard for science is seen as a modern religion, playing a similar role to that played by Christianity in Europe in earlier eras. It is suggested that the choices between scientific theories boils down to choices determined by the subjective values and wishes of individuals.

Feyerabend's skepticism about attempts to rationalise science are shared by more recent authors writing from a sociological or so-called "postmodernist" perspective.

This kind of response to the difficulties with traditional accounts of science and scientific method is resisted in this book. An attempt is made to accept what is valid in the challenges by Feyerabend and many others, but yet to give an account of science that captures its distinctive and special features in a way that can answer those challenges.

CHAPTER 1

Science as knowledge derived from the facts of experience

A widely held commonsense view of science

In the Introduction I ventured the suggestion that a popular conception of the distinctive feature of scientific knowledge is captured by the slogan "science is derived from the facts". In the first four chapters of this book this view is subjected to a critical scrutiny. We will find that much of what is typically taken to be implied by the slogan cannot be defended. Nevertheless, we will find that the slogan is not entirely misguided and I will attempt to formulate a defensible version of it.

When it is claimed that science is special because it is based on the facts, the facts are presumed to be claims about the world that can be directly established by a careful, unprejudiced use of the senses. Science is to be based on what we can see, hear and touch rather than on personal opinions or speculative imaginings. If observation of the world is carried out in a careful, unprejudiced way then the facts established in this way will constitute a secure, objective basis for science. If, further, the reasoning that takes us from this factual basis to the laws and theories that constitute scientific knowledge is sound, then the resulting knowledge can itself be taken to be securely established and objective.

The above remarks are the bare bones of a familiar story that is reflected in a wide range of literature about science. "Science is a structure built upon facts" writes J. J. Davies (1968, p. 8) in his book on the scientific method, a theme elaborated on by H. D. Anthony (1948, p. 145):

> It was not so much the observations and experiments which Galileo made that caused the break with tradition as his *attitude* to them. For him, the facts based on them were taken as facts, and not related to some preconceived idea ... The facts of

> observation might, or might not, fit into an acknowledged scheme of the universe, but the important thing, in Galileo's opinion, was to accept the facts and build the theory to fit them.

Anthony here not only gives clear expression to the view that scientific knowledge is based on the facts established by observation and experiment, but also gives a historical twist to the idea, and he is by no means alone in this. An influential claim is that, as a matter of historical fact, modern science was born in the early seventeenth century when the strategy of taking the facts of observation seriously as the basis for science was first seriously adopted. It is held by those who embrace and exploit this story about the birth of science that prior to the seventeenth century the observable facts were not taken seriously as the foundation for knowledge. Rather, so the familiar story goes, knowledge was based largely on authority, especially the authority of the philosopher Aristotle and the authority of the Bible. It was only when this authority was challenged by an appeal to experience, by pioneers of the new science such as Galileo, that modern science became possible. The following account of the oft-told story of Galileo and the Leaning Tower of Pisa, taken from Rowbotham (1918, pp. 27–9), nicely captures the idea.

> Galileo's first trial of strength with the university professors was connected with his researches into the laws of motion as illustrated by falling bodies. It was an accepted axiom of Aristotle that the speed of falling bodies was regulated by their respective weights: thus, a stone weighing two pounds would fall twice as quick as one weighing only a single pound and so on. No one seems to have questioned the correctness of this rule, until Galileo gave it his denial. He declared that weight had nothing to do with the matter, and that … two bodies of unequal weight … would reach the ground at the same moment. As Galileo's statement was flouted by the body of professors, he determined to put it to a public test. So he invited the whole University to witness the experiment which he was about to perform from the leaning tower. On the morning of the day fixed, Galileo, in the presence of the assembled University and townsfolk, mounted to the top of the tower, carrying with him two balls, one weighing one hundred pounds and the other weighing one pound. Balancing the balls carefully on the edge of the parapet, he rolled them over together; they were seen to fall evenly, and the next instant, with a load clang, they struck the ground together. The old tradition was false, and modern science, in the person of the young discoverer, had vindicated her position.

Two schools of thought that involve attempts to formalise what I have called a common view of science, that scientific knowledge is derived from the fact, are the empiricists and the positivists. The British empiricists of the seventeenth and eighteenth centuries, notably John Locke, George Berkeley and David Hume, held that all knowledge should be derived from ideas implanted in the mind by way of sense perception. The positivists had a somewhat broader and less psychologically orientated view of what facts amount to, but shared the view of the empiricists that knowledge should be derived from the facts of experience. The logical positivists, a school of philosophy that originated in Vienna in the 1920s, took up the positivism that had been introduced by Auguste Comte in the nineteenth century and attempted to formalise it, paying close attention to the logical form of the relationship between scientific knowledge and the facts. Empiricism and positivism share the common view that scientific knowledge should in some way be derived from the facts arrived at by observation.

There are two rather distinct issues involved in the claim that science is derived from the facts. One concerns the nature of these "facts" and how scientists are meant to have access to them. The second concerns how the laws and theories that constitute our knowledge are derived from the facts once they have been obtained. We will investigate these two issues in turn, devoting this and the next two chapters to a discussion of the nature of the facts on which science is alleged to be based and chapter 4 to the question of how scientific knowledge might be thought to be derived from them.

Three components of the stand on the facts assumed to be the basis of science in the common view can be distinguished. They are:

(a) Facts are directly given to careful, unprejudiced observers via the senses.
(b) Facts are prior to and independent of theory.
(c) Facts constitute a firm and reliable foundation for scientific knowledge.

As we shall see, each of these claims is faced with difficulties and, at best, can only be accepted in a highly qualified form.

Seeing is believing

Partly because the sense of sight is the sense most extensively used to observe the world, and partly for convenience, I will restrict my discussion of observation to the realm of seeing. In most cases, it will not be difficult to see how the argument presented could be re-cast so as to be applicable to the other senses. A simple account of seeing might run as follows. Humans see using their eyes. The most important components of the human eye are a lens and a retina, the latter acting as a screen on which images of objects external to the eye are formed by the lens. Rays of light from a viewed object pass from the object to the lens via the intervening medium. These rays are refracted by the material of the lens in such a way that they are brought to a focus on the retina, so forming an image of the object. Thus far, the functioning of the eye is analogous to that of a camera. A big difference is in the way the final image is recorded. Optic nerves pass from the retina to the central cortex of the brain. These carry information concerning the light striking the various regions of the retina. It is the recording of this information by the brain that constitutes the seeing of the object by the human observer. Of course, many details could be added to this simplified description, but the account offered captures the general idea.

Two points are strongly suggested by the forgoing account of observation through the sense of sight that are incorporated into the common or empiricist view of science. The first is that a human observer has more or less direct access to knowledge of some facts about the world insofar as they are recorded by the brain in the act of seeing. The second is that two normal observers viewing the same object or scene from the same place will "see" the same thing. An identical combination of light rays will strike the eyes of each observer, will be focused on their normal retinas by their normal eye lenses and give rise to similar images. Similar information will then travel to the brain of each observer via their normal optic nerves, resulting in the two observers seeing the same thing. In subsequent sections we will see why this kind of picture is seriously misleading.

Visual experiences not determined solely by the object viewed

In its starkest form, the common view has it that facts about the external world are directly given to us through the sense of sight. All we need to do is confront the world before us and record what is there to be seen. I can establish that there is a lamp on my desk or that my pencil is yellow simply by noting what is before my eyes. Such a view can be backed up by a story about how the eye works , as we have seen. If this was all there was to it, then what is seen would be determined by the nature of what is looked at, and observers would always have the same visual experiences when confronting the same scene. However, there is plenty of evidence to indicate that this is simply not the case. Two normal observers viewing the same object from the same place under the same physical circumstances do not necessarily have identical visual experiences, even though the images on their respective retinas may be virtually identical. There is an important sense in which two observers need not "see" the same thing. As N. R. Hanson (1958) has put it, "there is more to seeing than meets the eyeball". Some simple examples will illustrate the point.

Most of us, when first looking at Figure 1, see the drawing of a staircase with the upper surface of the stairs visible. But this is not the only way in which it can be seen. It can without

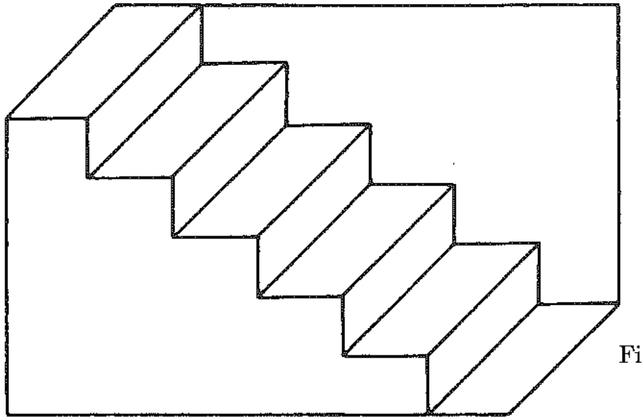

Figure 1

difficulty be seen as a staircase with the under surface of the stairs visible. Further, if one looks at the picture for some time, one generally finds that what one sees changes frequently, and involuntarily, from a staircase viewed from above to one viewed from below and back again. And yet it seems reasonable to suppose that, since it remains the same object viewed by the observer, the retinal images do not change. Whether the picture is seen as a staircase viewed from above or one viewed from below seems to depend on something other than the image on the retina of the viewer. I suspect that no reader of this book has questioned my claim that Figure 1 depicts a staircase. However, the results of experiments on members of African tribes whose culture does not include the custom of depicting three-dimensional objects by two-dimensional perspective drawings, nor staircases for that matter, indicate that members of those tribes would not see Figure 1 as a staircase at all. Again, it seems to follow that the perceptual experiences that individuals have in the act of seeing are not uniquely determined by the images on their retinas. Hanson (1958, chapter 1) contains some more captivating examples that illustrate this point.

Another instance is provided by a children's picture puzzle that involves finding the drawing of a human face among the foliage in the drawing of a tree. Here, what is seen, that is, the subjective impressions experienced by a person viewing the drawing, at first corresponds to a tree, with trunk, branches and leaves. But this changes once the human face has been detected. What was once seen as branches and leaves is now seen as a human face. Again, the same physical object is viewed before and after the solution of the puzzle, and presumably the image on the observer's retina does not change at the moment the puzzle is solved and the face found. If the picture is viewed at some later time, the face is readily and quickly seen by an observer who has already solved the puzzle once. It would seem that there is a sense in which what an observer sees is affected by his or her past experience.

"What", it might well be suggested, "have these contrived examples got to do with science?" In response, it is not difficult to produce examples from the practice of science that illustrate the same point, namely, that what observers see, the subjective experiences that they undergo, when viewing an object or scene is not determined solely by the images on their retinas but depends also on the experience, knowledge and expectations of the observer. The point is implicit in the uncontroversial realisation that one has to learn to be a competent observer in science. Anyone who has been through the experience of having to learn to see through a microscope will need no convincing of this. When the beginner looks at a slide prepared by an instructor through a microscope it is rare that the appropriate cell structures can be discerned, even though the instructor has no difficulty discerning them when looking at the same slide through the same microscope. It is significant to note, in this context, that microscopists found no great difficulty observing cells divide in suitably prepared circumstances once they were alert for what to look for, whereas prior to this discovery these cell divisions went unobserved, although we now know they must have been there to be observed in many of the samples examined through a microscope. Michael Polanyi (1973, p. 101) describes the changes in a medical student's perceptual experience when he is taught to make a diagnosis by inspecting an X-ray picture.

> Think of a medical student attending a course in the X-ray diagnosis of pulmonary diseases. He watches, in a darkened room, shadowy traces on a fluorescent screen placed against a patient's chest, and hears the radiologist commenting to his assistants, in technical language, on the significant features of these shadows. At first, the student is completely puzzled. For he can see in the X-ray picture of a chest only the shadows of the heart and ribs, with a few spidery blotches between them, The experts seem to be romancing about figments of their imagination; he can see nothing that they are talking about. Then, as he goes on listening for a few weeks, looking carefully at ever-new pictures of different cases, a tentative understanding will dawn on him; he will gradually forget about the ribs and begin to see the lungs. And eventually, if he perseveres intelligently, a rich panorama of significant details will be revealed to him; of physiological variations and pathological changes, of scars, of chronic infections and signs of acute disease. He has entered a new world. He still sees only a fraction of what the experts can see, but the pictures are definitely making sense now and so do most of the comments made on them.

The experienced and skilled observer does not have perceptual experiences identical to those of the untrained novice when the two confront the same situation. This clashes with a literal understanding of the claim that perceptions are given in a straightforward way via the senses.

A common response to the claim that I am making about observation, supported by the kinds of examples I have utilised, is that observers viewing the same scene from the same place see the same thing but interpret what they see differently. I wish to dispute this. As far as perception is concerned, the only things with which an observer has direct and immediate contact are his or her experiences. These experiences are not uniquely given and unchanging but vary with the knowledge and expectations possessed by the observer. What is uniquely given by the physical situation, I am prepared to admit, is the image on the retina of an observer, but an observer does not have direct perceptual contact with that image. When defenders of the common view assume that there is something unique given to us in perception that can be interpreted in various ways, they are assuming without argument, and in spite of much evidence to the contrary, that the images on our retinas uniquely determine our perceptual experiences. They are taking the camera analogy too far.

Having said all this, let me try to make clear what I do *not* mean to be claiming in this section, lest I be taken to be arguing for more than I intend to be. First, I am certainly not claiming that the physical causes of the images on our retinas have nothing to do with what we see. We cannot see just what we like. However, although the images on our retinas form part of the cause of what we see, another very important part of the cause is the inner state of our minds or brains, which will itself depend on our cultural upbringing, our knowledge and our expectations, and will not be determined solely by the physical properties of our eyes and the scene observed. Second, under a wide variety of circumstances, what we see in various situations remains fairly stable. The dependence of what we see on the state of our minds or brains is not so sensitive as to make communication, and science, impossible. Third, in all the examples quoted here, there is a sense in which all observers see the same thing. I accept and presuppose throughout this book that a single, unique, physical world exists independently of observers. Hence, when a number of observers look at a picture, a piece of apparatus, a microscope slide or whatever, there is a sense in which they are confronted by, look at, and hence see, the same thing. But it does not follow from this that they have identical perceptual experiences. There is a very important sense in which they do not see the same thing, and it is that latter sense on which I base some of my queries concerning the view that facts are unproblematically and directly given to observers through the senses. To what extent this undermines the view that facts adequate for science can be established by the senses remains to be seen.

Observable facts expressed as statements

In normal linguistic usage, the meaning of "fact" is ambiguous. It can refer to a statement that expresses the fact and it can also refer to the state of affairs referred to by such a statement. For example, it is a fact that there are mountains and craters on the moon. Here the fact can be taken as referring to the mountains or craters themselves. Alternatively, the statement "there are mountains and craters on the moon" can be taken as constituting the fact. When it is claimed that science is based on and derived from the facts, it is clearly the latter interpretation that is appropriate. Knowledge about the moon's surface is not based on and derived from mountains and craters but from factual statements about mountains and craters.

As well as distinguishing facts, understood as statements, from the states of affairs described by those statements, it is also clearly necessary to distinguish statements of facts from the perceptions that might occasion the acceptance of those statements as facts. For example, it is undoubtedly the case that when Darwin underwent his famous voyage on the *Beagle* he encountered many novel species of plant and animal, and so was subject to a range of novel perceptual experiences. However, he would have made no significant contribution to science had he left it at that. It was only when he had formulated statements describing the novelties and made them available to other scientists that he made a significant contribution to biology. To the extent that the voyage on the *Beagle* yielded novel facts from which an evolutionary theory could be derived, or to which an evolutionary theory could be related, it was statements that constituted those facts. For those who wish to claim that knowledge is derived from facts, they must have statements in mind, and neither perceptions nor objects like mountains and craters.

With this clarification behind us, let us return to the claims (a) to (c) about the nature of facts which concluded the first section of this chapter. Once we do so they immediately become highly problematic as they stand. Given that the facts that might constitute a suitable basis for science must be in the form of statements, the claim that facts are given in a straightforward way via the senses begins to look quite misconceived. For even if we set aside the difficulties highlighted in the previous section, and assume that perceptions are straightforwardly given in the act of seeing, it is clearly not the case that statements describing observable states of affairs (I will call them observation statements) are given to observers via the senses. It is absurd to think that *statements* of fact enter the brain by way of the senses.

Before an observer can formulate and assent to an observation statement, he or she must be in possession of the appropriate conceptual framework and a knowledge of how to appropriately apply it. That this is so becomes clear when we contemplate the way in which a child learns to describe (that is, make factual statements about) the world. Think of a parent teaching a child to recognise and describe apples. The parent shows the child an apple, points to it, and utters the word "apple". The child soon learns to repeat the word "apple" in imitation. Having mastered this particular accomplishment, perhaps on a later day the child encounters its sibling's tennis ball, points and says "apple". At this point the parent intervenes to explain that the ball is not an apple, demonstrating, for example, that one cannot bite it like an apple. Further mistakes by the child, such as the identification of a choko as an apple, will require somewhat more elaborate explanations from the parent. By the time the child can successfully say there is an apple present when there is one, it has learnt quite a lot about apples. So it would seem that it is a mistake to presume that we must first observe the facts about apples before deriving knowledge about them from those facts, because the appropriate facts, formulated as statements, presuppose quite a lot of knowledge about apples.

Let us move from talk of children to some examples that are more relevant to our task of understanding science. Imagine a skilled botanist accompanied by someone like myself who is largely ignorant of botany taking part in a field trip into the Australian bush, with the objective of collecting observable facts about the native flora. It is undoubtedly the case that the botanist will be capable of collecting facts that are far more numerous and discerning than those I am able to observe and formulate, and the reason is clear. The botanist has a more elaborate conceptual scheme to exploit than myself, and that is because he or she knows more botany than I do. A knowledge of botany is a prerequisite for the formulation of the observation statements that might constitute its factual basis.

Thus, the recording of observable facts requires more than the reception of the stimuli, in the form of light rays, that impinge on the eye. It requires the knowledge of the appropriate conceptual scheme and how to apply it. In this sense, assumptions (a) and (b) cannot be accepted as they stand. Statements of fact are not determined in a straightforward way by sensual stimuli, and observation statements presuppose knowledge, so it cannot be the case that we first establish the facts and then derive our knowledge from them.

Why should facts precede theory?

I have taken as my starting point a rather extreme interpretation of the claim that science is derived from the facts. I have taken it to imply that the facts must be established prior to the derivation of scientific knowledge from them. First establish the facts and then build your theory to fit them. Both the fact that our perceptions depend to some extent on our prior knowledge and hence on our state of preparedness and our expectations (discussed earlier in the chapter) and the fact that observation statements presuppose the appropriate conceptual framework (discussed in the previous section) indicate that it is a demand that is impossible to live up to. Indeed, once it is subject to a close inspection it is a rather silly idea, so silly that I doubt if any serious philosopher of science would wish to defend it. How can we establish significant facts about the world through observation if we do not have some guidance as to what kind of knowledge we are seeking or what problems we are trying to solve? In order to make observations that might make a significant contribution to botany, I need to know much botany to start with. What is more, the very idea that the adequacy of our scientific knowledge should be tested against the observable facts would make no sense if, in proper science, the relevant facts must always precede the knowledge that might be supported by them. Our search for relevant facts needs to be guided by our current state of knowledge, which tells us, for example, that measuring the ozone concentration at various locations in the atmosphere yields relevant facts, whereas measuring the average hair length of the youths in Sydney does not. So let us drop the demand that the acquisition of facts should come before the formulation of the laws and theories that constitute scientific knowledge, and see what we can salvage of the idea that science is based on the facts once we have done so.

According to our modified stand, we freely acknowledge that the formulation of observation statements presupposes significant knowledge, and that the search for relevant observable facts in science is guided by that knowledge. Neither acknowledgment necessarily undermines the claim that knowledge has a factual basis established by observation. Let us first take the point that the formulation of significant observation statements presupposes knowledge of the appropriate conceptual framework. Here we note that the availability of the conceptual resources for formulating observation statements is one thing. The truth or falsity of those statements is another. Looking at my solid state physics textbook, I can extract two observation statements, "the crystal structure of diamond has inversion symmetry" and "in a crystal of zinc sulphide there are four molecules per unit cell". A degree of knowledge about crystal structures and how they are characterised is necessary for the formulation and understanding of these statements. But even if you do not have that knowledge, you will be able to recognise that there are other, similar, statements that can be formulated using the same terms, statements such as "the crystal structure of diamond does not have inversion symmetry" and "the crystal of diamond has four molecules per unit cell". All of these statements are observation statements in the sense that once one has mastered the appropriate observational techniques their truth or falsity can be established by observation. When this is done, only the statements I extracted from my textbook are confirmed by observation, while the alternatives constructed from them are refuted. This illustrates the point that the fact that knowledge is necessary for the formulation of significant observation statements still leaves open the question of which of the statements so formulated are borne out by observation and which are not. Consequently, the idea that knowledge should be based on facts that are confirmed by observation is not undermined by the recognition that the formulation of the statements describing those facts are knowledge-dependent. There is only a problem if one sticks to the silly demand that the confirmation of facts relevant to some body of knowledge should precede the acquisition of any knowledge.

The idea that scientific knowledge should be based on facts established by observation need not be undermined, then, by the acknowledgment that the search for and formulation of those facts are knowledge-dependent. If the truth or falsity of observation statements can be established in a direct way by observation, then, irrespective of the way in which those statements came to be formulated, it would seem that the observation statements confirmed in this way provide us with a significant factual basis for scientific knowledge.

The fallibility of observation statements

We have made some headway in our search for a characterisation of the observational base of science, but we are not out of trouble yet. In the previous section our analysis presupposed that the truth or otherwise of observation statements can be securely established by observation in an unproblematic way. But is such a presupposition legitimate? We have already seen ways in which problems can arise from the fact that different observers do not necessarily have the same perceptions when viewing the same scene, and this can lead to disagreements about what the observable states of affairs are. The significance of this point for science is borne out by well-documented cases in the history of science, such as the dispute about whether or not the effects of so-called N-rays are observable, described by Nye (1980), and the disagreement between Sydney and Cambridge astronomers over what the observable facts were in the early years of radio astronomy, as described by Edge and Mulkay (1976). We have as yet said little to show how a secure observational basis for science can be established in the face of such difficulties. Further difficulties concerning the reliability of the observational basis of science arise from some of the ways in which judgments about the adequacy of observation statements draw on presupposed knowledge in a way that renders those judgments fallible. I will illustrate this with examples.

Aristotle included fire among the four elements of which all terrestrial objects are made. The assumption that fire is a distinctive substance, albeit a very light one, persisted for hundreds of years, and it took modern chemistry to thoroughly undermine it. Those who worked with this presupposition considered themselves to be observing fire directly when watching flames rise into the air, so that for them "the fire ascended" is an observation statement that was frequently borne out by direct observation. We now reject such observation statements. The point is that if the knowledge that provides the categories we use to describe our observations is defective, the observation statements that presuppose those categories are similarly defective.

My second example concerns the realisation, established in the sixteenth and seventeenth centuries, that the earth moves, spinning on its axis and orbiting the sun. Prior to the circumstances that made this realisation possible, it can be said that the statement "the earth is stationary" was a fact confirmed by observation. After all, one cannot see or feel it move, and if we jump in the air, the earth does not spin away beneath us. We, from a modern perspective, know that the observation statement in question is false in spite of these appearances. We understand inertia, and know that if we are moving in a horizontal direction at over one hundred metres per second because the earth is spinning, there is no reason why that should change when we jump in the air. It takes a force to change speed, and, in our example, there are no horizontal forces acting. So we retain the horizontal speed we share with the earth's surface and land where we took off. "The earth is stationary" is not established by the observable evidence in the way it was once thought to be. But to fully appreciate why this is so, we need to understand inertia. That understanding was a seventeenth-century innovation. We have an example that illustrates a way in which the judgment of the truth or otherwise of an observation statement depends on the knowledge that forms the background against which the judgment is made. It would seem that the scientific revolution involved not just a progressive transformation of scientific theory, but also a transformation in what were considered to be the observable facts!

This last point is further illustrated by my third example. It concerns the sizes of the planets Venus and Mars as viewed from earth during the course of the year. It is a consequence of Copernicus's suggestion that the earth circulates the sun, in an orbit outside that of Venus and inside that of Mars, that the apparent size of both Venus and Mars should change appreciably during the course of the year. This is because when the earth is around the same side of the sun as one of those planets it is relatively close to it, whereas when it is on the opposite side of the sun to one of them it is relatively distant from it. When the matter is considered quantitatively, as it can be within Copernicus's own version of his theory, the effect is a sizeable one, with a predicted change in apparent diameter by a factor of about eight in the case of Mars and

about six in the case of Venus. However, when the planets are observed carefully with the naked eye, no change in size can be detected for Venus, and Mars changes in size by no more than a factor of two. So the observation statement "the apparent size of Venus does not change size during the course of the year" was straightforwardly confirmed, and was referred to in the Preface to Copernicus's *On the Revolutions of the Heavenly Spheres* as a fact confirmed "by all the experience of the ages" (Duncan, 1976, p. 22). Osiander, who was the author of the Preface in question, was so impressed by the clash between the consequences of the Copernican theory and our "observable fact" that he used it to argue that the Copernican theory should not be taken literally. We now know that the naked-eye observations of planetary sizes are deceptive, and that the eye is a very unreliable device for gauging the size of small light sources against a dark background. But it took Galileo to point this out and to show how the predicted change in size can be clearly discerned if Venus and Mars are viewed through a telescope. Here we have a clear example of the correction of a mistake about the observable facts made possible by improved knowledge and technology. In itself the example is unremarkable and non-mysterious. But it does show that any view to the effect that scientific knowledge is based on the facts acquired by observation must allow that the facts as well as the knowledge are fallible and subject to correction and that scientific knowledge and the facts on which it might be said to be based are interdependent.

The intuition that I intended to capture with my slogan "science is derived from the facts" was that scientific knowledge has a special status in part because it is founded on a secure basis, solid facts firmly established by observation. Some of the considerations of this chapter pose a threat to this comfortable view. One difficulty concerns the extent to which perceptions are influenced by the background and expectations of the observer, so that what appears to be an observable fact for one need not be for another. The second source of difficulty stems from the extent to which judgments about the truth of observation statements depend on what is already known or assumed, thus rendering the observable facts as fallible as the presuppositions underlying them. Both kinds of difficulty suggest that maybe the observable basis for science is not as straightforward and secure as is widely and traditionally supposed. In the next chapter I try to mitigate these fears to some extent by considering the nature of observation, especially as it is employed in science, in a more discerning way than has been involved in our discussion up until now.

Further reading

For a classic discussion of how knowledge is seen by an empiricist as derived from what is delivered to the mind via the senses, see Locke (1967), and by a logical positivist, see Ayer (1940). Hanfling (1981) is an introduction to logical positivism generally, including its account of the observational basis of science. A challenge to these views at the level of perception is Hanson (1958, chapter 1). Useful discussions of the whole issue are to be found in Brown (1977) and Barnes, Bloor and Henry (1996, chapters 1–3).

CHAPTER 2

Observation as practical intervention

Observation: passive and private or public and active?

A common way in which observation is understood by a range of philosophers is to see it as a passive, private affair. It is passive insofar as it is presumed that when seeing, for example, we simply open and direct our eyes, let the information flow in, and record what is there to be seen. It is the perception itself in the mind or brain of the observer that is taken to directly validate the fact, which may be "there is a red tomato in front of me" for example. If it is understood in this way, then the establishment of observable facts is a very private affair. It is accomplished by the individual closely attending to what is presented to him or her in the act of perception. Since two observers do not have access to each other's perceptions, there is no way they can enter into a dialogue about the validity of the facts they are presumed to establish.

This view of perception or observation, as passive and private, is totally inadequate, and does not give an accurate account of perception in everyday life, let alone science. Everyday observation is far from passive. There are a range of things that are *done*, many of them automatically and perhaps unconsciously, to establish the validity of a perception. In the act of seeing we scan objects, move our heads to test for expected changes in the observed scene and so on. If we are not sure whether a scene viewed through a window is something out of the window or a reflection in the window, we can move our heads to check for the effect this has on the direction in which the scene is visible. It is a general point that if for any reason we doubt the validity of what seems to be the case on the basis of our perceptions, there are various actions we can take to remove the problem. If, in the example