A. F. Chalmers

What is this thing called Science?

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Preface to the first edition

This book is intended to be a simple, clear and elementary introduction to modern views about the nature of science. When teaching philosophy of science, either to philosophy undergraduates or to scientists wishing to become familiar with recent theories about science, I have become increasingly aware that there is no suitable single book, or even a small number of books, that one can recommend to the beginner. The only sources on the modern views that are available are the original ones. Many of these are too difficult for beginners, and in any case they are too numerous to be made easily available to a large number of students. This book will be no substitute for the original sources for anyone wishing to pursue the topic seriously, of course, but I hope it will provide a useful and easily accessible starting point that does not otherwise exist.

My intention of keeping the discussion simple proved to be reasonably realistic for about two-thirds of the book. By the time I had reached that stage and had begun to criticise the modern views, I found, to my surprise, first, that I disagreed with those views more than I had thought and, second, that from my criticism a fairly coherent alternative was emerging. That alternative is sketched in the latter chapters of the book. It would be pleasant for me to think that the second half of this book contains not only summaries of current views on the nature of science but also a summary of the next view.

My professional interest in history and philosophy of science began in London, in a climate that was dominated by the views of Professor Karl Popper. My debt to him, his writings, his lectures and his seminars, and also to the late Professor Imre Lakatos, must be very evident from the contents of this book. The form of the first half of it owes much to Lakatos's brilliant article on the methodology of research programs. A noteworthy feature of the Popperian school was the pressure it put on one to be clear about the problem one was interested in and to express one's views on it in a simple and straightforward way. Although I owe much to the example of Popper and Lakatos in this respect, any ability that I have to express myself simply and clearly stems mostly from my

interaction with Professor Heinz Post, who was my supervisor at Chelsea College while I was working on my doctorial thesis in the Department of History and Philosophy of Science there. I cannot rid myself of an uneasy feeling that his copy of this book will be returned to me along with the demand that I rewrite the bits he does not understand. Of my colleagues in London to whom I owe a special debt, most of them students at the time, Noretta Koertge, now at Indiana University, helped me considerably.

I referred above to the Popperian school as a *school*, and yet it was not until I came to Sydney from London that I fully realised the extent to which I had been in a school. I found, to my surprise, that there were philosophers influenced by Wittgenstein or Quine or Marx who thought that Popper was quite wrong on many issues, and some who even thought that his views were positively dangerous. I think I have learnt much from that experience. One of the things that I have learnt is that on a number of major issues Popper is indeed wrong, as is argued in the latter portions of this book. However, this does not alter the fact that the Popperian approach is infinitely better than the approach adopted in most philosophy departments that I have encountered.

I owe much to my friends in Sydney who have helped to waken me from my slumber. I do not wish to imply by this that I accept their views rather than Popperian ones. They know better than that. But since I have no time for obscurantist nonsense about the incommensurability of frameworks (here Popperians prick up their ears), the extent to which I have been forced to acknowledge and counter the views of my Sydney colleagues and adversaries has led me to understand the strengths of their views and the weaknesses of my own. I hope I will not upset anyone by singling out Jean Curthoys and Wal Suchting for special mention here.

Lucky and attentive readers will detect in this book the odd metaphor stolen from Vladimir Nabokov, and will realise that I owe him some ackowledgment (or apology).

I conclude with a warm 'hello' to those friends who don't care about the book, who won't read the book, and who had to put up with me while I wrote it.

Alan Chalmers
Sydney, 1976

Preface to the second edition

Judging by responses to the first edition of this book it would seem that the first eight chapters of it function quite well as 'a simple, clear and elementary introduction to modern views about the nature of science'. It also seems to be fairly universally agreed that the last four chapters fail to do so. Consequently, in this revised and extended edition I have left chapters 1–8 virtually unchanged and have replaced the last four chapters by six entirely new ones. One of the problems with the latter part of the first edition was that it ceased to be simple and elementary. I have tried to keep my new chapters simple, although I fear I have not entirely succeeded when dealing with the difficult issues of the final two chapters. Although I have tried to keep the discussion simple, I hope I have not thereby become uncontroversial.

Another problem with the latter part of the first edition is lack of clarity. Although I remain convinced that most of what I was groping for there was on the right track, I certainly failed to express a coherent and well-argued position, as my critics have made clear. Not all of this can be blamed on Louis Althusser, whose views were very much in vogue at the time of writing, and whose influence can still be discerned to some extent in this new edition. I have learnt my lesson and in future will be very wary of being unduly influenced by the latest Paris fashions.

My friends Terry Blake and Denise Russell have convinced me that there is more of importance in the writings of Paul Feyerabend than I was previously prepared to admit. I have given him more attention in this new edition and have tried to separate the wheat from the chaff, the anti-methodism from the dadaism. I have also been obliged to separate the important sense from 'obscurantist nonsense about the incommensurability of frameworks'.

The revision of this book owes much to the criticism of numerous colleagues, reviewers and correspondents. I will not attempt to name them all, but acknowledge my debt and offer my thanks.

Since the revision of this book has resulted in a new ending, the original point of the cat on the cover has been lost. However, the cat does seem to have a considerable following, despite her lack of whiskers, so we have retained her, and merely ask readers to reinterpret her grin.

Alan Chalmers
Sydney, 1981

Preface to the third edition

This edition represents a major reworking of the previous edition, in which very few of the original chapters have emerged unscathed and many have been replaced. There are also a number of new chapters. The changes were necessary for two reasons. First, the teaching of an introductory course in the philosophy of science that I have undertaken in the twenty years since first writing this book has taught me how to do the job better. Second, there have been important developments in the philosophy of science in the last decade or two that need to be taken account of in any introductory text.

A currently influential school in the philosophy of science involves an attempt to erect an account of science on Bayes' theorem, a theorem in the probability calculus. A second trend, 'the new experimentalism', involves paying more attention than hitherto to the nature and role of experiment in science. Chapters 12 and 13, respectively, contain a description and an appraisal of these schools of thought. Recent work, especially that of Nancy Cartwright, has brought to the fore questions about the nature of laws as they figure in science, so a chapter on this topic is included in this new edition, as is a chapter that aims to keep abreast of the debate between realist and anti-realist interpretations of science.

So while not pretending that I have arrived at the definitive answer to the question that forms the title of this book, I have endeavoured to keep abreast of the contemporary debate and to introduce the reader to it in a way that is not too technical. There are suggestions for further reading at the end of each chapter, which will be a useful and up-to-date starting point for those who wish to pursue these matters in greater depth.

I will not attempt to name all the colleagues and students from whom I have learnt how to improve this book. I learnt much at an international symposium held in Sydney in June 1997, 'What Is This Thing Called Science? Twenty Years On'. I thank the sponsors of that symposium, The British Council, the University of Queensland Press, the Open University Press, Hackett Publishing

Company and Uitgeverij Boom, and those colleagues and old friends who attended and participated in the proceedings. The event did much to boost my morale and gave me the incentive to undertake the major task that was involved in rewriting the text. Much of the rewriting was done while I was a Research Fellow at the Dibner Institute for the History of Science and Technology, MIT, for which I express my appreciation. I could not have hoped for a more supportive environment, and one more conducive to some concentrated work. I thank Hasok Chang for his careful reading of the manuscript and his helpful comments.

I have lost track of what the cat is meant to be grinning about, but I seem to detect a note of continuing approval, which is reassuring.

Alan Chalmers
Cambridge, Mass., 1998

Preface to the fourth edition

Since this book first appeared in 1976 I have twice seen fit to write a new edition of it, removing passages, or even whole chapters, that I found unhelpful, wrong-headed or insufficiently clear, and adding new passages or chapters drawing on developments in the literature as well as on clarifications of my own thoughts. With such an end in view, I recently subjected the third edition to a critical reading. I did not find much with which I was dissatisfied as had been the case with my reappraisal of the first and second editions. I did, nevertheless, discern ways in which key themes in the book could be clarified and extended. The main source for this rethinking was the work that went into the writing of my book *The Scientist's Atom and the Philosopher's Stone: How Science Succeeded and Philosophy Failed to Gain Knowledge of Atoms.* The story of how scientific knowledge of atoms became possible proves to be a ready source of examples to illustrate and support my main points concerning the distinctive character of scientific as opposed to other kinds of knowledge. Accordingly, I have included a Postscript in this fourth edition that draws on this material to help clarify what this thing called science is.

My academic home for the first decade of the twenty-first century was the Philosophy Department at Flinders University in Adelaide. I thank my colleagues there, especially Rodney Allen, George Couvalis and Greg O'Hair, for helping to make that period productive. Of the many academics that have provided me with help, support and constructive criticism, Ursula Klein, Deborah Mayo, Alan Musgrave and John Norton deserve special mention. From 2003 to 2005 my work was supported by a grant from the Australian Research Council. I benefited from Research Fellowships at the University of Canterbury, New Zealand, and the University of Pittsburgh and from a semester in the Department of Philosophy at the University of Bristol. All of this support was very helpful and much appreciated. Sandra Grimes has been a constant and much appreciated and valued, if unduly acknowledged, support.

Alan Chalmers
Sydney, 2012

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 well 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 boil down to choices determined by the subjective values and wishes of individuals.

Feyerabend's skepticism about attempts to rationalise science is shared by more recent authors 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 captured 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 assesmbled 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 loud 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 other 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 foregoing 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 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

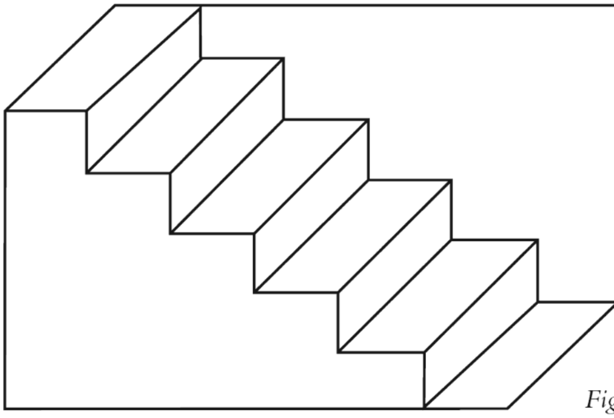

Figure 1

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 out 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 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 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 zinc sulphide has six 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 active and public?

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 is 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 above, we have reason to suspect that the image of the tomato is some cleverly contrived optical image rather than a real tomato, we can touch it as well as look at it, and if necessary we can taste it or dissect it.

With these few, somewhat elementary, observations I have only touched the surface of the detailed story psychologists can tell about the range of things that are done by individuals in the act of perception. More important for our task is to consider the significance of the point for the role of observation in science. An example that illustrates my point well is drawn from early uses of the microscope in science. When scientists such as Robert Hooke and Henry Power used the microscope to look at small insects such as flies and ants, they often disagreed about the observable facts, at least initially. Hooke traced the cause of some of the disagreements to different kinds of illumination. He pointed out that the eye of a fly appears like a lattice covered with holes in one kind of light (which, incidentally, seems to have led Power to believe that this was indeed the case), like a surface covered with cones in another and in yet another light like a surface covered with pyramids. Hooke proceeded to make practical interventions designed to clear up the problem. He endeavoured to eliminate spurious information arising from dazzle and complicated reflections by illuminating specimens uniformly. He did this by using for illumination the light of a candle diffused through a solution of brine. He also illuminated his specimens from various directions to determine which features remained invariant under such changes. Some of the insects needed to be thoroughly intoxicated with brandy to render them both motionless and undamaged.

Hooke's book, *Micrographia* (1665), contains many detailed descriptions and drawings that resulted from Hooke's actions and observations. These productions were and are public, not private. They can be checked, criticised and added to by others. If a fly's eye, in some kinds of illumination, appears to be covered with holes, then that state of affairs cannot be usefully evaluated by the observer closely attending to his or her perceptions. Hooke showed what could be *done* to check the authenticity of the appearances in such cases, and the procedures he recommended could be carried out by anyone suitably inclined or skilled. The observable facts about the structure of a fly's eye that eventuate result from a process that is both active and public.

The point that action can be taken to explore the adequacy of claims put forward as observable facts has the consequence that subjective aspects of perception need not be an intractable problem for science. Ways in which perceptions of the same scene can vary from observer to observer depending on their background, culture and expectations were discussed in the previous chapter. Problems that eventuate from this undoubted fact can be countered to a large extent by taking appropriate action. It should be no news to anyone that the perceptual judgments of individuals can be unreliable for a range of reasons. The challenge, in science, is to arrange the observable situation in such a way that the reliance on such judgments is minimised if not eliminated. An example will illustrate the point.

The moon illusion is a common phenomenon. When it is high in the sky, the moon appears much smaller than when it is low on the horizon. This is an illusion. The moon does not change size nor does its distance from earth alter during the few hours that it takes for its relative position to undergo the required change. However, we do not have to put our trust in subjective judgments about the moon's size. We can, for example, mount a sighting tube fitted with cross-wires in such a way that its orientation can be read on a scale. The angle subtended by the moon at the place of sighting can be determined by aligning the cross-wires with each side of the moon in turn and noting the difference in the corresponding scale readings. This can be done when the moon is high in the sky and repeated when it is near the horizon. The fact that the apparent size of the moon has remained unchanged is reflected in the fact that there is no significant variation in the differences between the scale readings in the two cases.

Galileo and the moons of Jupiter

In this section the relevance of the discussion in the previous section is illustrated with a historical example. Late in 1609 Galileo constructed a powerful telescope and used it to look at the heavens. Many of the novel observations he made in the ensuing three months were controversial, and very relevant to the astronomical debate concerning the validity of the Copernican theory, of which Galileo became an avid champion. Galileo claimed, for instance, to have sighted four moons orbiting the planet Jupiter, but he had trouble convincing others of the validity of his observations. The matter was of some moment. The Copernican theory involved the controversial claim that the earth moves, spinning on its axis once a day and orbiting the sun once a year. The received view that Copernicus had challenged in the first half of the previous century was that the earth is stationary, with the sun and planets orbiting it. One of the many, far from trivial, arguments against the motion of the earth was that, if it orbited the sun as Copernicus claimed, the moon would be left behind. This argument is undermined once it is acknowledged that Jupiter has moons. For even the opponents of Copernicus agreed that Jupiter moves. Consequently, any moons it has are carried with it, exhibiting the very phenomenon that the opponents of Copernicus claimed to be impossible in the case of the earth.

Whether Galileo's telescopic observations of moons around Jupiter were valid was a question of some moment then. In spite of the initial skepticism, and the apparent inability of a range of his contemporaries to discern the moons through the telescope, Galileo had convinced his rivals within a period of two years. Let us see how he was able to achieve that – how he was able to 'objectify' his observations of Jupiter's moons.

Galileo attached a scale, marked with equally spaced horizontal and vertical lines, to his telescope by a ring in such a way that the scale was face-on to the observer and could be slid up and down the length of the telescope. A viewer looking through the telescope with one eye could view the scale with the other. Sighting of the scale was facilitated by illuminating it with a small lamp. With the telescope trained on Jupiter, the scale was slid along the telescope until the image of Jupiter viewed through the telescope with one eye lay in the central square of the scale viewed with the other eye. With this accomplished, the position of a moon viewed through the telescope could be read on the scale, the reading corresponding to its distance from Jupiter in multiples of the diameter of Jupiter. The diameter of Jupiter was a convenient unit, since employing it as a standard automatically allowed for the fact that its apparent diameter as viewed from earth varies as that planet approaches and recedes from the earth.

Using this technique, Galileo was able to record the daily histories of the four 'starlets' accompanying Jupiter. He was able to show that the data were consistent with the assumption that the starlets were indeed moons orbiting Jupiter with a constant period. The assumption was borne out, not only by the quantitative measurements but also by the more qualitative observation that the satellites occasionally disappeared from view as they passed behind or in front of the parent planet or moved into its shadow.

Galileo was in a strong position to argue for the veracity of his observations of Jupiter's moons, in spite of the fact that they were invisible to the naked eye. He could, and did, argue against the suggestion that they were an illusion produced by the telescope by pointing out that that suggestion made it difficult to explain why the moons appeared near Jupiter and nowhere else. Galileo could also appeal to the consistency and repeatability of his measurements and their compatibility with the assumption that the moons orbit Jupiter with a constant period. Galileo's quantitative data were verified by independent observers, including observers at the Collegio Romano and the Court of the Pope in Rome who were opponents of the Copernican theory. What is more, Galileo was able to predict further positions of the moons and the occurrence of transits and eclipses, and these too were confirmed by himself and independent observers, as documented by Stillman Drake (1978, pp. 175–6, 236–7).

The veracity of the telescopic sightings was soon accepted by those of Galileo's contemporaries who were competent observers, even by those who had initially opposed him. It is true that some observers could never manage to discern the moons, but I suggest that this is of no more significance than the inability of James Thurber (1933, pp. 101–3) to discern the structure of plant cells through a microscope. The strength of Galileo's case for the veracity of his telescopic observations of the moons of Jupiter derives from the range of practical, objective tests that his claims could survive. Although his case might have stopped short of being absolutely conclusive, it was incomparably stronger than any that could be made for the alternative, namely, that his sightings were illusions or artifacts brought about by the telescope.

Observable facts objective but fallible

An attempt to rescue a reasonably strong version of what constitutes an observable fact from the criticisms that we have levelled at that notion might go along the following lines. An observation statement constitutes a fact worthy of forming part of the basis for science if it is such that it can be straightforwardly tested by the senses and withstands those tests. Here the 'straightforward' is intended to capture the idea that candidate observation statements should be such that their validity can be tested in ways that involve routine, objective procedures that do not necessitate fine, subjective judgments on the part of the observer. The emphasis on tests brings out the active, public character of the vindication of observation statements. In this way, perhaps we can capture a notion of fact unproblematically established by observation. After all, only a suitably addicted philosopher will wish to spend time doubting that such things as meter readings can be securely established, within some small margin of error, by careful use of the sense of sight.

A small price has to be paid for the notion of an observable fact put forward in the previous paragraph. That price is that observable facts are to some degree fallible and subject to revision. If a statement qualifies as an observable fact because it has passsed all the tests that can be levelled at it hitherto, this does not mean that it will necessarily survive new kinds of tests that become possible in the light of advances in knowledge and technology. We have already met two significant examples of observation statements that were accepted as facts on good grounds but were eventually rejected in the light of such advances, namely, 'the earth is stationary' and 'the apparent size of Mars and Venus does not change appreciably during the course of the year'.

According to the view put forward here, observations suitable for constituting a basis for scientific knowledge are both objective and fallible. They are objective insofar as they can be publicly tested by straightforward procedures, and they are fallible insofar as they may be undermined by new kinds of tests made possible by advances in science and technology. This point can be illustrated by another example from the work of Galileo. In his *Dialogue Concerning the Two Chief World Systems* (1967, pp. 361–3) Galileo described an objective method for measuring the diameter of a star. He hung a cord between himself and the star at a distance such that the cord just blocked out the star. Galileo argued that the angle subtended at the eye by the cord was then equal to the angle subtended at the eye by the star. We now know that Galileo's results were spurious. The apparent size of a star as perceived by us is due entirely to atmospheric and other noise effects and has no determinate relation to the star's physical size. Galileo's measurements of star-size rested on implicit assumptions that are now rejected. But this rejection has nothing to do with subjective aspects of perception. Galileo's observations were objective in the sense that they involved routine procedures which, if repeated today, would give much the same results as obtained by Galileo. In the next chapter we will have cause to develop further the point that the lack of an infallible observational base for science does not derive solely from subjective aspects of perception.

Further reading

For a classic discussion of the empirical basis of science as those statements that withstand tests, see Popper (1972, chapter 5). The active aspects of observation are stressed in the second half of Hacking (1983), in Popper (1979, pp. 341–61) and in Chalmers (1990, chapter 4). Also of relevance is Shapere (1982).

CHAPTER 3

Experiment

Not just facts but relevant facts

In this chapter I assume for the sake of argument that secure facts can be established by careful use of the senses. After all, as I have already suggested, there is a range of situations relevant to science where this assumption is surely justified. Counting clicks on a Geiger counter and noting the position of a needle on a scale are unproblematic examples. Does the availability of such facts solve our problem about the factual basis for science? Do the statements that we assume can be established by observation constitute the facts from which scientific knowledge can be derived? In this chapter we will see that the answer to these questions is a decisive 'no'.

One point that should be noted is that what is needed in science is not just facts but relevant facts. The vast majority of facts that can be established by observation, such as the number of books in my office or the colour of my neighbour's car, are totally irrelevant for science, and scientists would be wasting their time collecting them. Which facts are relevant and which are not relevant to a science will be relative to the current state of development of that science. Science poses the questions, and ideally observation can provide answers. This is part of the answer to the question of what constitutes a relevant fact for science.

However, there is a more substantial point to be made, which I will introduce with a story. When I was young, my brother and I disagreed about how to explain the fact that the grass grows longer among the cow pats in a field than elsewhere in the same field, a fact that I am sure we were not the first to notice. My brother was of the opinion that it was the fertilising effect of the dung that was responsible, whereas I suspected that it was a mulching effect, the dung trapping moisture beneath it and inhibiting evaporation. I now have a strong suspicion that neither of us was entirely right and that the main explanation is simply that cows are disinclined to eat the grass around their own dung. Presumably all three of these effects play some role, but it is not possible to sort out the relative magnitudes of the effects by observations of the kind made by my brother and me. Some intervention would be necessary, such as, for example, locking the cows out of a field for a season to see if this reduced or eliminated the longer growth among the cow pats, by grinding the dung in such a way that the mulching effect is eliminated but the fertilising effect retained, and so on.

The situation exemplified here is typical. Many kinds of processes are at work in the world around us, and they are all superimposed on, and interact with, each other in complicated ways. A falling leaf is subject to gravity, air resistance and the force of winds and will also rot to some small degree as it falls. It is not possible to arrive at an understanding of these various processes by careful observation of events as they typically and naturally occur. Observation of falling leaves will not yield Galileo's law of fall. The lesson to be learnt here is rather straightforward. To acquire facts relevant for the identification and specification of the various processes at work in nature it is, in general, necessary to practically intervene to try to isolate the process under investigation and eliminate the effects of others. In short, it is necessary to do experiments.

It has taken us a while to get to this point, but it should perhaps be somewhat obvious that if there are facts that constitute the basis for science, then those facts come in the form of experimental results rather than any old observable facts. As obvious as this might be, it is not until the last couple of decades that philosophers of science have taken a close look at the nature of experiment and the role it plays in science. Indeed, it is an issue that was given little attention in the previous editions of this book. Once we focus on experiment rather than mere observation as supplying the basis for science, the issues we have been discussing take on a somewhat different light, as we shall see in the remainder of this chapter.

The production and updating of experimental results

Experimental results are by no means straightforwardly given. As any experimentalist, and indeed any science student, knows, getting an experiment to work is no easy matter. A significant new experiment can take months or even years to successfully execute. A brief account of my own experiences as an experimental physicist in the 1960s will illustrate the point nicely. It is of no great importance whether the reader follows the detail of the story. I simply aim to give some idea of the complexity and practical struggle involved in the production of an experimental result.

The aim of my experiment was to scatter low-energy electrons from molecules to find out how much energy they lost in the process, thereby gaining information related to the energy levels in the molecules themselves. To reach this objective, it was necessary to produce a beam of electrons that all moved at the same velocity and hence had the same energy. It was necessary to arrange for them to collide with one target molecule only before entering the detector, otherwise the sought-for information would be lost, and it was necessary to measure the velocity, or energy, of the scattered electrons with a suitably designed detector. Each of these steps posed a practical challenge. The velocity selector involved two conducting plates bent into concentric circles with a potential difference between them. Electrons entering between the plates would only emerge from the other end of the circular channel if they had a velocity that matched the potential difference between the plates. Otherwise they would be deflected onto the conducting plates. To ensure that the electrons were likely to collide with only one target molecule it was necessary to do the experiment in a region that was highly evacuated, containing a sample of the target gas at very low pressure. This required pushing the available vacuum technology to its limits. The velocity of scattered electrons was to be measured by an arrangement of circular electrodes similar to that used in producing the mono-energetic beam. The intensity of electrons scattered with a particular velocity could be measured by setting the potential difference between the plates to a value that allowed only the electrons with that velocity to traverse the circle and emerge at the other end of the analyser. Detecting the emerging electrons involved measuring a minutely small current that again pushed the available technology to its limits.

That was the general idea, but each step presented a range of practical problems of a sort that will be familiar to anyone who has worked in this kind of field. It was very difficult to rid the apparatus of unwanted gases that were emitted from the various metals from which the apparatus was made. Molecules of these gases that were ionised by the electron beam could coagulate on the electrodes and cause spurious electric potentials. Our American rivals found that gold-plating the electrodes helped greatly to minimise these problems. We found that coating them with a carbon-based solvent called 'aquadag' was a big help, not quite as effective as gold-plating but more in keeping with our research budget. My patience (and my research scholarship) ran out well before this experiment was made to yield significant results. I understand that a few more research students came to grief before significant results were eventually obtained. Now, decades later, low-energy electron spectroscopy is a pretty standard technique.

The details of my efforts, and those of my successors who were more successful, are not important. What I have said should be sufficient to illustrate what should be an uncontentious point. If experimental results constitute the facts on which science is based, then they are certainly not straightforwardly given via the senses. They have to be worked for, and their establishment involves considerable know-how and practical trial and error as well as exploitation of the available technology.

Nor are judgments about the adequacy of experimental results straightforward. Experiments are adequate, and interpretable as displaying or measuring what they are intended to display or measure, only if the experimental set-up is appropriate and disturbing factors have been eliminated. This in turn will require that it is known what those disturbing factors are and how they can be eliminated. Any inadequacies in the relevant knowledge about these factors could lead to inappropriate experimental measures and faulty conclusions. So there is a significant sense in which experimental facts and theory are interrelated. Experimental results can be faulty if the knowledge informing them is deficient or faulty.

A consequence of these general, and in a sense quite mundane, features of experiment is that experimental results are fallible, and can be updated or replaced for reasonably straightforward reasons. Experimental results can become outmoded because of advances in technology, they can be rejected because of some advance in understanding (in the light of which an experimental set-up comes to be seen as inadequate) and they can be ignored as irrelevant in the light of some shift in theoretical understanding. These points and their significance are illustrated by historical examples in the next section.

Transforming the experimental base of science: historical examples

Discharge tube phenomena commanded great scientific interest in the final quarter of the nineteenth century. If a high voltage is connected across metal plates inserted at each end of an enclosed glass tube, an electric discharge occurs, causing various kinds of glowing within the tube. If the gas pressure within the tube is not too great, streamers are produced, joining the negative plate (the cathode) and the positive plate (the anode). These became known as cathode rays, and their nature was a matter of considerable interest to scientists of the time. The German physicist Heinrich Hertz conducted a series of experiments in the early 1880s intended to shed light on their nature. As a result of these experiments Hertz concluded that cathode rays are not beams of charged particles. He reached this conclusion in part because the rays did not seem to be deflected when they were subjected to an electric field perpendicular to their direction of motion as would be expected of a beam of charged particles. We now regard Hertz's conclusion as false and his experiments inadequate. Before the century had ended, J. J. Thomson had conducted experiments that showed convincingly that cathode rays are deflected by electric and magnetic fields in a way that is consistent with their being beams of charged particles and was able to measure the ratio of the electric charge to the mass of the particles.

It was improved technology and improved understanding of the situation that made it possible for Thomson to improve on and reject Hertz's experimental results. The electrons that constitute the cathode rays can ionise the molecules of the gas in the tube, that is, displace an electron or two from them so that they become positively charged. These ions can collect on metal plates in the apparatus and lead to what, from the point of view of the experiments under consideration, are spurious electric fields. It was presumably such fields that prevented Hertz producing the deflections that Thomson was eventually to be able to produce and measure. The main way that Thomson was able to improve on Hertz's efforts was to take advantage of improved vacuum technology to remove more gas molecules from the tube. He subjected his apparatus to prolonged baking to drive residual gas from the various surfaces within the tube. He ran the vacuum pump for several days to remove as much of the residual gas as possible. With an improved vacuum, and with a more appropriate arrangement of electrodes, Thomson was able to establish the deflections that Hertz had declared to be non-existent. When Thomson allowed the pressure in his apparatus to rise to what it had been in Hertz's, Thomson could not detect a deflection either. It is important to realise here that Hertz is not to be blamed for drawing the conclusion that he did. Given his understanding of the situation, and drawing on the knowledge available to him, he had good reasons to believe that the pressure in his apparatus was sufficiently low and that his apparatus was appropriately arranged. It was only in the light of subsequent theoretical and technological advances that his experiment came to be seen as deficient. The moral, of course, is this: who knows which contemporary experimental results will be shown to be deficient by advances that lie ahead?

Far from being a shoddy experimentalist, the fact that Hertz was one of the very best is borne out by his success in being the first to produce radio waves in 1888, as the culmination of two years of brilliant experimental research. Apart from revealing a new phenomenon to be explored and developed experimentally, Hertz's waves had considerable theoretical significance, since they confirmed Maxwell's electromagnetic theory, which he had formulated in the mid-1860s and which had the consequence that there be such waves (although Maxwell himself had not realised this). Most aspects of Hertz's results remain acceptable and retain their significance today. However, some of his results needed to be replaced and one of his main interpretations of them rejected. Both of these points illustrate the way in which experimental results are subject to revision and improvement.

Hertz was able to use his apparatus to generate standing waves, which enabled him to measure their wavelength, from which he could deduce their velocity. His results indicated that the waves of longer wavelength travelled at a greater speed in air than along wires, and faster than light, whereas Maxwell's theory predicted that they would travel at the speed of light both in air and along the wires of Hertz's apparatus. The results were inadequate for reasons that Hertz already suspected. Waves reflected back onto the apparatus from the walls of the laboratory were causing unwanted interference. Hertz (1962, p. 14) himself reflected on the results as follows:

> The reader may perhaps ask why I have not endeavored to settle the doubtful point myself by repeating the experiments. I have indeed repeated the experiments, but have only found, as might be expected, that a simple repetition under the same conditions cannot remove the doubt, but rather increases it. A definite decision can only be arrived at by experiments carried out under more favorable conditions. More favorable conditions here mean larger rooms, and such were not at my disposal. I again emphasise the statement that care in making the observations cannot make up for want of space. If the long waves cannot develop, they clearly cannot be observed.

Hertz's experimental results were inadequate because his experimental set-up was inappropriate for the task in hand. The wavelengths of the waves investigated needed to be small compared with the dimensions of the laboratory if unwanted interference from reflected waves was to be removed. As it transpired, within a few years experiments were carried out 'under more favorable conditions' and yielded velocities in line with the theoretical predictions.

A point to be stressed here is that experimental results are required not only to be adequate, in the sense of being accurate recordings of what happened, but also to be appropriate or significant. They will typically be designed to cast light on some significant question. Judgments about what is a significant question and about whether some specific set of experiments is an adequate way of answering it will depend heavily on how the practical and theoretical situation is understood. It was the existence of competing theories of electromagnetism and the fact that one of the major contenders predicted radio waves travelling with the speed of light that made Hertz's attempt to measure the velocity of his waves particularly significant, while it was an understanding of the reflection behaviour of the waves that led to the appreciation that Hertz's experimental set-up was inappropriate. These particular results of Hertz's were rejected and soon replaced for reasons that are straightforward and non-mysterious from the point of view of physics.

As well as illustrating the point that experiments need to be appropriate or significant, and that experimental results are replaced or rejected when they cease to be so, this episode in Hertz's researches and his own reflections on it clearly bring out the respect in which the rejection of his velocity measurements has nothing whatsoever to do with problems of human perception. There is no reason whatsoever to doubt that Hertz carefully observed his apparatus, measuring distances, noting the presence or absence of sparks across the gaps in his detectors, and recording instrument readings. His results can be assumed to be objective in the sense that anyone who repeats them will get similar results. Hertz himself stressed this point. The problem with Hertz's experimental results stems neither from inadequacies in his observations nor from any lack of repeatability, but rather from the inadequacy of the experimental set-up. As Hertz pointed out, 'care in making the observations cannot make up for want of space'. Even if we concede that Hertz was able to establish secure facts by way of careful observation, we can see that this in itself was insufficient to yield experimental results adequate for the scientific task in question.

The above discussion can be construed as illustrating how the acceptability of experimental results is theory-dependent, and how judgments in this respect are subject to change as our scientific understanding develops. This is illustrated at a more general level by the way in which the significance of Hertz's production of radio waves has changed since Hertz first produced them. At that time, one of the several competing theories of electromagnetism was that of James Clerk Maxwell, who had developed the key ideas of

Michael Faraday and had understood electric and magnetic states as the mechanical states of an all-pervasive ether. This theory, unlike its competitors, which assumed that electric currents, charges and magnets acted on each other at a distance and did not involve an ether, predicted the possibility of radio waves moving at the speed of light. This is the aspect of the state of development of physics that gave Hertz's results their theoretical significance. Consequently, Hertz and his contemporaries were able to construe the production of radio waves as, among other things, *confirmation of the existence of an ether.* Two decades later the ether was dispensed with in the light of Einstein's special theory of relativity. Hertz's results are still regarded as confirming Maxwell's theory, but only a rewritten version of it that dispenses with the ether, and treats electric and magnetic fields as real entities in their own right.

Another example, concerning nineteenth-century measurements of molecular weights, further illustrates the way in which the relevance and interpretation of experimental results depend on the theoretical context. Measurements of the molecular weights of naturally occurring elements and compounds were considered to be of fundamental importance by chemists in the second half of the nineteenth century in the light of the atomic theory of chemical combination. This was especially so for those who favoured Prout's hypothesis that the hydrogen atom is the basic building block from which other atoms are constructed, for this led one to expect that molecular weights measured relative to hydrogen would be whole numbers. The painstaking measurements of molecular weights by the leading experimental chemists in the nineteenth century became largely irrelevant from the point of view of theoretical chemistry once it was realised that naturally occurring elements contain a mixture of isotopes in proportions that had no particular theoretical significance. This situation inspired the chemist F. Soddy to comment on its outcome as follows (Lakatos and Musgrave, 1970, p. 140):

> There is something surely akin to if not transcending tragedy in the fate that has overtaken the life work of this distinguished galaxy of nineteenth-century chemists, rightly revered by their contemporaries as representing the crown and perfection of accurate scientific measurements. Their hard won results, for the

> moment at least, appear as of little significance as the determination of the average weight of a collection of bottles, some of them full and some of them more or less empty.

Here we witness old experimental results being set aside as irrelevant, and for reasons that do not stem from problematic features of human perception. The nineteenth-century chemists involved were 'revered by their contemporaries as representing the crown and perfection of accurate scientific measurement' and we have no reason to doubt their observations. Nor need we doubt the objectivity of the latter. I have no doubt that similar results would be obtained by contemporary chemists if they were to repeat the same experiments. That they be adequately performed is a necessary but not sufficient condition for the acceptability of experimental results. They need also to be relevant and significant.

The points I have been making with the aid of examples can be summed up in a way that I believe is quite uncontentious from the point of view of physics and chemistry and their practice. The stock of experimental results regarded as an appropriate basis for science is constantly updated. Old experimental results are rejected as inadequate and replaced by more adequate ones, for a range of fairly straightforward reasons. They can be rejected because the experiment involved inadequate precautions against possible sources of interference, because the measurements employed insensitive and outmoded methods of detection, because the experiments came to be understood as incapable of solving the problem in hand, or because the questions they were designed to answer became discredited. Although these observations can be seen as fairly obvious comments on everyday scientific activity, they nevertheless have serious implications for much orthodox philosophy of science, for they undermine the widely held notion that science rests on secure foundations. What is more, the reasons why it does not has nothing much to do with problematic features of human perception.

Experiment as an adequate basis for science

In the previous sections of this chapter I have subjected to critical scrutiny the idea that experimental results are straightforwardly given and totally secure. I have made a case to the effect that they are theory-dependent in certain respects and fallible and revisable. This can be interpreted as a serious threat to the idea that scientific knowledge is special because it is supported by experience in some especially demanding and convincing way. If, it might be argued, the experimental basis of science is as fallible and revisable as I have argued it to be, then the knowledge based on it must be equally fallible and revisable. The worry can be strengthened by pointing to a threat of circularity in the way scientific theories are alleged to be borne out by experiment. If theories are appealed to in order to judge the adequacy of experimental results, and those same experimental results are taken as the evidence for the theories, then it would seem that we are caught in a circle. It would seem that there is a strong possibility that science will not provide the resources to settle a dispute between the proponents of opposing theories by appeal to experimental results. One group would appeal to its theory to vindicate certain experimental results, and the opposing camp would appeal to its rival theory to vindicate different experimental results. In this section I give reasons for resisting these extreme conclusions.

It must be acknowledged that there is the possibility that the relationship between theory and experiment might involve a circular argument. This can be illustrated by the following story from my schoolteaching days. My pupils were required to conduct an experiment along the following lines. The aim was to measure the deflection of a current-carrying coil suspended between the poles of a horseshoe magnet and free to rotate about an axis perpendicular to the line joining the poles of the magnet. The coil formed part of a circuit containing a battery to supply a current, an ammeter to measure the current and a variable resistance to make it possible to adjust the strength of the current. The aim was to note the deflection of the magnet corresponding to various values of the current in the circuit as registered by the ammeter. The experiment was to be deemed a success for those pupils who got a nice straight-line graph when they plotted deflection against current, revealing the proportionality of the two. I remember being disconcerted by this experiment, although, perhaps wisely, I did not transmit my worry to my pupils. My worry stemmed from the fact that I knew what was inside the ammeter. What was inside was a coil suspended between the poles of a magnet in such a way that it was deflected by a current through the coil causing a needle to move on the visible and evenly calibrated scale of the ammeter. In this experiment, then, the proportionality of deflection to current was already presupposed when the reading of the ammeter was taken as a measure of the current. What was taken to be supported by the experiment was already presupposed in it, and there was indeed a circularity.

My example illustrates how circularity can arise in arguments that appeal to experiment. But the very same example serves to show that this need not be the case. The above experiment could have, and indeed should have, used a method of measuring the current in the circuit that did not employ the deflection of a coil in a magnetic field. All experiments will presume the truth of some theories to help judge that the set-up is adequate and the instruments are reading what they are meant to read. But these presupposed theories need not be identical to the theory under test, and it would seem reasonable to assume that a prerequisite of good experimental design is to ensure that they are not.

Another point that serves to get the 'theory-dependence of experiment' in perspective is that, however informed by theory an experiment is, there is a strong sense in which the results of an experiment are determined by the world and not by the theories. Once the apparatus is set up, the circuits completed, the switches thrown and so on, there will or will not be a flash on the screen, the beam may or may not be deflected, the reading on the ammeter may or may not increase. We cannot make the outcomes conform to our theories. It was because the physical world is the way it is that the experiment conducted by Hertz yielded no deflection of cathode rays and the modified experiment conducted by Thomson did. It was the material differences in the experimental arrangements of the two physicists that led to the differing outcomes, not the differences in the theories held by them. It is the sense in which experimental outcomes are determined by the workings of the world rather than by theoretical views about the world that provides the possibility of testing theories against the world. This is not to say that significant results are easily achievable and infallible, nor that their significance is always straightforward. But it does help to establish the point that the attempt to test the adequacy of scientific theories against experimental results is a meaningful quest. What is more, the history of science gives us examples of cases where the challenge was successfully met.

Further reading

The second half of Hacking (1983) was an important early move in the new interest philosophers of science have taken in experiment. Other explorations of the topic are Franklin (1986), Franklin (1990), Galison (1987) and Mayo (1996), although these detailed treatments will take on their full significance only in the light of chapter 13, on the 'new experimentalism'.

CHAPTER 4

Deriving theories from the facts: induction

Introduction

In these early chapters of the book we have been considering the idea that what is characteristic of scientific knowledge is that it is derived from the facts. We have reached a stage where we have given some detailed attention to the nature of the observational and experimental facts that can be considered as the basis from which scientific knowledge might be derived, although we have seen that those facts cannot be established as straightforwardly and securely as is commonly supposed. Let us assume, then, that appropriate facts can be established in science. We must now face the question of how scientific knowledge can be derived from those facts.

'Science is derived from the facts' could be interpreted to mean that scientific knowledge is constructed by first establishing the facts and then subsequently building the theory to fit them. We discussed this view in chapter 1 and rejected it as unreasonable. The issue that I wish to explore involves interpreting 'derive' in some kind of logical rather than temporal sense. No matter which comes first, the facts or the theory, the question to be addressed is the extent to which the theory is borne out by the facts. The strongest possible claim would be that the theory can be logically derived from the facts. That is, given the facts, the theory can be proven as a consequence of them. This strong claim cannot be substantiated. To see why this is so we must look at some of the basic features of logical reasoning.

Baby logic

Logic is concerned with the deduction of statements from other, given, statements. It is concerned with what follows from what. No attempt will be made to give a detailed account and appraisal of logic or deductive reasoning here. Rather, I will make the points that will be sufficient for our purpose with the aid of some very simple examples.

Here is an example of a logical argument that is perfectly adequate or, to use the technical term used by logicians, perfectly valid.

Example 1

1. All books on philosophy are boring.
2. This book is a book on philosophy.

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3. This book is boring.

In this argument, (1) and (2) are the premises and (3) is the conclusion. It is evident, I take it, that if (1) and (2) are true then (3) is bound to be true. It is not possible for (3) to be false once it is given that (1) and (2) are true. To assert (1) and (2) as true and to deny (3) is to contradict oneself. This is the key feature of a *logically valid* deduction. If the premises are true then the conclusion must be true. Logic is truth preserving.

A slight modification of Example 1 will give us an instance of an argument that is not valid.

Example 2

1. Many books on philosophy are boring.
2. This book is a book on philosophy.

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3. This book is boring.

In this example, (3) does not follow of necessity from (1) and (2). Even if (1) and (2) are true, then this book might yet turn out to be one of the minority of books on philosophy that are not boring. Accepting (1) and (2) as true and holding (3) to be false does not involve a contradiction. The argument is invalid.

The reader may by now be feeling bored. Experiences of that kind certainly have a bearing on the truth of statements (1) and (3) in Example 1 and Example 2. But a point that needs to be stressed here is that logical deduction alone cannot establish the truth of factual statements of the kind figuring in our examples. All that logic can offer in this connection is that *if* the premises are true and the argument is valid *then* the conclusion must be true. But whether the premises are true or not is not a question that can be settled by an appeal to logic. An argument can be a perfectly valid deduction even if it involves a false premise. Here is an example.

Example 3
1. All cats have five legs.
2. Bugs Pussy is my cat.

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3. Bugs Pussy has five legs.

This is a perfectly valid deduction. If (1) and (2) are true then (3) must be true. It so happens that, in this example (1) and (3) are false. But this does not affect the fact that the argument is valid.

There is a strong sense, then, in which logic alone is not a source of new truths. The truth of the factual statements that constitute the premises of arguments cannot be established by appeal to logic. Logic can simply reveal what follows from, or what in a sense is already contained in, the statements we already have to hand. Against this limitation we have the great strength of logic, namely, its truth-preserving character. If we can be sure our premises are true then we can be equally sure that everything we logically derive from them will also be true.

Can scientific laws be derived from the facts?

With this discussion of the nature of logic behind us, it can be straightforwardly shown that scientific knowledge cannot be derived from the facts if 'derive' is interpreted as 'logically deduce'.

Some simple examples of scientific knowledge will be sufficient for the illustration of this basic point. Let us consider some low-level scientific laws such as 'metals expand when heated' or 'acids turn litmus red'. These are general statements. They are examples of what philosophers refer to as universal statements. They refer to all events of a particular kind, all instances of metals being heated and all instances of litmus being immersed in acid. Scientific knowledge invariably involves general statements of this kind. The situation is quite otherwise when it comes to the observation statements that constitute the facts that provide the evidence for general scientific laws. Those observable facts or experimental results are specific claims about a state of affairs that obtains at a particular time. They are what philosophers call singular statements. They include statements such as 'the length of the copper bar increased when it was heated' or 'the litmus paper turned red when immersed in the beaker of hydrochloric acid'. Suppose we have a large number of such facts at our disposal as the basis from which we hope to derive some scientific knowledge (about metals or acids in the case of our examples). What kind of argument can take us from those facts, as premises, to the scientific laws we seek to derive as conclusions? In the case of our example concerning the expansion of metals the argument can be schematised as follows:

Premises
1. Metal x_1 expanded when heated on occasion t_1.
2. Metal x_2 expanded when heated on occasion t_2.
n. Metal x_n expanded when heated on occasion t_n.
Conclusion
All metals expand when heated.

This is not a logically valid argument. It lacks the basic features of such an argument. It is simply not the case that if the statements constituting the premises are true then the conclusion must be true. However many observations of expanding metals we have to work with, that is, however great n might be in our example, there can be no *logical* guarantee that some sample of metal might on some occasion contract when heated. There is no contradiction involved in claiming both that all known examples of the heating of metals has resulted in expansion and that 'all metals expand when heated' is false.

This straightforward point is illustrated by a somewhat gruesome example attributed to Bertrand Russell. It concerns a turkey who noted on his first morning at the turkey farm that he was fed at 9 am. After this experience had been repeated daily for several weeks the turkey felt safe in drawing the conclusuion 'I am always fed at 9 am'. Alas, this conclusion was shown to be false in no uncertain manner when, on Christmas eve, instead of being fed, the turkey's throat was cut. The turkey's argument led it from a number of true observations to a false conclusion, clearly indicating the invalidity of the argument from a logical point of view.

Arguments of the kind I have illustrated with the example concerning the expansion of metals, which proceed from a finite number of specific facts to a general conclusion, are called *inductive* arguments, as distinct from logical, *deductive* arguments. A characteristic of inductive arguments that distinguishes them from deductive ones is that, by proceeding as they do from statements about *some* to statements about *all* events of a particular kind, they go beyond what is contained in the premises. General scientific laws invariably go beyond the finite amount of observable evidence that is available to support them, and that is why they can never be proven in the sense of being logically deduced from that evidence.

What constitutes a good inductive argument?

We have seen that if scientific knowledge is to be understood as being derived from the facts, then 'derive' must be understood in an inductive rather than a deductive sense. But what are the characteristics of a good inductive argument? The question is of fundamental importance because it is clear that not all generalisations from the observable facts are warranted. Some of them we will wish to regard as overhasty or based on insufficient evidence, as when, perhaps, we condemn the attribution of some characteristic to an entire ethnic group based on some unpleasant encounters with just one pair of neighbours. Under precisely what circumstances is it legitimate to assert that a scientific law has been 'derived' from some finite body of observational and experimental evidence?

A first attempt at an answer to this question involves the demand that, if an inductive inference from observable facts to laws is to be justified, then the following conditions must be satisfied:

1. The number of observations forming the basis of a generalisation must be large.
2. The observations must be repeated under a wide variety of conditions.

3. No accepted observation statement should conflict with the derived law.

Condition 1 is regarded as necessary because it is clearly not legitimate to conclude that all metals expand when heated on the basis of just one observation of an iron bar's expansion, say, any more than it is legitimate to conclude that all Australians are drunkards on the basis of one observation of an intoxicated Australian. A large number of independent observations would appear to be necessary before either generalisation can be justified. A good inductive argument does not jump to conclusions.

One way of increasing the number of observations in the examples mentioned would be to repeatedly heat a single bar of metal or to continually observe a particular Australian getting drunk night after night, and perhaps morning after morning. Clearly, a list of observation statements acquired in such a way would form a very unsatisfactory basis for the respective generalisations. That is why Condition 2 is necessary. 'All metals expand when heated' will be a legitimate generalisation only if the observations of expansion on which it is based range over a wide variety of conditions. Various kinds of metals should be heated, long bars, short bars, silver bars, copper bats, etc. should be heated at high and low pressures and high and low temperatures and so on. Only if on all such occasions expansion results is it legitimate to generalise by induction to the general law. Further, it is evident that if a particular sample of metal is observed not to expand when heated, then the generalisation to the law will not be justified. Condition 3 is essential.

The above can be summed up by the following statement of *the principle of induction*.

> If a large number of **A**s have been observed under a wide variety of conditions, and if all those **A**s without exception possess the property **B**, then all **A**s have the property **B**.

There are serious problems with this characterisation of induction. Let us consider Condition 1, the demand for large numbers of observations. One problem with it is the vagueness of 'large'. Are a hundred, a thousand or more observations required? If we do attempt to introduce precision by introducing a number here, then there would surely be a great deal of arbitrariness in the number chosen. The problems do not stop here. There are many instances in which the demand for a large number of instances seems inappropriate. To illustrate this, consider the strong public reaction against nuclear warfare that was provoked by the dropping of the first atomic bomb on Hiroshima towards the end of the Second World War. That reaction was based on the realisation of the extent to which atomic bombs cause widespread destruction and human suffering. And yet this widespread, and surely reasonable, belief was based on just one dramatic observation. In a similar vein, it would be a very stubborn investigator who insisted on putting his hand in the fire many times before concluding that fire burns. Let us consider a less fanciful example related to scientific practice. Suppose I reproduced an experiment reported in some recent scientific journal, and sent my results off for publication. Surely the editor of the journal would reject my paper, explaining that the experiment had already been done! Condition 1 is riddled with problems.

Condition 2 has serious problems too, stemming from difficulties surrounding the question of what counts as a significant variation in circumstances. What counts as a significant variation in the circumstances under which the expansion of a heated metal is to be investigated? Is it necessary to vary the type of metal, the pressure and the time of day? The answer is 'yes' in the first and possibly the second case but 'no' in the third. But what are the grounds for that answer? The question is important because unless it can be answered the list of variations can be extended indefinitely by endlessly adding further variations, such as the size of the laboratory and the colour of the experimenter's socks. Unless such 'superfluous' variations can be eliminated, the conditions under which an inductive inference can be accepted can never be satisfied. What are the grounds, then, for regarding a range of possible variations as superfluous? The commonsense answer is straightforward enough. We draw on our prior knowledge of the situation to distinguish between the factors that might and those that cannot influence the system we are investigating. It is our knowledge of metals and the kinds of ways that they can be acted on that leads us to the expectation that their physical behaviour will depend on the type of metal and the surrounding pressure but not on the time of day or the colour of the experimenter's socks. We draw on our current stock of knowledge to help judge what is a relevant circumstance that might need to be varied when investigating the generality of an effect under investigation.

This response to the problem is surely correct. However, it poses a problem for a sufficiently strong version of the claim that scientific knowledge should be derived from the facts by induction. The problem arises when we pose the question of how the knowledge appealed to when judging the relevance or otherwise of some circumstances to a phenomenon under investigation (such as the expansion of metals) is itself vindicated. If we demand that that knowledge itself is to be arrived at by induction, then our problem will recur, because those further inductive arguments will themselves require the specification of the relevant circumstances and so on. Each inductive argument invovles an appeal to prior knowledge, which needs an inductive argument to justify it, which involves an appeal to further prior knowledge and so on in a never-ending chain. The demand that all knowledge be justified by induction becomes a demand that cannot be met.

Even Condition 3 is problematic since little scientific knowledge would survive the demand that there be no known exceptions. This is a point that will be discussed in some detail in chapter 7.

Further problems with inductivism

Let us call the position according to which scientific knowledge is to be derived from the observable facts by some kind of inductive inference *inductivism* and those who subscribe to that view *inductivists.* We have already pointed to a serious problem inherent in that view, namely, the problem of stating precisely under what conditions a generalisation constitutes a good inductive inference. That is, it is not clear what induction amounts to. There are further problems with the inductivist position.

If we take contemporary scientific knowledge at anything like face value, then it has to be admitted that much of that knowledge refers to the unobservable. It refers to such things as protons and electrons, genes and DNA molecules and so on. How can such knowledge be accommodated into the inductivist position? Insofar as inductive reasoning involves some kind of generalisation from observable facts, it would appear that such reasoning is not capable of yielding knowledge of the unobservable. Any generalisation from facts about the observable world can yield nothing other than generations about the observable world. Consequently, scientific knowledge of the unobservable world can never be established by the kind of inductive reasoning we have discussed. This leaves the inductivist in the uncomfortable position of having to reject much contemporary science on the grounds that it involves going beyond what can be justified by inductive generalisation from the observable.

Another problem stems from the fact that many scientific laws take the form of exact, mathematically formulated laws. The law of gravitation, which states that the force between any two masses is proportional to the product of those masses divided by the square of the distance that separates them, is a straightforward example. Compared with the exactness of such laws we have the inexactness of any of the measurements that constitute the observable evidence for them. It is well appreciated that all observations are subject to some degree of error, as reflected in the practice of scientists when they write the result of a particular measurement as $x \pm dx$, where dx represents the estimated margin of error. If scientific laws are inductive generalisations from observable facts it is difficult to see how one can escape the inexactness of the measurements that constitute the premises of the inductive arguments. It is difficult to see how exact laws can ever be inductively justified on the basis of inexact evidence.

A third problem for the inductivist is an old philosophical chestnut called the problem of induction. The problem arises for anyone who subscribes to the view that scientific knowledge in all its aspects must be justified either by an appeal to (deductive) logic or by deriving it from experience. David Hume was an eighteenth-century philosopher who did subscribe to that view, and it was he who clearly articulated the problem I am about to highlight.

The problem arises when we raise the question of how induction itself is to be justified. How is the principle of induction to be vindicated? Those who take the view under discussion have only two options, to justify it by an appeal to logic or by an appeal to experience. We have already seen that the first option will not do. Inductive inferences are not logical (deductive) inferences. This leaves us with the second option, to attempt to justify induction by an appeal to experience. What would such a justification be like? Presumably, it would go something like this. Induction has been observed to work on a large number of occasions. For instance, the laws of optics, derived by induction from the results of laboratory experiments, have been used on numerous occasions in the design of optical instruments that have operated satisfactorily, and the laws of planetary motion, inductively derived from the observation of planetary positions, have been successfully used to predict eclipses and conjunctions. This list could be greatly extended with accounts of successful predictions and explanations that we presume to be made on the basis of inductively derived scientific laws and theories. Thus, so the argument goes, induction is justified by experience.

This justification of induction is unacceptable. This can be seen once the form of the argument is spelt out schematically as follows:

> The principle of induction worked successfully on occasion x_1
> The principle of induction worked successfully on occasion x_2 etc.
> ___
> The principle of induction always works.

A general statement asserting the validity of the principle of induction is here inferred from a number of individual instances of its successful application. The argument is therefore itself an inductive one. Consequently, the attempt to justify induction by an appeal to experience involves assuming what one is trying to prove. It involves justifying induction by appealing to induction, and so is totally unsatisfactory.

One attempt to avoid the problem of induction involves weakening the demand that scientific knowledge be proven true, and resting content with the claim that scientific claims can be shown to be probably true in the light of the evidence. So the vast number of observations that can be invoked to support the claim that