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Induction and inductivism

1.1 The sceptic's challenge

Our starting point is the desire to arbitrate the following dispute that arises when Alice, who has been reading *A Brief History of Time* by Stephen Hawking, is trying to explain the exciting things she has learned about the Big Bang and the history of the universe to her friend Thomas.

Alice: . . . and so one second after the Big Bang the temperature of the universe was about ten thousand million degrees, which is about the same as the temperature in the middle of the explosion of a nuclear bomb.

Thomas: Do you really buy all that stuff? Don't you think it's a bit far-fetched?

Alice: Of course I believe it, and I don't think it is any more far-fetched than the fact that this table we are sitting at is almost all empty space and that it is made of atoms so tiny that millions of them could fit on the end of a pin.

Thomas: Exactly, it is just as far-fetched and you are just gullible for believing it.

Alice: But that is what science tells us.

Thomas: 'Science' doesn't tell us anything; scientists, people like you or me, tell us things and like all people they tell us what is in their interest to tell us.

Alice: What do you mean?

Thomas: Isn't it obvious? A used-car dealer will tell you that a car is a lovely little runner with one previous owner because they want you to buy the car, priests tell you that you must come to church so you can go to heaven, because otherwise they would be out of a job, and scientists tell us all that nonsense so we will be amazed at how clever they are and keep spending taxpayers' money on their research grants.

Alice: Now you are just being cynical; not everyone is out for themselves you know.

Thomas: And you are just being naïve; anyway, even supposing that scientists really believe their theories, can't you see that science is just the modern religion?

Alice: What do you mean?

Thomas: Well, if you were living five hundred years ago you would believe in angels and saints and the Garden of Eden; science has just replaced religion as the dominant belief system of the West. If you were living in a tribe in the jungle somewhere you would believe in whatever creation myths the elders of the tribe passed down to you, but you happen to be living here and now, so you believe what the experts in our tribe, who happen to be the scientists, tell us.

Alice: You can't compare religious dogma and myth with science.

Thomas: Why not?

Alice: Because scientists develop and test their beliefs according to proper methods rather than just accepting what they are told.

Thomas: Well you are right that they *claim* to have a method that ensures their theories are accurate but I don't believe it myself, otherwise they would all come to the same conclusions and we know that scientists are always arguing with each other, like about whether salt or sugar is really bad for you.

Alice: Well it takes time for theories to be proven but they will find out eventually.

Thomas: Your faith is astounding – and you claim that science and religion are totally different. The scientific method is a myth put about by scientists who want us to believe their claims. Look at all the drugs that have been tested by scientific methods and pronounced safe only to be withdrawn a few years later when people find out how dangerous they are.

Alice: Yes but what about all the successful drugs and the other amazing things science has done.

Thomas: Trial and error, that's the only scientific method there is, it's as simple as that. The rest is just propaganda.

Alice: I can't believe that; scientific theories, like the Big Bang theory, are proved by experiments and observations, that is why we ought to believe them and that is what makes them different from creation myths and religious beliefs.

Thomas: So you say but how can experiments and observations prove a theory to be true?

Alice: I suppose I don't really know.

Thomas: Well let me know when you've found out.

In this dialogue, one of the characters challenges the other to explain why her beliefs, which are based on what she has been told by scientists, are any better supported than belief in angels and devils or the spirits and witchcraft of animistic religions. Of course, there are lots of things that each of us believe that we cannot justify directly ourselves; for example, I believe that large doses of arsenic are toxic to humans, but I have never even seen any arsenic as far as I am aware, and I have certainly never tested its effects. We all believe all kinds of things to be the case because we rely upon what others tell us directly or indirectly; whether or not we are justified depends upon whether or not they are justified. Most readers of this book probably believe that the Earth revolves around the Sun, that we as human beings evolved from animals that were more like apes, that water is made of twice as much hydrogen as oxygen, that diseases are often caused by viruses and other tiny organisms, and so on. If we believe these things it is because the experts in our tribe (the scientists) tell us them; in that way, the causes of our beliefs are of much the same kind as those of someone who believes what the local witch-doctor tells them about, say, the cause of disease being the witchcraft of another person. We like to think that there is a difference between our beliefs and belief in witchcraft nonetheless; if there isn't then why do we spend so much money on modern drugs and treatments when a few sacrifices or spells would do just as well?

Our believer (Alice) thinks that the scientific method is what makes the difference, in that our beliefs are ultimately produced and proven by it, and that it has something to do with experiments and observation. In this chapter we will investigate the nature of the scientific method, if indeed there is one, beginning with the origins of modern science in the search for a new method of inquiry to replace reliance on the authority of the Church and the pronouncements of the ancients. Our goal will be to determine whether Alice, who believes in what science tells her, is entitled to her faith or whether the attitude of the sceptic, Thomas, is in fact the more reasonable one.

1.2 The scientific revolution

The crucial developments in the emergence of modern science in the western world took place during the late sixteenth and the seventeenth centuries. Within a relatively short space of time, not only was much of what had previously been taken for granted discredited and abandoned, but also a host of new theoretical developments in astronomy, physics, physiology and other sciences were established. The study of the motion of matter in collisions and under the influence of gravity (which is known as mechanics) was completely revolutionised and, beginning with the work of Galileo Galilei (1564–1642) in the early sixteen hundreds and culminating in the publication of Isaac Newton's (1642–1727) mathematical physics in 1687, this part of physics became a shining example of scientific achievement because of its spectacular success in making accurate and precise predictions of the behaviour of physical systems. There were equally great advances in other areas and powerful new technologies, such as the telescope and microscope, were developed.

This period in intellectual history is often called *the Scientific revolution* and embraces *the Copernican revolution*, which is the name

given to the period during which the theory of the solar system and the wider cosmos, which had the Earth at the centre of everything (geocentrism), was replaced by the theory that the Earth revolved around the Sun (heliocentrism). From the philosophical point of view the most important development during the scientific revolution was the increasingly widespread break with the theories of Aristotle (384–322 BC). As new ideas were proposed, some thinkers began to search for a new method that could be guaranteed to bring knowledge. In the Introduction we found that for a belief to count as knowledge it must be justified, so if we want to have knowledge we might aim to follow a procedure when forming our beliefs that simultaneously provides us with a justification for them; the debate about what such a procedure might consist of, which happened during the scientific revolution, was the beginning of the modern debate about scientific method.

In medieval times, Aristotle's philosophy had been combined with the doctrines of Christianity to form a cosmology and philosophy of nature (often called *scholasticism*) that described everything from the motions of the planets to the behaviour of falling bodies on the Earth, the essentials of which were largely unquestioned by most western intellectuals. According to the Aristotelian view, the Earth and the heavens were completely different in their nature. The Earth and all things on and above it, up as far as the Moon, were held to be subject to change and decay and were imperfect; everything here was composed of a combination of the elements of earth, air, fire and water, and all natural motion on the Earth was fundamentally in a straight line, either straight up for fire and air, or straight down for water and earth. The heavens, on the other hand, were thought to be perfect and changeless; all the objects that filled them were supposed to be made up of a quite different substance, the fifth essence (or quintessence), and all motion was circular and continued forever.

Although not everyone in Europe prior to the scientific revolution was an Aristotelian, this was the dominant philosophical outlook, especially because of its incorporation within official Catholic doctrine. The break with Aristotelian philosophy began slowly and with great controversy, but by the end of the seventeenth century the radically non-Aristotelian theories of Galileo, Newton and others were widely accepted. Perhaps the most significant event in this process was the publication in 1543 of a theory of the motions of the planets by the astronomer Nicolaus Copernicus (1473–1543). In the Aristotelian picture, the Earth was at the centre of the universe and all the heavenly bodies, the Moon, the planets, the Sun and the stars revolved around the Earth following circular orbits. An astronomer and mathematician called Ptolemy of Alexandria (circa AD 150) systematically described these orbits mathematically. However, the planets' motions in the sky are difficult to reproduce in this way because sometimes they appear to go backwards for a while (this is called retrograde motion). Ptolemy found that to get the theory to agree at all well with observations, the motions of the planets had to be along circles that themselves revolved around the Earth, and this made the theory very complex and difficult to use (see Figure 1).

Copernicus retained the circular motions but placed the Sun rather than the Earth at the centre of the system, and then had the Earth rotating both about its own axis and around the Sun, and this considerably simplified matters mathematically. Subsequently, Copernicus' theory was improved by the work of Johannes Kepler (1571–1630), who treated the planets as having not circular but elliptical orbits, and it was the latter's theory of the motions of the planets that Newton elaborated with his gravitational force and which is still used today for most practical purposes.

One thing to note about the Copernican system is that it may seem to be counter to our experience in the sense that we do not feel the

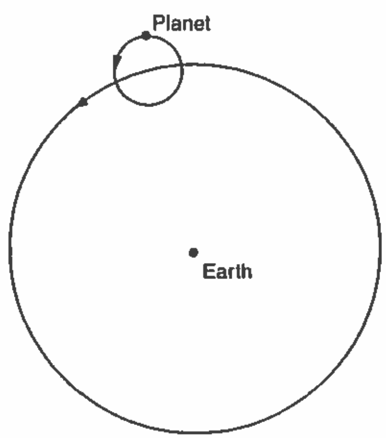

Figure 1

Earth to be moving when we stand still upon it, and moreover we observe the Sun to move over our heads during the day. This is an important example of how scientific theories seem to describe a *reality* distinct from the *appearance* of things. This distinction between appearance and reality is central to metaphysics because the latter seeks to describe things 'as they really are' rather than how they merely appear to be. When Copernicus' book was published, after his death, it included a preface by Andreas Osiander (1498–1552) (a friend of Copernicus who had helped prepare the book for publication) which declared that the motion of the Earth was a convenient assumption made by Copernicus but which need only be regarded as a mathematical fiction, rather than being taken literally as asserting that the Earth really was in orbit around the Sun. This is an early example of the philosophical thesis of **instrumentalism**, according to which scientific theories need not be believed to be true, but rather should be thought of as useful or convenient fictions. On the other hand, to be a realist about Copernicus' theory is to think that it should be taken literally and to believe that the Earth really does orbit the Sun. Realists, unlike instrumentalists, think that scientific theories can answer metaphysical questions. (We shall return to the realism versus instrumentalism debate later.)

The doctrine that the Earth is not at the centre of the universe and that it is, in fact, in motion around the Sun was in direct contradiction with Catholic doctrine and Osiander's preface did not prevent a controversy arising about Copernicus' theory. This controversy became quite fierce by the early years of the seventeenth century and, in 1616, Copernicus' book and all others that adopted the heliocentric hypothesis were placed on a list of books that Catholics were banned from teaching or even reading. It may be hard to appreciate why the Church was so worried about a theory in astronomy, but heliocentrism not only conflicted with the Aristotelian picture of the universe and rendered its explanations of motion inapplicable, it also conflicted with the traditional understanding of the Book of Genesis and the Fall of Adam and Eve, the relationship between the Earth and the Devil on the one hand and the Heavens and God on the other, and so on. The consequence of this was that if one were to adopt the Copernican theory, a great deal of what one took for granted was thrown into doubt – hence the need for a way of replacing the Aristotelian picture of the world with a set of beliefs that were equally comprehensive, but more up to date.

1.3 The 'new tool' of induction

The emergence of modern science required not just the contribution of those like Copernicus and Galileo who proposed new theories, but also the contribution of people who could describe and then advocate and propagate the new ways of thinking. In modern parlance, science needed to be marketed and sold to intellectuals who would otherwise have accepted the established Aristotelian thinking. Greatest among the propagandists of the emerging sciences was Francis Bacon (1561–1626), who explicitly proposed a method for the sciences to replace that of Aristotle. In his book *Novum Organum* of 1620 he set out this method in great detail and it still forms the core of what many people take the scientific method to be. Many of Bacon's contemporaries thought that the ancients had understood all there was to be known and that it was just a matter of recovering what had been lost. By contrast, Bacon was profoundly ambitious about what new things could be known and how such knowledge could be employed practically (he is often credited with originating the phrase 'knowledge is power').

Bacon's method is thoroughly egalitarian and collectivist in spirit: he believed that if it was followed by many ordinary people working together, rather than a few great minds, then as a social process it would lead to the production of useful and sure beliefs about the functioning of nature. When one bears in mind that nowadays a single paper in physics is routinely co-authored by tens of people, it is apparent that Bacon was prophetic, both in his vision of science as a systematic and collaborative effort involving the co-ordinated labour of many individuals to produce knowledge, and in his belief that the practical applications of science would enable people to control and manipulate natural phenomena to great effect. (On the other hand, one consequence of the growth of scientific knowledge has been that a great deal of training is now necessary before someone can become a researcher in, say, microbiology or theoretical physics.)

The translation of *Novum Organum* is New Tool, and Bacon

proposed his method as a replacement for the *Organum* of Aristotle, this being the contemporary name for the textbook that contained Aristotelian logic. Logic is the study of reasoning abstracted from what that reasoning is about. Hence, in logic the following two arguments are treated as if they were the same because their form or structure are equivalent despite the difference in their content:

(1) All human beings are mortal (PREMISE)
Socrates is a human being (PREMISE)
Therefore Socrates is mortal (CONCLUSION)

(2) All guard dogs are good philosophers
Fido is a guard dog
Therefore Fido is a good philosopher

The premises of the first argument are true and so is the conclusion, while the first premise of the second argument is probably false and so is the conclusion. What they have in common is that they exemplify the following structure:

All Xs are Y
A is X
Therefore A is Y

Such an argument is *valid*, which is to say if the premises are true then so must be the conclusion; in other words, if an argument is valid then it is *impossible* for the premises all to be true and the conclusion false.

An *invalid* argument is one in which the premises may all be true and the conclusion false, so for example, consider:

All Xs are Ys
A is Y
Therefore A is X

This argument is invalid as we can see if we have the following premises and conclusion:

All guard dogs are good philosophers
James is a good philosopher
Therefore James is a guard dog

Even if we suppose the first and second premises to be true, implausible as they may seem, it does not follow that James is a guard dog. (To reason in accordance with an invalid form of argument is to fall prey to a *logical fallacy*.) That this argument form is invalid is obvious when we consider the following argument that has the same structure but true premises and a false conclusion:

All human beings are animals
Bess is an animal
Therefore Bess is a human being

Here we have an instance of the same form of argument where it is obviously possible for the premises to be true and the conclusion false (actually Bess is a dog) and hence it must be invalid. (Make sure you understand why this argument has the same form as the one immediately preceding it, and why both are invalid. It is important that validity has nothing to do with whether the premises or conclusion are actually true or false; it is a matter of how the premises and conclusion are related in form or structure. If a valid argument happens to have true premises it is said to be *sound*.)

Deductive logic is the study of valid arguments and Aristotelian logic is a type of deductive logic. The paradigm of deductive reasoning in science is Euclidean geometry. From a small number of premises (called axioms) it is possible to deduce an enormous number of conclusions (called theorems) about the properties of geometric figures. The good thing about deductive logic is that it is truth-preserving, which is to say that if you have a valid argument with true premises (such as argument (1)), then the conclusion will be true as well. The problem with deductive logic is that the conclusion of a deductively valid argument cannot say more than is implicit in the premises. In a sense, such arguments do not expand our knowledge because their conclusions merely reveal what their premises already state, although where the argument is complex we may find the conclusion surprising just because we hadn't noticed that it was already implicit in the premises, as with Pythagoras' theorem for example. Where the argument is simple, the fact that the conclusion says nothing new is obvious: if I already know that all humans are mortal, and that I am a human, I don't really learn anything from the conclusion that I am mortal, although I may find it strikes me with more force when it is made explicit.

The Aristotelian conception of knowledge (or scientia) restricts the domain of what is knowable to what is necessary and cannot be otherwise. Knowledge of some fact about the natural world, for example that flames go upwards but not downwards, consists of having a deductive argument that demonstrates the causal necessity of that fact from first principles; in this case, all things seek their natural place, the natural place of the element of fire is at the top of the terrestrial sphere, therefore flames near the surface of the Earth rise. In this view, geometry (in particular) and mathematics (in general) provide a model for knowledge of the natural world. Hence, the premises that one proceeds with have to concern the essence of the relevant entities. This knowledge of the essence of things, say that the natural place of fire is at the top of the terrestrial sphere, is presupposed by a demonstration, so the natural question is where does this knowledge of essences come from? The Aristotelian answer to this appeals to a kind of faculty of intellectual intuition that allows someone to perceive the causes of things directly, and among the causes that Aristotelian scientific inquiry aims to determine are the final causes of things, which is to say the ends towards which they are moving. Hence, Aristotelian science is concerned with teleology, which is the study of purposive behaviour.

The obvious objection to all this from the modern point of view is that there is little about the role of actual sensory experience in the acquisition of knowledge of how things work. If we want to know whether metals expand when heated we expect to go out and look at how metal actually behaves in various circumstances, rather than to try and deduce a conclusion from first principles. To the modern mind, science is immediately associated with experiments and the gathering of data about what actually happens in various circumstances and hence with a school of thought in epistemology called empiricism. Empiricists believe that knowledge can only be obtained through the use of the senses to find out about the world and not by the use of pure thought or reason; in other words, the way to arrive at justified beliefs about the world is to obtain evidence by making observations or gathering data. Aristotle's logic was deductive and, although he took great interest in empirical data and his knowledge of natural phenomena, especially zoology and botany, was vast, apparently he never carried out any experiments. Bacon proposed his 'inductive logic' to replace Aristotelian methods and gave a much more central role to experience and experiments.

Remember, as we saw in the discussion of Fido the guard dog, not all valid arguments are good ones. Another example of a valid but bad argument is the following:

The Bible says that God exists
The Bible is the word of God and therefore true
Therefore God exists

This argument is deductively valid because it is not possible for the premises both to be true and the conclusion false, and indeed it may even have true premises, but it is not a good argument because it is circular; we only have a reason to believe that the second premise is true if the conclusion is true, and so a non-believer is unlikely to be persuaded by it. Similarly, perhaps not all invalid arguments are intuitively bad arguments. For example:

Jimmy claims to be a philosopher
I have no reason to believe he is lying
Therefore Jimmy is a philosopher

This argument is invalid because it is possible for both premises to be true, but for the conclusion to be false, but it is nonetheless persuasive in ordinary circumstances. Validity is a formal property of arguments. Inductive reasoning, or **induction**, is the name given to various kinds of deductively invalid but allegedly good arguments. What distinguishes bad invalid arguments from good ones, if indeed there are any of the latter? Bacon claims to have an answer to this question that vastly improves on Aristotle's answer. A large part of what Bacon advocates is negative in the sense that it amounts to a way of avoiding falling into error when making judgements rather than offering a way of gaining new judgements. This negative side to the scientific method is recognisable in science today when people insist that to be a scientist one must be sceptical and prepared to break with received wisdom, and also not leap to conclusions early in the process of investigation of some phenomenon. Bacon called the things that could get in the way of right inductive reasoning the *Idols of the Mind* (which are analogous to fallacies of reasoning in deductive logic).

The first of these are the *Idols of the Tribe*, which refers to the

tendency of all human beings to perceive more order and regularity in nature than there is in reality, for example, the long-standing view mentioned above that all heavenly bodies move in perfect circles, and to see things in terms of our preconceptions and ignore what doesn't fit in with them. The *Idols of the Cave* are individual weaknesses in reasoning due to particular personalities and likes and dislikes; someone may, for example, be either conservative or radical in temperament and this may prejudice them in their view of some subject matter. The *Idols of the Marketplace* are the confusions engendered by our received language and terminology, which may be inappropriate yet which condition our thinking; so, for example, we may be led into error by our using the same word for the metal lead and for that part of a pencil that makes a mark on paper. Finally, the *Idols of the Theatre* are the philosophical systems that incorporate mistaken methods, such as Aristotle's, for acquiring knowledge.

So much for the negative aspects of Bacon's philosophy, but what of the positive proposals for how to acquire knowledge of the workings of the natural world? His method begins with the making of observations that are free from the malign influence of the first three Idols. The idea is to reach the truth by gathering a mass of information about particular states of affairs and building from them step by step to reach a general conclusion. This process is what Bacon called the composition of a Natural and Experimental History. Experiments are important because if we simply observe what happens around us we are limited in the data we can gather; when we perform an experiment we control the conditions of observation as far as is possible and manipulate the conditions of the experiment to see what happens in circumstances that may never happen otherwise. Experiments allow us to ask 'what would happen if . . .?'. Bacon says that by carrying out experiments we are able to 'torture nature for her secrets'. (Some feminist philosophers have emphasised that the conception of science as the masculine torture of feminine nature was very common in the scientific revolution and have argued that the science that we have today has inherited this gender bias.)

Experiments are supposed to be repeatable if at all possible, so that others can check the results obtained if they wish. Similarly, scientists prefer the results of experiments to be recorded by instruments that measure quantities according to standard definitions and scales so that the perception of the individual performing the experiment does not affect the way the outcome is reported to others. Bacon stressed the role of instruments to eliminate, as far as possible, the unreliable senses from scientific data gathering. In this way the scientific method of gathering data that will count as evidence for or against some view or other is supposed to ensure objectivity or impartiality. It seems obvious to the modern mind that science is all to do with experiments, but prior to the scientific revolution experiments were mainly associated with the practices of alchemists, and experiments played almost no role in Aristotle's methods.

Having gathered data from naturally occurring examples of the phenomenon we are interested in, as well as those produced by the ingenious manipulation of experimental design, we must then put the data in tables of various kinds. This process is best illustrated with Bacon's own example of the investigation of the phenomenon of heat. The first table to be drawn up is that of Essence and Presence, which consists of a list of all the things of which heat is a feature, for example, the Sun at noon, lava, fire, boiling liquid, things that have been vigorously rubbed and so on. The next table is that of Deviation and Absence by Proximity, which includes things that are as close to the above phenomena as possible but which differ by not involving heat; so, for example, the full Moon, rock, air, water that is cold, and so on. One big problem with the little that Aristotle did say about induction, as far as Bacon was concerned, was that it seemed to sanction the inference from particular instances straight to a generalisation without the mediation of so-called middle axioms. For Bacon the advantage of his inductive method was that it would avoid this problem by searching for negative instances and not just positive ones. There follows a table of Degrees or Comparisons in which the phenomena in which heat features are quantified and ranked according to the amount of heat they involve.

Having drawn up all these tables, the final stage of Bacon's method is the Induction itself. This involves studying all the information displayed in the tables and finding something that is present in all instances of the phenomenon in question, and absent when the phenomenon is absent, and furthermore, which increases and decreases in amount in proportion with the increases and decrease of the phenomenon. The thing that satisfies these conditions is to be found by

elimination and not by merely guessing. Something like the method of elimination is used by people all the time, for example, when trying to find the source of a fault with an electrical appliance such as a hi-fi system. First, one might try another appliance in the same socket; if it works then the socket is not to blame so one might next change the fuse, if the system still does not work the fuse is not to blame so one might check the connections in the plug, then one might test the amplifier, and so on. In the case of heat Bacon decides that heat is a special case of motion, in particular the 'expansive motion of parts' of a thing. This accords remarkably well with the modern understanding of heat (which was not developed until the mid-nineteenth century), known as the kinetic theory of heat according to which heat consists of molecular motion, and the faster the average velocity of the molecules in some substance then the hotter it will be.

According to Bacon, the form of expansive motion of parts is what underlies the phenomenon of heat as it is observed. Bacon thought that, following his method, one could discover the forms, which, although not directly observable, produce the phenomena that we can perceive with the senses. Once knowledge of the true forms of things was obtained then nature could be manipulated and controlled for the benefit of people. Bacon suggested that the kind of power over nature that was claimed by magicians in the Renaissance could be achieved through scientific methods. If we consider the development of science and technology since Bacon's time it certainly seems that technology has accomplished feats that surpass the wildest boasts of magicians: who would have believed a magus who claimed to be able to travel to the Moon or to the depths of the oceans; who would have imagined synthesising the materials out of which computers are made, or the transmission of images by photograph, film and television?

When Bacon says that science ought to discover the forms of things, he means, as in the case of heat, the concrete and immediate physical causes of them, and not the final causes that Aristotelians aimed to find by direct intuition, such as the cause of the motion of a dropped stone towards the Earth being the fact that the 'natural place' of the element of which the stone is composed is at the centre of the Earth. Such explanations seemed vacuous to Bacon, as with the notorious claim that opium sends people to sleep because it possesses a dormative virtue. The abandonment of the search for final causes was one of the main consequences of the scientific revolution. By the eighteenth century, the French writer Voltaire (1694–1778) in his play *Candide* was ridiculing the Aristotelian model of explanation; the character Doctor Pangloss explains the shape of the nose of human beings in terms of its function in holding a pair of glasses on the face. Bacon explicitly urged that teleological reasoning be confined to the explanation of human affairs where it is legitimate since people are agents who act so as to bring about their goals. One characteristic of natural science since Bacon is that explanations are required to refer only to the immediate physical causes of things and the laws of nature that govern them. (Whether or not this requirement is satisfied is a controversial issue, especially because evolutionary biology has reintroduced talk of functions and design into science. However, it is often claimed that such talk is only legitimate because it is, in principle, eliminable or reducible to a series of proper causal explanations. We shall return to this issue in Chapter 7.)

So the 'forms' of Bacon are the immediate causes or the general principles or laws that govern phenomena in the material world. However, Bacon's account of scientific theorising leaves us with a problem to which we shall return throughout this book, namely how exactly do we come to conceive of the forms of things given that they are not observable? In the case of heat we may be relatively happy with Bacon's induction, but motion is a feature of the observable world too and not confined to the hidden forms of things. When it comes to something like radioactivity, which has no observable counterpart, how could we ever induce its presence from tables like Bacon's? Baconian induction is meant to be a purely mechanical procedure but there will be many cases where no single account of the form of some phenomenon presents itself and where different scientists suggest different forms for the same phenomenon; an example is the debate about the nature of light which concerned two theories, a wave theory and a particle theory.

Bacon does offer us something else that may help with this problem, which is his notion of a 'pejorative instance' (although this is the subject of great controversy, as we shall see). He argues that when we have two rival theories that offer different accounts of the form of

something then we should try and design an experiment that could result in two different outcomes where one is predicted by one theory and the other by the other theory so that, if we perform the experiment and observe the actual outcome, we can choose between them. (The great seventeenth century scientist Robert Hooke (1635–1703) called such experiments 'crucial experiments'.) An example Bacon suggests is an experiment to see if gravity is really caused by the force of attraction produced by large bodies like the planets and the Sun; if this is really so then a clock that works by the gravitational motion of a pendulum ought to behave differently if it were placed up a church tower, or down a mine (further from, or closer to, the centre of the Earth respectively), hence, performing this experiment ought to allow us to tell whether the attractive hypothesis is correct. (In fact, the gravitational attraction of the Earth is stronger down a mine-shaft than up a tower, but the difference is very small and hence very hard to detect.)

This is an important idea because it implies that experiments in science will not be a simple matter of going out and gathering data but rather will involve the designing of experiments with the testing of different theories already in mind. This may seem to undermine Bacon's claim that we should record our natural and experimental history of the phenomenon we are studying without being influenced by our preconceptions (and so avoid the Idols of the Theatre), however, Bacon would argue that the need for pejorative instances will only arise once we have carried out our initial investigations and ended up with more than one candidate for the form of the phenomenon.

1.4 (Naïve) inductivism

We can abstract Bacon's method and arrive at a simple account of the scientific method. The method of Bacon rested on two pillars, *observation* and *induction*. Observation is supposed to be undertaken without prejudice or preconception, and we are to record the results of the data of sensory experience, what we can see, hear, and smell, whether of the world as we find it, or of the special circumstances of our experiments. The results of observation are expressed in what are called *observation statements*. Once we have made a whole host of observations these are to be used as the basis for scientific laws and theories. Many scientific laws are of the form of what are called *universal generalisations*; these are statements that generalise about the properties of all things of a certain kind. So, for example, 'all metals conduct electricity' is a universal generalisation about metals, 'all birds lay eggs' is a universal generalisation about birds, and so on. These are simple examples but, of course, scientific theories are often much more complicated and the generalisations and laws often take the form of mathematical equations relating different quantities. Some well known examples include:

- *Boyle's law*, which states that for a fixed mass of a gas at constant temperature, the product of pressure and volume is constant.
- Newton's *law of universal gravitation*, which states that the gravitational force, F, between two bodies with masses m_1, m_2, and separated by distance r, is given by: $F = m_1m_2G/r^2$ (where G is the gravitational constant).
- The *law of reflection*, which states that the angle at which a beam of light strikes a mirror is equal to the angle at which it is reflected.

Induction in the broadest sense is just any form of reasoning that is not deductive, but in the narrower sense that Bacon uses it, it is the form of reasoning where we generalise from a whole collection of particular instances to a general conclusion. The simplest form of induction is *enumerative induction*, which is where we simply observe that some large number of instances of some phenomenon has some characteristic (say some salt being put in a pot of water dissolves), and then infer that the phenomenon always has that property (whenever salt is put in a pot of water it will dissolve). Sometimes scientific reasoning is like this, for example, many of the drug and other medical treatments that are used today are based on trial and error. Aspirin was used to relieve headaches a long time before there were any detailed explanations available of how it worked, simply because it had been observed on many occasions that headaches ceased following the taking of the drug.

The question that we must now ask is: 'when is it legitimate to infer a universal generalisation from a collection of observation statements?', for example, when can we infer that 'all animals with hearts

have livers' on the basis of the observation of many instances of animals having hearts having livers as well. The answer according to naïve inductivism is *when a large number of observations of Xs under a wide variety of conditions have been made, and when all Xs have been found to possess property Y, and when no instance has been found to contradict the universal generalisation 'all Xs possess property Y'*. So, for example, we need to observe many kinds of animals in all parts of the Earth, and we need to look out for any instance that contradicts our generalisation. If we carry out a lot of observations and all support the law while none refute it, then we are entitled to infer the generalisation.

This accords with our common sense; someone who concluded that all philosophers are neurotic, having observed only a handful of philosophers in Bristol to be neurotic, would be considered quite unreasonable. Similarly, someone who drew such an inference having observed one perfectly stable and balanced philosopher would be considered unreasonable no matter how many other philosophers they had observed showing signs of neurosis. However, if someone claimed to believe that all philosophers are neurotic and when questioned it turned out they had observed philosophers both young and old, of both sexes and in various parts of the world over many years and they had all been neurotic to varying degrees and not one had no trace of neurosis, we would think their conclusion quite reasonable in the circumstances.

What we have just been discussing is known as a *Principle of Induction*; it is a principle of reasoning that sanctions inference from the observation of particular instances to a generalisation that embraces them all and more. We must take care to observe the world carefully and without preconception, and to satisfy the conditions expressed in the principle, but if we do this then, according to the naïve inductivist, we are following the scientific method and our resulting beliefs will be justified. Once we have inductively inferred our generalisation in accordance with the scientific method, then it assumes the status of a law or theory and we can use deduction to deduce consequences of the law that will be predictions or explanations.

It's time we caught up with the discussion with which this chapter began:

Alice: ... and so the scientific method consists in the unbiased accumulation of observations and inductive inference from them to generalisations about phenomena.

Thomas: But even if I buy that for claims about metals conducting electricity and the like, which I don't, I still don't see how induction explains how we know about atoms and all that stuff you were going on about before.

Alice: I guess it's to do with Bacon's idea about crucial experiments; someone says that there are atoms and someone else works out how to do an experiment that ought to go one way if there are atoms and another way if there are not.

Thomas: Well anyway, let's forget about atoms for now and just concentrate on your principle of induction and Bacon's idea about observation without prejudice or preconception. I can already think of problems with both of these; for one thing, how do you know that your principle of induction is true, and for another, how would you know what to start observing unless you already had the idea of metals and electricity? Observation without any bias whatsoever is impossible, and you haven't explained to me why I should believe in induction. I still reckon that science is just witchcraft in a white coat.

Further reading

For an excellent account of the scientific revolution see Steven Shapin *The Scientific Revolution* (Chicago University Press, 1996). Another introductory book is I. Bernard Cohen, *The Birth of a New Physics* (Pelican, 1987). On Francis Bacon see Chapter 3 of Barry Gower, *Scientific Method: An Historical and Philosophical Introduction* (Routledge, 1997), Chapter 2 of Roger Woolhouse, *The Empiricists* (Oxford University Press, 1988), Peter Urbach, *Francis Bacon's Philosophy of Science: An Account and a Reappraisal* (Open Court, 1987), and also the references to Bacon's works in the bibliography.

2

The problem of induction and other problems with inductivism

According to the account of scientific method that was introduced in the previous chapter (naïve inductivism), scientific knowledge derives its justification by being based on generalisation from experience. Observations made in a variety of circumstances are to be recorded impartially and then induction is used to arrive at a general law. This is an attractive view, not least because it agrees with what many scientists have claimed about their own practice. It also explains the alleged objectivity of scientific knowledge by reference to the open-mindedness of scientists when they make observations, and it keeps scientific knowledge firmly rooted in experience. I hope it is a reasonably familiar conception of how science works and how scientific knowledge acquires its justification.

We need to distinguish two questions in order to evaluate inductivism as a theory of scientific methodology:

(1) Does inductivism seem to be the method that has actually been followed by particular individuals in the history of science?
(2) Would the inductive method produce knowledge if we did use it?

The first question obviously calls for some empirical inquiry; to answer it we need to gather information from artefacts, journals, letters, testimony and so on. The second question is characteristically philosophical and concerns not our actual beliefs but whether the inductive method will confer justification on beliefs that are produced using it. We will return to question (1) later, while in the next section we will consider whether or not induction is justified.

2.1 The problem of induction

The classic discussion of the problem of induction is in *An Enquiry Concerning Human Understanding* by David Hume (1711–1776). Hume relates induction to the nature of causation and the laws of nature, and his influence on the development of western philosophy in general, and philosophy of science in particular, has been profound. To understand Hume's arguments about scientific knowledge it will be helpful to have a basic grasp of his general epistemology and theory of 'ideas'.

Hume makes a distinction between two types of proposition, namely those that concern *relations of ideas* and those that concern *matters of fact*. The former are propositions whose content is confined to our concepts or ideas, such as a horse is an animal, bachelors are unmarried, and checkmate is the end of a game of chess. (Hume also included mathematics in this category, so triangles have angles totalling 180° is another example.) Propositions concerning matters of fact are those that go beyond the nature of our concepts and tell us something informative about how the actual world is. So, for example, snow is white, Paris is the capital of France, all metals expand when heated, and the battle of Hastings was in 1066 are all propositions that concern matters of fact. Of course, these propositions are all true (as far as I know), but the distinction between relations of ideas and matters of fact applies equally to propositions that are false, so for example, a whale is a fish is a false proposition concerning relations among our ideas, and Plato died in 399 BC is a false proposition concerning a matter of fact.

According to Hume, any true proposition about the relations among our ideas is provable by deduction, because its negation will imply a contradiction. Those who have studied mathematics or logic will be familiar with the method of *reductio ad absurdum*. Essentially, the idea is that some proposition, say that there are an infinite number of prime numbers, can be proved if you can show that the negation of it is inconsistent with other things you already know. Such a proof would begin with the assumption that there is a biggest prime number. This is then used in conjunction with other assumed facts about numbers (in particular, about the existence of prime factors) to derive a contradiction. (Not all proofs have this form on the

surface but the definition of a logically necessary truth is that its negation is a contradiction.) In everyday life, something similar to this method is also sometimes employed when people try to show that an absurd or known to be false consequence follows from some proposition under discussion.

On the other hand, Hume argued that knowledge of matters of fact could only be derived from the senses because the ideas involved are logically unrelated and hence the propositions are not deductively provable. Take the proposition that Everest is the tallest mountain on Earth. The concepts involved – mountain, tallest, Earth, and that of some specific mountain in the Himalayas – have no logical relation to each other that determines the truth of the proposition, and there is no contradiction in supposing that some other mountain is the tallest. Hence, it is not possible to find out if the proposition is true merely by reasoning; only by using the senses can the status of such propositions be investigated. (Hume, who was Scottish, is a central figure in the philosophical tradition known as British empiricism, which also includes the English John Locke (1632–1704) and the Irish George Berkeley (1685–1753).) All these thinkers shared the belief that there are no innate concepts and that all our knowledge of the world is derived from, and justified by, our sensory perceptions, hence they all deny that any *a priori* knowledge of matters of fact is possible.

Hume was also very sceptical about metaphysical or theological speculation. Now, many people, including some philosophers, think that philosophy is often concerned with concepts so abstract and distanced from everyday life that they have no bearing on anything one could measure or experience, and that because of this they are more or less meaningless. Some people would also argue that thinking in this manner is a waste of time. Hume agreed and suggested that if one takes some book, or other text, and it contains neither 'abstract reasoning concerning quantity or number', nor 'experimental reasoning concerning matter of fact and existence', then it should be burned since it is merely 'sophistry and illusion'. This dichotomy is known as *Hume's fork*. (I leave it as an exercise for the reader to decide what ought to be done with the present volume.)

Hume's distinction between matters of fact and relations of ideas roughly corresponds to Immanuel Kant's (1724–1804) distinction between synthetic and analytic truths. Kant was inspired by Hume and made the latter distinction a central part of his (critical) philosophy. In the hands of a group of philosophers of science, called the *logical positivists*, in the early twentieth century, it became a way of distinguishing form from content in formal mathematical and logical languages that were used to represent scientific theories. They thought that they could separate the empirical content of theories, the synthetic part, from the theoretical and analytic part. The positivists argued that a factual statement was not meaningful if it said nothing about any past, present or future observations, in other words if it has no empirical content. This gives us a way of deciding whether someone is talking nonsense or not; we check to see if what he or she is saying has any implications for what we can observe. Positivism, which will often come up again (see especially 5.3), was very influential among philosophers and scientists for a while, and still has adherents. Many people sympathise with the idea that scientific and philosophical theories should have a definite connection to what can somehow be observed, and perhaps also measured, recorded and ultimately given a theoretical description in terms of laws and causes.

Now, it is plausible to argue that some of our knowledge of matters of fact is directly based on experience. That it is windy, cloudy and cold outside, that the light is on and the tea luke-warm, all this I seem to know by my present sensory experience. Another class of the things I know are those that I learned by the same means in the past; such knowledge is based on my memory of my perceptions. What of my beliefs about things I have not myself observed? I certainly have many such beliefs, for example, I believe that the Sun will rise tomorrow, that Everest is the tallest mountain, that my friend is currently in Scotland, and so on. These are all matters of fact because, in each case, the negation of the proposition is not a contradiction and so we cannot deductively prove them to be true. How can we *know* such things, if indeed we can?

Hume claimed that all reasoning that goes beyond past and present experiences is based on cause and effect. Suppose that you play pool a lot; it doesn't take long to notice that if you hit the white ball off centre it will impart a particular kind of spin to the next ball it hits. This is a useful generalisation about the behaviour of pool balls. You infer that hitting the ball off centre *causes* it to spin and that you can reliably predict the behaviour of the balls in future on this basis,

provided of course you can hit them right. Similarly, we observe that when the Sun is out, the Earth and the objects on its surface become warmer and we infer that this pattern of behaviour will continue in the future and that the Sun causes the objects to heat up. Hume pointed out that there is nothing logically inconsistent in a pool ball suddenly spinning the opposite way or not at all, nor is there any contradiction in supposing that the Sun might cool down the Earth. The only way we connect these ideas is by supposing that there is some causal connection between them.

Of course, many of our beliefs depend upon the testimony of others, whether in the form of spoken accounts, books, newspapers, or whatever. In such cases we believe in a causal relation between what has happened or is the case, and what the person experiences and then communicates. Once again it is a causal relation that connects ideas that have no logical relation. This is the basis of induction according to Hume, and so if we want to understand our knowledge of matters of fact we need to consider our knowledge of the relation of cause and effect. Hume argues that we can only obtain our knowledge of cause and effect by experience because there is no contradiction in supposing that some particular causal relation does not hold, and so this knowledge is of a matter of fact that could be otherwise. We cannot tell that fire will burn us or that gunpowder will explode without trying it out because there is no contradiction in supposing that, for example, the next fire we test will not burn but freeze a hand placed in it. (Of course we may be told about causal relations, but then the source of our information is ultimately still someone's experience.)

What more can we say about this relation of cause and effect? Hume argues that, just as it is only by experience that we can find out about particular causal relations, and hence make inductive inferences about the future behaviour of things in the world, so it is only by examining our experience of the relation of cause and effect that we can understand its nature, and hence see whether it is fit to offer a justification for our inductive practices. When we examine our experience of causal relations, Hume argues that it is apparent that our knowledge of cause and effect is the result of extrapolating from past experience of how the world has behaved to how it will behave in future. For example, because the experience of eating bread has always been followed in the past by the experience of feeling nourished, I suppose that bread nourishes in general and hence that the next piece of bread I eat will be nourishing. Fundamentally then, for Hume, causation is a matter of what is known as *constant conjunction*; A causes B means A is constantly conjoined in our experience with B: 'I have found that such an object has always been attended with such an effect, and I foresee, that other objects, which are, in appearance similar, will be attended with similar effects' (Hume 1963: 34–35). But of course we have not yet experienced the future behaviour of the objects in question and so belief in a particular relation of cause and effect relies upon the belief that the future will resemble the past. (This is a crucial point to which we shall return below.)

Hume further examines the concept of causality and finds that an important feature of it is that of *contiguity*, which is the relation of being connected in space and time. Often, when a causal connection is postulated between events, the events are either close in space and time or connected by a chain of causes and effects, each member of which is close in space and time to the next. So, for example, there is a causal relation between someone typing words into a computer and someone else reading words on a page, because there is an intermediate chain of contiguous causes and effects, however long and complicated. However, Hume does not say that this is always the case where there is a postulated causal connection.

Another characteristic of causal relations is that causes usually precede effects in time. Whether this is always so is not immediately obvious, because sometimes it seems that causes and effects can be simultaneous, as when we say that the heavy oak beam is the cause of the roof staying up. Furthermore, some philosophers hold that 'backwards causation' where a cause brings about an effect in the past is possible. In any case, Hume has identified the following features that usually pertain to the relation A causes B:

(1) Events of type A precede events of type B in time.
(2) Events of type A are constantly conjoined in our experience with events of type B.
(3) Events of type A are spatio-temporally contiguous with events of type B.

(4) Events of type A lead to the *expectation* that events of type B will follow.

This is called the Humean analysis of causation, but is that all there is to causal relations? Consider the following example; a pool ball X strikes another Y, and Y moves off at speed. We say that X causes Y to move, but what does this mean? We are inclined to say things like the following; X made Y move, X produced the movement in Y, Y had to move because X hit it, and so on. Hume is well aware that many philosophers have held the view that X causes Y means that there is some sort of *necessary connection* between X happening and Y happening, but he argues that this notion is not one that we really understand. His empiricism led him to argue that since we have no experience of a necessary connection over and above our experience of constant conjunction, we have no reason to believe that there is anything corresponding to the concept of a necessary connection in nature. All we ever see are events conjoined; we never see the alleged connection between them, but over time we see the same kinds of events followed by similar effects and so we get into the habit of expecting this to continue in future.

In a form of argument we will return to later he argues as follows. Consider two theories about causation: according to the first, a causal relation consists of nothing more than the Humean analysis above reveals; according to the second there is all that but also some kind of necessary connection (call this the *necessitarian* view). Hume points out that there is nothing that can be found in our experience that will tell in favour of either one of these hypotheses over the other. These are two different hypotheses that agree about everything we can observe; yet one of them posits the existence of something that the other does not. Hence, Hume argues, we should adopt the Humean analysis because it does without metaphysical complications. Implicit in this argument is an appeal to the principle called 'Occam's razor' according to which, whenever we have two competing hypotheses, then if all other considerations are equal, the simpler of the two is to be preferred. Hume's empiricism means that he thinks that, because the two hypotheses entail exactly the same thing with respect to what we are able to observe, then all other considerations that are worth worrying about are indeed equal.

So, although our inductive reasoning is founded on reasoning about cause and effect, this is no foundation at all since it is always possible that a causal relation will be different in the future. Hume argues that the only justification we have for such beliefs as that the Sun will rise tomorrow, or that pool balls will continue to behave as they do, is that they have always been true up to now, and this isn't really any justification at all. Of course, we may appeal to the conservation of momentum and the laws of mechanics to explain why X caused Y to move. Similarly, we can now appeal to proofs of the stability of the solar system and predictions of the lifetime of the Sun to justify our belief that the Sun will rise tomorrow. However, Hume would say that the causal links and laws we are appealing to are just more correlations and regularities.

Fundamentally, Hume's problem with induction is that the conclusion of an inductive argument could always be false no matter how many observations we have made. Indeed, there are notable cases where huge numbers of observations have been taken to support a particular generalisation and it has subsequently been found to be wrong, as in the famous case of the generalisation *all swans are white* which was believed by Europeans on the basis of many observations until they visited Australia and found black swans. As Bertrand Russell (1872–1970) famously argued in the *Problems of Philosophy*, sometimes inductive reasoning may be no more sophisticated than that of a turkey who believes that it will be fed every day because it has been fed every day of its life so far, until one day it is not fed but eaten. The worrying thought is that our belief that the Sun will rise tomorrow may be of this nature.

Of course, we are capable of being more discriminating. Many of our beliefs seem to be based on something like the principle of induction that we discussed at the end of the previous chapter, which allows the inference from particular observations to a generalisation when there are many observations made under a wide variety of circumstances, none of which contradict the generalisation but all of which are instances of it. Yet, such a principle also expresses a tacit commitment to the uniformity of natural phenomena in space and time. But why should the future resemble the past or the laws of nature be the same in different places? Hume points out that the proposition that the future will not be like the past is not contradictory.

Of course, in the past we have observed patterns and believed that they will continue to hold in the future and we have been right. But for Hume this is just to restate the problem, for the fact that in the past the future has been like the past doesn't mean that, in the future, the future will be like the past. In other words, our past experience can only justify our beliefs about the future if we have independent grounds for believing that the future will be like the past, and we do not have such grounds.

Similarly, we might try and defend induction with an inductive argument along the lines of the following; induction has worked on a large number of occasions under a variety of conditions, therefore induction works in general. But Hume argues that this is viciously circular: it is inductive arguments whose justification is in doubt, therefore it is illegitimate to use an inductive argument to support induction, to do so would be like trying to persuade someone that what you have just said is true by informing them that you always tell the truth; if they already doubt what you have said then they already doubt that you always tell the truth and simply asserting that you do will not move them. By definition, in inductive arguments, it is possible the premises may all be true and the conclusion nonetheless false. So any defence of induction must either appeal to a principle of induction or presuppose the justification of inductive inference. Hence, Hume thought all justifications of induction are circular.

Note that, although we have taken inductive reasoning to be that which proceeds from past experience to some generalisation about the future behaviour of things, it is really the extrapolation from the observed to the unobserved that is at issue. Hume thinks that the same problem arises even if we infer not a generalisation but just some particular prediction, like that the Sun will rise tomorrow or that the next piece of bread I eat will be nourishing.

Of course, in order to survive we have to act in various ways and so we have no choice but to assume that the next piece of fresh bread we eat will be nourishing, that the Sun will rise tomorrow, and that in numerous other ways the future will be like the past. Hume does not think his scepticism seriously threatens what we actually believe and how we will behave. However, he also thinks that we will continue to make inductive inferences because of our psychological disposition to do so, rather than because they are rational or justified. It is our passions, our desires, and our animal drives that compel us to go beyond what reason sanctions and believe in the uniformity of nature and the relation of cause and effect.

To summarise, Hume observes that our inductive practices are founded on the relation of cause and effect, but when he analyses this relation he finds that all that it is, from an empiricist point of view, is the constant conjunction of events, in other words, the objective content of a posited causal relation is always merely that some regularity or pattern in the behaviour of things holds. Since the original problem is that of justifying the extrapolation from some past regularity to the future behaviour of things appealing to the relation of cause and effect is to no avail. Since it is logically possible that any regularity will fail to hold in the future, the only basis we have for inductive inference is the belief that the future will resemble the past. But that the future will resemble the past is something that is only justified by past experience, which is to say, by induction, and the justification of induction is precisely what is in question. Hence, we have no justification for our inductive practices and they are the product of animal instinct and habit rather than reason. If Hume is right, then it seems all our supposed scientific knowledge is entirely without a rational foundation.

2.2 Solutions and dissolutions of the problem of induction

Hume accepts that scepticism cannot be defeated but also that we have to get on with our lives. However, he argues that what is sometimes today called inductive reasoning, inductive inference or ampliative inference, is not really reasoning at all, but rather merely a habit or a psychological tendency to form beliefs about what has not yet been observed on the basis of what has already been observed. He is quite sure that, despite learning of the problem of induction, people will continue to employ induction in science and everyday life, indeed he thinks that we cannot help but do so in order to be able to live our lives, but he does not think this behaviour can be justified on rational grounds. Because of the way he tries to resolve philosophical problems by appealing to natural facts about human beings and their physiological and psychological make-up, Hume is an important

figure in a philosophical tradition, called naturalism, that is particularly prominent in contemporary philosophy, although nowadays naturalists are not usually sceptics like Hume (recall from the Introduction that naturalists think that philosophy is continuous with empirical inquiry in science).

Most philosophers have not been satisfied with his sceptical naturalism and various strategies have been adopted to solve or dissolve the problem of induction. Note that some philosophers have employed more than one of the following.

(1) Induction is rational by definition

This response comes in crude and sophisticated versions; the crude version is as follows: in everyday life – in other words outside of academic philosophy – people do not use the term 'rational' to apply only to deductively valid inferences, indeed they often describe inductive inferences as rational. For example, consider three ways of making inferences about the fortunes of a football team based on past experience: if we are following the first method we predict the results of the next match by reading tea leaves; if we are following the second method we look at how the team did in their last few matches and then infer that they will do well next time if they did badly last time and vice versa; if we are following the third method we will again look at how the team did in their last few matches but then infer that they will do well next time if they did well last time and vice versa. Obviously the latter method is the one that everyone would say was the rational method, but this method is just the one that assumes that the future will be like that past and that nature is uniform. Indeed, most people would say that, in general, it is rational to base beliefs about the future on knowledge about the past. Hence, it is part of what everyone means by 'rational' that induction is rational.

This mode of philosophical argument was once very fashionable, but it is not sufficient to dispel philosophical worries about induction because when we ordinarily use a term like rational we are taking it to have some *normative* (or *prescriptive*) as well as descriptive content. In other words, we suppose that reasoning is rational because it conforms to some sort of standard and that it is the sort of reasoning that will tend to lead us to truth and away from falsity. Merely being called 'rational' is not enough to make a mode of reasoning justified, for it does not establish that the reasoning in question has the other properties that we take rational reasoning to have.

The second version of this response is more subtle. Instead of arguing that induction is rational because everyone uses the word 'rational' in a way that applies to it, we can argue that we are more certain of the general rationality of induction than we are of the validity of Hume's argument against it. In other words, we can treat Hume's argument like a paradox that leads to a conclusion that must be false (that induction is always irrational), and hence conclude that one or more of its premises must be false (although we may not be able to identify which one). This is, in fact, how most philosophers regard Hume's argument; they do not take it to show that induction is always irrational but rather to show that we do not know how to justify it. Adopting this strategy commits us to the task of working out exactly where the flaw is in Hume's argument, and also to giving some positive account of induction to replace Hume's negative one, but the point is that we may argue that there must be some such flaw even when we have no idea what it is. (Some philosophers argue that, in fact, this is the position that Hume himself held although most philosophers have taken him to be a sceptic who thinks that induction is unreasonable.)

(2) *Hume is asking for a deductive defence of induction, which is unreasonable*

Some philosophers have accused Hume of demanding a deductive defence of induction. They argue that Hume assumes, without any argument, that deduction is the only possible source of justification for all beliefs other than those we directly experience or remember. Initially this claim is attractive, after all Hume doesn't say much about what inductive reasoning is like, other than it is not deductive, and he does seem to argue that induction is unjustified because of the fact that, in an inductive argument, it is possible that the premises are all true and the conclusion nonetheless false, which is just to say that the argument is deductively invalid. So it may look as if he is arguing

that beliefs reached by inductive inference are unjustified just because the inference is non-deductive.

However, it is clear that Hume has more in mind than this because he diagnoses inductive inferences as all depending on the principle that nature is uniform. It is the fact that we have no independent reason to believe this principle that motivates scepticism about induction, in other words, because we have no reason to believe that nature is uniform in the sense that the future will resemble the past, then we have no reason to believe the conclusion of an inductive argument. This response is therefore not sufficient to dispel Hume's inductive scepticism.

(3) *Induction is justified by the theory of probability*

Many philosophers have tried to solve the problem of induction by appealing to the mathematical theory of probability. Perhaps the most detailed and sustained attempts of this kind were by Rudolf Carnap (1891–1970) and Hans Reichenbach (1891–1953), two of the greatest philosophers of science of the twentieth century. They tried to construct an *a priori* theory of inductive logic that would allow the calculation of the degree to which any particular hypothesis is confirmed. The problem with this strategy is that the application of technical results in mathematics to our knowledge of the world is impossible unless we make some substantial assumptions about how the world behaves, and such assumptions can never be justified on purely logical or mathematical grounds. Hence, we will still need to supplement our appeal to probability theory with some principle that assures us that it is applicable to the world (see the next strategy), and the problem will then be pushed back to the question of what justifies our belief that such a principle will hold in the future.

(4) *Induction is justified by a principle of induction or of the uniformity of nature*

One response to the problem of induction, which takes various forms, is to adopt some principle and insert it as a premise into inductive arguments to render them deductively valid. Suppose, for example, that we have often observed that sodium burns with an orange flame when heated with a bunsen burner. We have an inductive argument of the form:

> *N* samples of sodium have been observed to burn with an orange flame when heated with a bunsen burner.
>
> ---
>
> All samples of sodium will burn with an orange flame when heated with a bunsen burner.

As it stands this is invalid, but it becomes a valid deductive argument if we add the following premise: whenever *N* As are observed to also be Bs then all As are Bs; and let A be 'sample of sodium' and B be 'things that burn with an orange flame when heated in a bunsen burner'.

This principle is general and will also allow us to infer that all bread is nourishing by observing that *N* samples of bread have been observed to be nourishing so far. Of course, as we learned in Chapter 1, we need to add to the principles that the observations of As must be made under a wide variety of conditions, and that no instance has been found to contradict the universal generalisation that all As are Bs. If we do this, then we will be able to infer such generalisations validly as follows:

> *N* As have been observed under a wide variety of conditions and all were found to be Bs.
> No As have been observed to be non-Bs.
> If *N* observations of As under a wide variety of conditions have been made, and all were found to be Bs, and no As have been found to be non-Bs, then all As are Bs.
>
> ---
>
> All As are Bs

This is valid because it is not possible for the premises all to be true and the conclusion false; however, the obvious problem with this is that we have not yet specified how big the number *N* needs to be. Whatever number we come up with is going to seem arbitrary and, moreover, our inductive reasoning will have the following extremely counterintuitive feature; we will have no reason to believe all As are Bs at all, no matter how much evidence we have until we reach the number of observations *N* and then suddenly we will have complete

certainty that all As are Bs and further observations will be completely irrelevant. But why should any particular number of observations allow one to be certain? This problem can be avoided by weakening the conclusion so that it states that 'probably all As are Bs', and stipulating that the probability here is proportional to the size of *N*. (We shall return to this approach below.)

The other obvious problem is that we seem to lack any justification for the principle of induction that is proposed. It does not seem to be an analytic truth (a relation among our ideas) because its negation is not a contradiction, but rather a synthetic proposition (a matter of fact). So if Hume is right it must be justified by experience and then we are back to the circularity problem again.

However, perhaps Hume is wrong and some synthetic truths can be known *a priori*. This is the response to the problem of induction inspired by Kant's idea that certain principles can be known to be true *a priori* because they are, in fact, descriptive of the way our minds work and express preconditions for us to have any experience of the world at all. Kant argued that the principle that all events have causes, and perhaps also the specific laws to be found in Newton's physics are known in this way. In the eighteenth century, when Kant was writing, this may have seemed plausible because at the time Newton's laws were being applied to all kinds of celestial and terrestrial phenomena and were successful time and time again. The image of a clockwork universe in which every event follows from previous events with necessity and predictability according to the basic laws of mechanics was a great source of inspiration to scientists and philosophers, and indeed in the nineteenth century most philosophers were not too worried by the problem of induction. However, once Newtonian mechanics was found to be false because of the inaccurate predictions it gave for observations of bodies moving with very high relative velocities, and for the behaviour of very small and very large objects, the problem of induction acquired a new urgency. From the modern perspective, Kant's belief in synthetic *a priori* knowledge seems hopelessly optimistic.

(5) *Hume's argument is too general. Since it does not appeal to anything specific about our inductive practices, it can only be premised on the fact that induction is not deduction*

The point of this response is to argue that Hume's argument is supposed to apply to all forms of inductive inference but that the description Hume gave of our inductive practices was over simplistic. Hume claimed that in forming expectations about the future behaviour of things we have previously observed, we assume that the future will resemble the past. However, it is ridiculous to suggest that this is all there is to our inductive practices. Sometimes we need only observe something a few times before we conclude that it will always behave in a similar way; for example, when trying a new recipe one would conclude after two or three successful trials that the dish will usually be tasty in future, whilst on other occasions we are very cautious about inferring the future behaviour of things even after many observations. Furthermore, we may observe that certain events are repeatedly conjoined in past experience but not conclude that they will be in future; for example, I observe that all my breaths to date have been followed by further breaths but I do not infer that all my breaths will be followed by further breaths, because I fit this pattern into the rest of my inductive knowledge that includes the claim that all human beings eventually die. Hence, our inductive reasoning is more complex than Hume suggests and usually when we infer a causal connection it is because we have tested a regularity in various circumstances and found a certain stability to the behaviour of things.

Human beings and other animals are, in fact, much better at induction than they would be if they just used enumerative induction, and it is easy to see why: an animal that could only learn that something was dangerous by testing this many, many times would not survive for long; hence a child learns not to put his or her hand on a hot stove after a couple of unpleasant sensations and does not wait until it has repeated the observation over and over again. Indeed, even in science, sometimes a single experiment or a few observations is taken to provide sufficient evidence for a theory, as in the case of the famous experiment which confirmed the prediction of general relativity that the path of light would be bent by passing close to the Sun. Only a

lunatic would suggest that we need to do some more experiments to confirm that the catastrophic effects of the nuclear bombs dropped on Hiroshima and Nagasaki would recur if someone tried the same thing in the future.

So it seems that if there really are such things as inductive inferences then they are more complicated than the enumerative induction that Hume considers. Of course, this shows only that we need to describe our inductive practices in more detail before considering whether or not they are justified, but nonetheless it is argued that Hume's argument does not give us any reason to doubt them just because they are inductive. This is a promising strategy that is currently popular among some philosophers but I suspect that Hume would argue that, however sophisticated and complex our inductive practices are, they will ultimately depend on the assumption that the future will resemble the past, and that hence, if that principle cannot be justified, our inductive practices cannot be justified.

(6) *Induction is really (a species of) inference to the best explanation, which is justified*

Inference to the best explanation, which is sometimes called *abduction*, is the mode of reasoning that we employ when we infer something on the grounds that it is the best explanation of the facts we already know. For example, when somebody doesn't answer the door or the phone, we usually infer that they are not at home because that best explains the data we have. Similarly, it is argued, in science hypotheses are often adopted because of their explanatory power, for example, the hypothesis that the continents are not fixed on the surface of the Earth but are very slowly drifting in relation to one another is adopted by geologists because it explains the common characteristics of some rocks that are now thousands of miles apart, and also some correlations between the shapes of different continents.

This is a very popular way of solving Hume's problem and the appeal to inference to the best explanation is very important in the context of the debate about scientific realism. In order to evaluate this strategy we will need to consider the nature of explanation and that will be one of the main tasks of Chapter 7. For now, note that this strategy is often combined with the next one, for it is argued that the positing of causal relations or laws of nature is justified because it is the best way of explaining the existence of stable regularities in how things behave.

(7) *There really are necessary connections that we can discover*

If there really are necessary connections between events then they will ensure that the regularities we observe will continue to hold in the future (because a necessary connection is one that could not be otherwise). This idea can be developed either in terms of laws of nature or in terms of causal powers. Hume assumes that we cannot observe the necessary connections that are supposed to constitute causal relations, and argues that, therefore, we cannot know about them at all, and hence that the inductive reasoning, which depends upon the postulation of them for its justification, is without any foundation. Similarly, a Humean view of laws says that there is nothing to a law of nature over and above some regularity in events. However, we might argue that we can know about necessary connections after all. One way to defend this would be to argue that necessary connections do not need to be directly observed despite what Hume says. As mentioned above, we might argue that we know about necessary connections by inference to the best explanation. Usually when we posit some causal connection or law of nature it is not just because we have observed some regularity in phenomena, such as objects falling when we drop them, but we have also some understanding of how stable the regularity is if we vary various conditions, for example, we drop things in air, in water, we add wings to them and we observe that smoke does not fall when dropped and so on. Again we will have to postpone a proper discussion of this strategy until later.

(8) *Induction can be inductively justified after all, because even deduction can only be given a circular (in other words, deductive) justification*

This is a more sophisticated version of the circular defence of induction that Hume considers and rejects. A common way of putting Hume's argument is as follows. Induction must be justified by either a deductive or an inductive argument. A deductive argument with the conclusion that induction is justified would only be valid if at least one of the premises assumes that induction is justified (as in strategy 4 above). On the other hand, an inductive argument will only persuade us that induction is justified if we already accept that inductive arguments support their conclusions. Hence, there cannot be a non-circular or non-question-begging defence of induction.

However, as was famously illustrated in a story by Lewis Carroll (1895), deductive inference is only defensible by appeal to deductive inference and yet that doesn't lead us to reject it as irrational, so why is induction any worse off? To see this, consider the following pattern of deductive inference; someone believes some proposition, p, and they also believe that if p is true then another proposition q follows, and so they infer q. What could they say to someone who refused to accept this form of inference? They might argue as follows; look, you believe p, and you believe if p then q, so you must believe q, because if p is true and if p then q is true then q must be true as well. They reply, 'OK, I believe p, and I believe if p then q, and I even believe that if p is true and if p then q is true then q must be true as well; however, I don't believe q'. What can we say now? We can only point out that if you believe p, and you believe if p then q and you believe if p is true and if p then q is true then q must be true as well, then you ought to believe q, but once again we are just forming an if . . . then . . . statement and insisting upon the mode of inference which, by hypothesis, the person we are seeking to persuade rejects. The upshot is that this fundamental form of deductive inference, which is called *modus ponens*, cannot be justified to someone who does not already reason deductively.

The suggestion is that it is impossible to give a non-question-begging defence of any form of inference. Perhaps, then, our strategy with the inductive sceptic ought to take account of this. Hence, we can offer an inductive defence of induction to reassure those who already employ induction that it is self-supporting, but we will give up on trying to persuade someone who completely rejects inductive inference that it is legitimate, on the grounds that such a task cannot even be carried out for deduction.

(9) *Retreat to probable knowledge*

This strategy amounts to modifying the principle of induction so that it only sanctions the conclusion that all As *probably* possess property B. All scientific knowledge, it is sometimes said, is merely probable and never completely certain; the more evidence we accumulate the more certain we become but there is no end point to this process and any hypothesis, no matter how well-supported, may be false after all. Although this response to the problem of induction begins by conceding that we can never be 100 per cent certain that a generalisation will continue to hold in the future, the probabilist argues that we can come very close to certainty and that is all we need for the justification of scientific knowledge. Some versions of this response involve a theory of *degrees of belief*, according to which belief is not an all or nothing matter but a matter of degree. Degrees of belief are usually associated with dispositions to bet at different odds; for example, if you have a degree of belief of 0.5 then you are likely to bet in favour of the hypothesis only when the odds offered for it being true rise above evens. (In the form of the theory of confirmation known as *Bayesianism*, this response has been given a precise mathematical form.)

However, note that Hume's conclusion is not merely that we cannot be certain of the conclusion of an inductive argument, but the much more radical claim that we can have no reason at all to believe it to be true rather than false. This is because we have no reason to believe in the uniformity of nature. The retreat to probable knowledge does not give us any new grounds to believe in the latter, so it does not seem to solve Hume's problem. Furthermore, usually judgements about probabilities are based on the observation of frequencies; for example, we might observe that two-thirds of the population of England have brown eyes and infer that the probability of someone in England whose eyes we have not yet seen being

brown is approximately 66 per cent. However, the problem with inductive inferences, in general, is that we have no idea what proportion of the total number of instances we have observed. Indeed, universal generalisations entail an infinite number of observations and so any proportion that we observe, no matter how large, will always be a negligible fraction of the total. This is enough to show that the mere retreat to probabilism is insufficient to solve Hume's problem.

(10) *Agree that induction is unjustified and offer an account of knowledge, in particular scientific knowledge, which dispenses with the need for inductive inference*

This is the radical response to the problem of induction proposed by Karl Popper (1902–1994). We shall consider his views in the next chapter.

It should be noted that various combinations of strategies 1, 5, 6, 7, 8 and 9 are the most popular in contemporary philosophy. Hence, someone might argue that Hume's argument shows us not that induction is irrational but that something is wrong with his reasoning (the sophisticated version of strategy 1), that what is wrong is that his account of our inductive practices is too crude (strategy 5), that our inductive practices really depend on inference to the best explanation where the explanations in question involve the existence of causal relations or laws of nature (strategies 6 and 7), and that inference to the best explanation cannot be justified in a completely non-question-begging way, but then no form of inference can (strategy 8). To this we might add that we only ever end up with a high degree of belief rather than certainty and that this is the best we can achieve and is, moreover, psychologically realistic (strategy 9). Together, this amounts to a pretty strong response to the problem of induction, but even if we can solve or dissolve Hume's problem of induction we still need to provide some positive account of what it is for something to count as evidence in favour of a hypothesis. Such an account is called a *theory of confirmation* and there are several available (Bayesianism is probably currently the most popular among philosophers). The articulation of inductivism in the history of philosophy of science is closely tied to the development of increasingly sophisticated mathematical theories of probability, and the increasing use of statistics in science. However, it is worth noting that, despite a long history, there is no generally agreed upon solution to the problem of induction. It is for this reason that the philosopher C.D. Broad (1887–1971) called induction the glory of science and the scandal of philosophy.

2.3 Inductivism and the history of science

The problem of induction is a significant difficulty for inductivism as a theory of scientific methodology; however, since the former also threatens most of our everyday knowledge we ought not to reject inductivism too hastily on that basis. If we can somehow solve or dissolve the problem of induction and vindicate inductive reasoning, then in principle a large number of observations may be used to justify belief in some generalisation or scientific law. However, we still need to ask whether the account of scientific method that we developed in the previous chapter is a plausible reconstruction of the method employed in the actual history of science (recall question (1) at the beginning of this chapter). If it is not then we face a dilemma: either we conclude that the history of science is not as it should be and that scientific knowledge is therefore not justified after all; or we conclude that inductivism must be mistaken as an account of the scientific method because it fails to characterise the methods that have been used in the production of our best scientific knowledge.

Obviously, if there are just a few cases of marginal scientific theories where the method employed to develop them does not fit the inductivist model then the former horn of the dilemma may reasonably be grasped. After all, we do not expect the history of science to be always ideal and clearly there are cases where the verdict of the scientific community itself is that some scientists have not followed the scientific method. However, in such cases this also gives us good reason to reject their theories, as in the case of the manifestly racist and sexist anatomy advocated by some scientists in the nineteenth century that modern scientists regard as completely bogus. On the other hand, the more the practice of science fails to fit the inductivist account of the scientific method, especially if cases of the development

of what are taken to be among the best and most successful theories fail to fit the account, the more plausible it becomes to take this as evidence that the inductivist account is flawed.

There is a certain kind of circularity here. On the one hand we want to know whether what we take to be scientific knowledge is really justified, and on the other, any account of the nature of the scientific method that entails that most scientific theories are not justified at all is liable to be rejected for that reason. This circularity arises because most philosophers of science have some kind of prior commitment, although perhaps minimal and restricted, to the rationality of science and the justification of scientific knowledge (for example, as mentioned in Chapter 1, antirealists may limit this knowledge to a description of the phenomena and not believe that scientific theories are true descriptions of the underlying causes of what we observe). Hence, most philosophers of science think that certain core scientific generalisations, such as sodium burns orange when heated, or all metals expand when heated, are as justified as any empirical knowledge could be. From this perspective, the philosophy of science aims to articulate the nature and source of the justification that our best theories enjoy, and hence an account of the scientific method and the source of justification in science will be inadequate if it fails to apply to the development of theories that are regarded as our best examples of scientific knowledge, such as Newton's mechanics, Maxwell's theory of electromagnetism and so on.

The point about these theories is that they are used every day by engineers in numerous practical applications and, even though we know that they are only accurate to a certain extent and that they give answers that are quite wrong in certain cases, it is inconceivable that we could come to regard either as bad science. However, it is important to note that this attitude is born out of many years' experience of using these theories. I am not here claiming that we should have a prior commitment to the rationality of the practice of any particular current science, nor to the accuracy of all scientific theories. It is only with the benefit of hindsight and the ability to look back on the development of mechanics and electrodynamics over several centuries that one can be sure that these theories, like the basic principles of optics and thermodynamics among others, embody some reliable and robust generalisations about how things usually behave. Again, what

I have said in this section is not intended to suggest that we ought to believe in the literal truth of what these theories say about the causes and explanations of those generalisations, nor should we think that the predictions issued by such theories are immune from future improvement.

Given this, it is clear that, as in other areas of philosophy, we need to reach what is known as a 'reflective equilibrium' between our pre-philosophical beliefs and the results of philosophical inquiry. Consider the following analogy; in ethics we inquire into questions about the nature of the good and the general principles that will guide us in trying to resolve controversial moral issues, such as abortion and euthanasia. However, ethicists would reject any ethical theory that implied that the recreational torturing of human beings was morally acceptable, no matter how plausible the arguments for it seemed. In ethics we demand that accounts of the good do not conflict with our most fundamental moral beliefs, although we will allow them to force us to revise some of our less central moral views. So it is with the philosophy of science; accounts of the scientific method that entail that those scientists who produced what we usually take to be the best among our scientific theories were proceeding in quite the wrong way will be rejected, but we will allow that an account of the scientific method can demand some revisions in scientific practice in certain areas. Indeed, it is permissible that we might conclude that most current science is being done very badly, or we might even conclude that most scientists are bad scientists; nonetheless, we ought not to conclude that our best science is bad science.

Hence, philosophy of science needs to be informed by careful work in the history of science and not just by accepting scientists' own pronouncements about how their work proceeds. In fact many histories of science – for example, of the discoveries of Galileo, Newton and the discovery of vaccination by Edward Jenner (1749–1823) – have been written from an inductivist perspective. Newton famously claimed not to make hypotheses, but to have inductively inferred his laws from the phenomena. It will be instructive briefly to consider the development of Newton's theory to see if it fits with the inductivist model.

In his celebrated *Principia* (the full title translates as *The Mathematical Principles of Natural Philosophy*), Newton presented his

three laws of motion and his law of universal gravitation, and went on to use them to explain Kepler's laws of planetary motion, the behaviour of the tides, the paths of projectiles (such as a cannon ball) fired from the surface of the Earth, and many other phenomena. The law of gravitation stated that all massive bodies attract each other with a force (F) that is proportional to the product of their masses (m_1m_2) and is inversely proportional to the square of the distance (r) between them.

$$F=\frac{m_1m_2G}{r^2} \quad \text{(where } G \text{ is a constant)}$$

(This means that two bodies that are 10 m apart experience a force that is 100 times less than two equally massive bodies that are 1 m apart.) Newton makes a distinction between the law itself and some account of the cause or explanation of the law, and claims that his law has been inferred from the data, but also that, because no such inference leads to an account of what causes the gravitational force in accordance with the law, he suspends judgement as to what the cause might be. Indeed, Newton says that 'hypotheses', by which he means statements that have not been inferred from observations, have no place in 'experimental philosophy', being merely speculative.

A major problem with Newton's account of his own discoveries was famously pointed out by the historian and philosopher of science Pierre Duhem (1861–1916), namely that Kepler's laws say that the planets move in perfect ellipses around the Sun, but because each planet exerts a gravitational force on all the others and the Sun itself, Newton's own law of gravitation predicts that the paths of the planets will never be perfect ellipses. So Newton can hardly have inferred his laws directly from Kepler's if the latter are actually inconsistent with the former. Now consider Newton's first law, which states that every body will, unless acted upon by an external force, maintain its state of uniform motion (if it is already moving) or will remain at rest (if it is not). We have never been able to observe a body that is not acted upon by some external force or other, so again this law cannot have been inferred directly from the observational data. Furthermore, Newton introduced new theoretical concepts in his work. In particular, the notions of mass and force are both made precise and quantitative in the *Principia* and feature in the law of gravitation. However, Kepler's laws relate positions, distances, areas, time intervals and velocities and make no mention of forces and masses. How could a law, which is stated in terms of these theoretical concepts, be inferred from data where they are entirely absent?

Another historical example that is often taken to support inductivism is Kepler's discovery of his laws of planetary motion. Between 1576 and 1597, the astronomer Tycho Brahe (1546–1601) made thousands of observations of the planets, and Kepler used this data to produce his three laws, so it seems that here at least we have a case where a theory was inferred from a mass of observational evidence. However, Kepler was unable just to read off his laws from the data, rather he was motivated to search for a reasonably simple pattern to planetary motion by his somewhat mystical (Pythagorean) belief in a mathematically elegant form to the motion of the planets, which he thought of as the harmony of the spheres. There are numerous other examples of creative thinking in science where scientists certainly did not derive their theories from the data.

2.4 Theory and observation

Consider the requirement that before making an inductive inference we must examine the phenomena in question in a wide variety of conditions. Now, there are many cases of scientific laws and generalisations that were thought to be true without exception, but were then later found to be false when tested in certain situations. Newtonian mechanics is a prime example, since it is completely inaccurate when applied to things moving at very high relative velocities, yet it had been tested at lower speeds millions of times and always found to be pretty accurate. How do we know in advance what circumstances are significantly similar and different? Of course, we assume that it doesn't make any difference if the experimental device we are using is painted red or green but how do we know it doesn't? Similarly, we do not expect it to make any difference to whether a metal expands on heating whether we test this on one day or a year later, or we do it in the northern hemisphere or the southern hemisphere.

Obviously we rely upon background knowledge in deciding which circumstances to vary and which not to vary. If we are testing to see if

all metals expand when heated, we think that it may be relevant whether we use a different type of metal, how we heat the metal, and how pure the sample is, but not whether the experimenter's name has an 'e' in it or in what order we test the metals. Here, we are using our background knowledge of what factors are causally relevant. What the experimenter's name is makes no difference to experiments we have carried out in the past so we do not expect it to make any difference to the next experiment. The accuracy of experimental techniques depends upon being able to detect and screen out extraneous influences. If we are doing basic mechanics with billiard balls we try and use a very smooth and flat surface to minimise the effect of friction. We may go on to study such systems in a vacuum to minimise air resistance. This process is called 'idealisation'. Often science proceeds by studying ideal systems where various complicating factors are not present, and then applying the derived laws to real systems and modifying them as appropriate.

Bacon recommended that we free our minds of all preconceptions when undertaking scientific inquiry, but is this possible and is it even desirable? We have seen how, to be plausible as an account of the scientific method, inductivism must admit that we need to use background knowledge to screen out causal factors in which we aren't interested. It may have seemed okay to start from scratch in Bacon's time in order to avoid being misled by the received Aristotelian wisdom that had become dogmatic and unproductive, but nineteenth and twentieth century scientists were building upon well-established and complex theories. They wanted to consolidate and extend that success and not ignore it when investigating new domains. So they needed to use the theories of optics to help build telescopes to study stars and microscopes to study cells. Modern science is so complex and developed it is absurd to suggest that a practising scientist has no preconceptions when undertaking research. Scientists need specialised knowledge to calibrate instruments and design experiments. We cannot just begin with the data, we need guidance as to what data are relevant and what to observe, as well as what known causal factors to take into account and what can safely be ignored.

There is another problem with inductive inference that we face even if we could show that the future is like the past. The problem was discovered by Nelson Goodman (1906–1998) and is known as the 'new riddle of induction'. Goodman argued as follows: suppose that the future will be like the past; we observe that every emerald we have ever come across has been green and so we infer that all emeralds are green. This is an exemplary case of enumerative induction where the generalisation is supposed to be supported or justified by the observation of a large number of instances consistent with it and none that contradict it, and suppose too that we have observed emeralds under a wide variety of conditions. Now consider the property 'grue', where a thing is grue just in case it is observed prior to the year 2001 and is green, or it is only observed after 2001 and is blue. Now all the emeralds we have observed up to now have been grue by this definition, and hence all the data we have supports the generalisation 'all emeralds are grue' just as much as it supports the generalisation 'all emeralds are green'.

Of course, the predicate 'grue' is artificial but Goodman's point is that we need some way of distinguishing those predicates with which we can legitimately make inductive inferences (call these 'projectible predicates'), from those predicates which we cannot legitimately make inductive inferences with (call these 'non-projectible predicates'). Goodman's problem remains even if we solve the ordinary problem of induction, and it also shows us that we need to say more about observation. On the simple model of observation we have assumed, it is just a matter of setting up some experiment and recording what happens objectively. But the possibility of grue type predicates means that we will get into trouble if we record our observations in the wrong language. (We shall return to the problematic relationship between theory and observation later.)

2.5 Conclusions

The general lessons to be learned from the history and practice of science are as follows:

(1) Sometimes new theories refine our understanding of the data we already have and so, in general, the former cannot be simply read off or inferred from the latter. For example, we come to regard the deviations of the paths of the planets from perfect ellipses not

as observational errors but as revealing the effects of the planets' gravitational attractions between themselves.

(2) The history of science has often involved the introduction of new concepts and properties that could not have been simply inferred from the data.

(3) Theories guide us in deciding what to observe under what conditions and especially in the case of modern science; presuppositionless observation would be detrimental even if it were possible. The relationship between theory and observation is much more complex than it seems at first sight.

(4) Many different influences (dreams, religious beliefs, metaphysical beliefs, and so on) may inspire a scientist to propose a particular hypothesis other than the data he or she already knows about.

So it seems that the model of science presented at the end of the previous chapter, which the reader may have taken to be quite natural and may even have been explicitly taught at school, is inadequate. In the next chapter we will consider the influential rival account of the nature of science and the scientific method advocated by Popper.

Alice: I can't give you a reason to follow the principle of induction, but that doesn't matter because it is impossible to get someone to follow any form of argument if they just refuse to. The fact remains that the vast majority of people think it is perfectly reasonable to base expectations of the future on past experience.

Thomas: That's it? So basically you're saying that most people use induction and those that don't are mad and you can't reason with them. What makes you think you're the sane one?

Alice: The thing is, it doesn't really matter either way. Sometimes there is no way of persuading someone who refuses to believe something that everyone else knows is justified. For example when someone is in denial about something. You know people who can't admit that they are an alcoholic, or that the person they are seeing is cheating on them, when everyone else thinks it is obvious. The stupid thing is the sceptic about induction gets proved wrong all the time, every time they step and gravity pulls them down to Earth.

Thomas: But lots of the time you can't predict how the future will be, and the patterns of the past are broken.

Alice: All I'm saying is that the fact that induction can't be justified to someone who doesn't reason that way doesn't mean that those of us who do can't know that it is generally reliable and justifies our scientific knowledge. Take those people who joined that religious cult that thought the world would end in 1999, and all killed themselves at some appointed hour to join a spaceship near that comet.

Thomas: That was beautiful that comet.

Alice: It was, and we don't need to think it's anything other than a natural phenomenon to appreciate that, just like we don't need to think that a rainbow is something other than the scattering of light waves by their passage through air that has lots of small droplets of water suspended in it. A comet is a bundle of frozen ammonia and water with a few other elements thrown in, in orbit around the sun like the rest of us. It is basically just a rock reflecting light not a chariot of some god or an alien spacecraft. We know this because we have theories that have been confirmed by predicting such phenomena in the past.

Thomas: So you say, but you can't just read off the right theory from what you see.

Alice: Well you can argue all you like but I am going to carry on believing the scientists and not the people who tell me that the world will end and that I had better repent, and give them all my money. By induction, I know that they are very probably wrong, and the fact that I can't convince them doesn't mean they aren't all off their heads.

Thomas: I take your point, but look, what I said in the first place was that there is no more to the scientific method than trial and error. I try and learn by my mistakes, so if you want to call that induction then I agree that I use it but that doesn't get us any closer to atoms and all that. You still haven't explained to me how you get from the fact that we all have to use induction sometimes, to believing all that stuff about the Big Bang. Anyway, I think the point about cultists and people like that is that they aren't prepared to abandon their beliefs in the face of the evidence. They just make up some just-so story to explain why they got it wrong and carry on regardless. The only thing that is good about science is an attitude of scepticism towards the traditional dogma.

Further reading

Hume

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Induction

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Inductivism and the history of science

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3

Falsificationism

One reason for wanting a theory of scientific method is so that we can ascertain whether scientific knowledge is justified and, if so, what its limits are. This may be important for interpreting scientific results about whether there is a risk associated with eating certain foods or releasing genetically engineered organisms into the environment. It may also be important for evaluating whether scientists' theories about the origin of the universe or the nature of matter are true or merely good guides to what we observe. Even if scientific theories, such as Newtonian mechanics, are recognised by all sides to be extremely reliable for predicting all kinds of phenomena, it remains an open question whether our best scientific theories also accurately describe unobservable entities that cause what we observe.

However, there is another reason for seeking an account of the scientific method, namely that if we have such an account we may be able to use it to decide whether some theory or discipline is scientific or not. In the United States of America, for example, there is a law that bans the state from establishing any particular religion. This law has been interpreted as prohibiting the teaching in state schools of the biblical account of God's creation of the world, animals and human beings. Yet, some of those who adhere to this doctrine call it 'creation science'. They argue that since their interpretation of the biblical account of the creation is a scientific theory it ought to be taught to students as part of the science curriculum. Many people disagree with the claim that the doctrine of the creation is genuinely scientific, although of course they admit it is possible to adopt the style and superficial appearance of science. It is therefore of considerable legal,

political and religious importance whether it really is science, and this means that some account of the nature of science is needed. No matter how much self-styled creation scientists cite their alleged empirical evidence for the Garden of Eden, Noah's ark and the flood and other events of the Bible, most geologists and biologists are convinced that all the scientific evidence points to the Earth and the life it supports having been in existence for millions rather than thousands of years. (Whether or not God created the universe is another matter.) But even if they are right, is creation science just bad science rather than non-science?

Other allegations that particular theories or practices are pseudo-scientific are very much a part of contemporary scientific and political debate. For example, some scientists and philosophers have alleged that the notion of an 'intelligence quotient' (IQ) and the testing of it is pseudo-scientific (which means 'is claimed to be scientific but is not'), yet this and other forms of psychometric testing are used by schools, employers and medical agencies. Sometimes within a particular scientific discipline dissidents are labelled as pseudo-scientific. For example, an issue of the popular science magazine *New Scientist*, which I happened to read while I was writing this chapter, had an article on why some researchers were thinking of boycotting the 2000 World Aids Conference in South Africa. Some scientists think that the government there is neglecting its responsibilities by not funding the use of certain AIDS drugs and by questioning the widely held belief that AIDS is caused by the HIV virus. Professor Malegapuru Makgoba of the Medical Research Council of South Africa is quoted as saying that South Africa is becoming 'fertile ground for pseudo-science' (*New Scientist*, 29 April 2000: 15). By the way, there was also an issue around the same time on creation science which stated unequivocally 'science and religion inhabit different domains' (*New Scientist*, 22 April 2000: 3), yet if the Bible makes statements about the creation of the Earth how can this be? There seems no avoiding the fact that sometimes religious doctrines may conflict with accepted scientific theories, so if the former are dressed up in the guise of scientific theories, they need to be evaluated as such. But how can we tell whether creation science is genuine science or not? For each of parapsychology, acupuncture, astrology, homeopathy and many other practices, there have been people who have claimed the practice is scientific and others who claim it is not. Should publicly funded health, education and legal institutions be teaching and using such practices? Given the status science and scientists enjoy in contemporary life, it ought to be clear that deciding whether something is scientific or not will often be a decision with significant consequences for people's lives.

In the previous chapter, I argued that the simple account of the scientific method that was presented in Chapter 1 was inadequate. The problem of induction certainly shows that the justification of scientific knowledge is problematic and that there is a need for a precise theory of confirmation if any form of inductivism is to be defensible. However, the problem of induction also casts doubt on pretty much all of our empirical knowledge, even of everyday and trivial facts, such as that bread is nourishing or that salt placed in water will dissolve. Hence, someone wedded to naïve inductivism may be inclined to say that the problem of induction cannot be sufficient to refute it since we will be obliged to abandon so much common sense with it. Nonetheless, naïve inductivism also seems to be factually incorrect as an account of how many scientific theories were actually developed. Furthermore, the idea of presuppositionless observation seems both impossible and undesirable. It seems that naïve inductivism cannot deliver the demarcation of science from non-science because it does not give us a plausible account of how science develops, and it forces us to reject our core intuition that a theory such as Newtonian mechanics is an example of a good scientific theory. In this chapter, we will consider an alternative theory of the nature of the scientific method, and the grounds for the demarcation of science from non-science, called **falsificationism**. The discussion of falsificationism will suggest ways in which we can improve on naïve inductivism while retaining some of the core intuitions behind it, and at the end of the chapter I will formulate a more sophisticated inductivism.

3.1 Popper and the critique of Marxism and psychoanalysis

Karl Popper had a considerable influence on philosophy of science during the twentieth century and many scientists took up his ideas.

As a result, he was made a member of the Royal Society of London, which is one of the most prestigious scientific associations. In fact, Popper's falsificationism is probably now more popular among scientists than it is among philosophers. Popper also played an important role in the intellectual critique of Marxism, and his books *The Poverty of Historicism* and *The Open Society and Its Enemies* are still widely read by political theorists today. His interest in philosophy of science began with the search for a demarcation between science and pseudo-science. He tried to work out what the difference was between theories he greatly admired in physics, and theories he thought were unscientific in psychology and sociology, and soon came to the conclusion that part of the reason why people erroneously thought that mere pseudo-sciences were scientific was that they had a mistaken view about what made physics scientific.

The main battleground of the debate about demarcation is social science. The ideal of social science was a product of the eighteenth century, which was a time of general intellectual excitement and enthusiasm for the success of Newtonian physics and the other new sciences of chemistry, physiology and so on, that had recently advanced and expanded rapidly. Various thinkers suggested that the logical next step was the application of the same methods to the discovery of the laws that governed human behaviour and the way societies functioned. This period in intellectual history is known as 'the age of enlightenment and reason' and it was characterised by a profound optimism about what could be achieved if only human beings could learn to organise themselves on a rational basis in accordance with a genuine science of society. At the time, when Popper formed his views about science, in the early part of the twentieth century, there were theories of the social and psychological nature of human beings that were claimed by their adherents to fulfil the Enlightenment promise of a genuine science of society and human behaviour. Marxism and psychoanalysis were prominent among these theories.

At the funeral of Karl Marx (1818–1883), his friend and collaborator Frederick Engels (1820–1895) said that just as Darwin had discovered the scientific principles underlying the development of species, so Marx had discovered the scientific principles underlying the development of societies. Similarly, Sigmund Freud (1856–1939) claimed that his own discoveries were comparable to those of Copernicus and Darwin, and considered his theories of sexual repression, and of ego, id and superego and so on to be fully scientific. For various reasons, Marxism and psychoanalysis are both widely perceived (perhaps correctly) as somewhat discredited today; however, many of the twentieth century's greatest intellects were influenced by one or other of them, and it is arguable that their effect on the history of the twentieth century was profound. When he was young, Popper was attracted by both Marxism and psychoanalysis yet fairly quickly he grew disillusioned with them. He came to regard both as pseudo-scientific and set about trying to explain what it was about them and the way they were practised that led him to this view.

Popper realised that it was easy to think of both these theories as very successful sciences if one assumed that scientific knowledge proceeds, and is justified, by the accumulation of positive instances of theories and laws. On this view, as we have seen, the justification of a law such as all metals expand on heating would be a matter of there being many cases of particular metals that expanded when heated. Marxists and psychoanalysts both had numerous examples of phenomena that were instances of their general principles. The problem, as far as Popper was concerned, is that it is just too easy to accumulate positive instances which support some theory, especially when the theory is so general in its claims that its seems not to rule anything out. Popper certainly seems to be on to something here. People are often disdainful of horoscopes precisely because they are so general it is hard to see what would not count as supporting evidence for their claims. For example, your horoscope might read 'you will have money worries shortly'. There are not many people who don't regularly have money troubles. Similarly, suppose your astrological chart says that you lack confidence, or that you are friendly but sometimes shy. Very few people can claim to be confident in all respects or never to feel shy in some circumstances. Of course, I am not arguing here that astrology is a psuedo-science, and I am sure that some astrologers say things that are much more specific. The point is that if someone does make such vague pronouncements, it is surely not enough to make their theory scientific that many instances can be found that conform to it. Hence, Popper thought that theories that

seem to have great explanatory power are suspect precisely because so much can be explained by them.

Similarly, Popper says that many adherents of Marxism and psychoanalysis are over-impressed with explanatory power and see confirmations everywhere. He argues that Marxists see every strike as further evidence for the theory of class struggle, and that psychoanalysts treat every instance of neurosis as further evidence for Freud's theories. The trouble with their theories is they do not make precise predictions, and any phenomena that occur can be accounted for. Indeed, both theories are able to explain evidence that seems at first sight to refute them. So, for example, various measures to safeguard the safety and welfare of workers were introduced in England in the nineteenth century and this fact would seem to conflict with Marxism, according to which the ruling class has no interest in ensuring decent living and working conditions for the poor. Yet some Marxists have argued that, in fact, the introduction of the poor laws and so on confirm Marxism because they show that the capitalists were aware of the imminence of the proletarian revolution and were trying to placate the workers in order to stop or delay it.

In the case of psychoanalysis, Popper gives two different examples of human behaviour; the first is that of a man pushing a child into water intending to drown it and the second is of a man jumping in and sacrificing his life to save the child. Freud could explain the first by positing that the man suffered from repression, and the second by saying he had achieved sublimation. Alfred Adler (1870–1937) could explain the first by saying that the man suffered from feelings of inferiority and so needed to prove to himself that he could commit the crime, and the second by saying that the man also suffered from feelings of inferiority but needed to prove to himself that he was brave enough to rescue the child. Popper's complaint then is that the central principles of these theories are so general as to be compatible with any particular observations and too many of those who believe them cannot even imagine circumstances under which they would be empirically refuted because they are like a lens through which they view the world.

So, in general, Popper's worry about the idea that confirmation is fundamental to the scientific method is that if you are in the grip of a theory it is easy to find confirming instances, especially if the theory is one that is vague and general. By contrast, Popper was particularly impressed by the experimental confirmation of Einstein's general theory of relativity in 1917. The latter predicted that light passing close to the Sun ought to have its path bent by the Sun's gravitational field. The admirable thing about the theory as far as Popper was concerned was that it made a prediction that was very risky, which is to say that could easily have turned out to be false. There are plenty of other examples of such potentially falsifying, and therefore risky, predictions made by scientific theories. For example, Newton's theory predicted the return of Halley's comet during 1758, and made numerous other precise predictions for the behaviour of mechanical systems. However, the most compelling types of prediction for Popper were so-called novel predictions, which were predictions of new types of phenomena or entities. The example from general relativity mentioned above is of this kind. Another famous example is Dmitry Mendeléeff's (1834–1907) prediction of the existence of the previously unknown elements of gallium and selenium derived from the structure of the Periodic Table of the elements. Popper thought that the issuing of novel and risky predictions was a common characteristic of scientific theories and that this, combined with scientists' willingness to reject a theory if its predictions were not observed, was what made science so intellectually respectable.

So Popper argued that the 'confirmation' that a theory is supposed to get from observation of an instance that fits the theory, only really counts for anything when it is an instance that was a risky prediction by the theory; that is, if it is a potential falsifier of the theory. He thought that the impressive thing about genuine scientific theories is that they make precise predictions of surprising phenomena and genuine scientists are prepared to reject them if their predictions are not borne out by experiments. Not only are Marxism and psychoanalysis too vague to be subject to refutation by experience, but furthermore, Marxists and psychoanalysts are also sometimes inclined to side-step intellectual critique because their theories explain why people will oppose them. If one rejects Marxism one may well be accused of having a class interest in maintaining the capitalist system; similarly, someone who strongly opposes psychoanalysis may well be accused of being repressed. Of course, it is possible either or both of these claims are correct in many or even all cases; the point is just that

these theories seem to foreclose the possibility of criticism, and it was this characteristic that Popper considered anathema to science. Hence, Popper came to the view that it is not confirmation but falsification that is at the heart of the scientific method.

3.2 Popper's *solution* to the problem of induction

Popper's solution to the problem of induction is simply to argue that it does not show that scientific knowledge is not justified, because science does not depend on induction at all. Popper pointed out that there is a logical asymmetry between confirmation and falsification of a universal generalisation. The problem of induction arises because no matter how many positive instances of a generalisation are observed it is still possible that the next instance will falsify it. However, if we take a generalisation such as all swans are white, then we need only observe one swan that is not white to falsify this hypothesis. Popper argued that science is fundamentally about falsifying rather than confirming theories, and so he thought that science could proceed without induction because the inference from a falsifying instance to the falsity of a theory is purely deductive. (Hence, his theory of scientific method is called *falsificationism*.)

Popper argued that a theory that was, in principle, unfalsifiable by experience was unscientific. Examples of statements that are not falsifiable include:

> Either it is raining or it is not raining.
> God has no cause.
> All bachelors are unmarried.
> It is logically possible that space is infinite.
> Human beings have free will.

Clearly, no number of observations would be sufficient to refute any of these theories. Now as we have seen, Popper also thought that a theory like 'all neuroses are caused by childhood trauma' was unfalsifiable and so unscientific. On the other hand, he though that Marxism was falsifiable and so potentially scientific, since it predicted an internationalisation of the working class and a communist revolution. Popper just thought that Marxists were clinging on to a refuted theory and so were bad scientists. (It should be noted that here the distinction between being a bad scientist and a pseudo-scientist becomes somewhat unclear.) On the other hand, the examples of scientific theories we have considered are falsifiable because there are observations that are inconsistent with them. If we were to observe a metal that did not expand when heated we would know that the generalisation 'all metals expand when heated' was false. Similarly, if light did not obey the law of reflection we could observe this, and if bodies do not obey Newton's law of gravitation we ought to be able to observe their deviations from its predictions.

Having distinguished between falsifiable and unfalsifiable hypotheses, Popper argues that science proceeds not by testing a theory and accumulating positive inductive support for it, but by trying to falsify theories; the true way to test a theory is not to try and show that it is true but to try and show that it is false. Once a hypothesis has been developed, predictions must be deduced from it so that it can be subjected to experimental testing. If it is falsified then it is abandoned, but if it is not falsified this just means it ought to be subjected to ever more stringent tests and ingenious attempts to falsify it. So what we call confirmation is, according to Popper, really just unsuccessful falsification:

> [F]alsificationists like myself much prefer an attempt to solve an interesting problem by a bold conjecture, even (and especially) if it soon turns out to be false, to any recital of a sequence of irrelevant truisms. We prefer this because we believe that this is the way in which we can learn from our mistakes; and that in finding that our conjecture was false we shall have learnt much about the truth, and shall have got nearer to the truth.
>
> (Popper 1969: 231)

This is why Popper's methodology of science is often called the method of 'conjectures and refutations' (and indeed that was the name of one of his books). 'Bold' conjectures are those from which we can deduce the sort of novel predictions discussed above. According to Popper, science proceeds by something like natural selection and scientists learn only from their mistakes. There is no positive support for the fittest theories, rather they are just those that

repeatedly survive attempts to falsify them and so are the ones that are retained by the scientific community. It is always possible that our best theories will be falsified tomorrow and so their status is that of conjectures that have not yet been refuted rather than that of confirmed theories. Popper thought that it is here that the intellectual corruption of Marxists and psychoanalysts lies for whether or not their theories are falsifiable – they do not state clearly the conditions under which they would give up their theories. It is this *commitment* to their theories that Popper thinks is unscientific. In fact, he demands of scientists that they specify in advance under what experimental conditions they would give up their most basic assumptions. For Popper, everything in science is provisional and subject to correction or replacement:

> [W]e must not look upon science as a 'body of knowledge', but rather as a system of hypotheses which in principle cannot be justified, but with which we work as long as they stand up to tests, and of which we are never justified in saying that we know they are 'true' or 'more or less certain' or even 'probable'.
>
> (Popper 1959: 317)

The view that knowledge must be certain, a matter of proof and not subject to error has a long history in philosophy. However, from Popper we learn that we should always have a critical attitude to our best scientific theories. The history of science teaches us that even theories that in their time were considered highly confirmed and which enjoyed a huge amount of empirical success, have been shown to be quite mistaken in certain domains. Overall, the history of science has seen profound changes in fundamental principles. For example, the Newtonian conception of a world of material particles exerting gravitational forces on each other and subject to the laws of Newtonian mechanics whizzing around in the void was replaced by the idea of a field that was present at all the points of space. Special relativity and quantum mechanics meant that the basic laws of mechanics had to be revised, and general relativity has led to radical changes in the way we view the universe and space and time. On a more mundane level, heat was once widely believed to be a material fluid ('caloric') that flowed unseen but felt, but now it is thought of as a manifestation of the kinetic energy of particles; whales are no longer regarded as fish, and the age of the Earth is now thought to be millions not thousands of years.

In the light of all this, it is not surprising that today not many people believe that any scientific theory is provable beyond all doubt. Popper fully endorses the philosophical position known as *fallibilism* according to which all our knowledge of the world is provisional and subject to correction in the future. His theory of knowledge is thoroughly anti-authoritarian and this is linked to his critique of totalitarian systems of government. In his view, the programmes to create ideal societies proposed by the likes of Plato and Marx demanded rigid adherence to a single fixed ideology and the repression of all dissenting views. On the contrary, Popper thought that science flourished in an atmosphere where nothing is sacred and scientists can be extremely adventurous in the theories they propose. As his colleague Imre Lakatos (1922–1974) says, according to Popper, 'virtue lies not in caution in avoiding errors but in ruthlessness in eliminating them' (Lakatos 1968: 150). This accords with the familiar idea that scientists should be sceptical even about their own theories and should be ready to challenge any dogma if experiment demands it.

It is important to note that, unlike the logical positivists, Popper did not offer a way of distinguishing meaningful from meaningless statements and then argue that pseudo-science is meaningless. On the contrary, he thought that hypotheses that were not falsifiable could still be perfectly meaningful. Nor indeed did Popper argue that only what was falsifiable was helpful or productive even within science. Hence, he did not think that unfalsifiable metaphysical theories ought to be rejected altogether, for he recognised that sometimes scientists might be inspired to make interesting bold conjectures by beliefs that are themselves unscientific. So for example, many scientists have been influenced by their belief in God, or by their belief in the simplicity of the basic laws of physics, but clearly neither the proposition that God exists or that the fundamental structure of the world is simple is falsifiable by experience. Popper's theory of the scientific method allows such beliefs to play a role in scientific life even though they are not themselves scientific hypotheses.

Popper's main concern was to criticise pseudo-science because its adherents try to persuade people that their theories are scientific

when they are not. It does not follow from the demarcation of science from pseudo-science that he proposed that there is anything wrong with a discipline or practice being non-scientific. In fact, Popper thought that both Marxism and psychoanalysis might embody important insights into the human condition; his point is just that they are not scientific, not that they are therefore not valuable. Obviously a strong case could be made for the value of religious beliefs, and it is perfectly possible for someone with religious faith and beliefs to accept a definite demarcation between science and religion (in fact I suspect this may be the case with many scientists).

As I pointed out above, the falsificationist does not view all scientific theories equally. Some theories are falsifiable but the phenomena they predict are not interesting or surprising. Hence, the hypothesis that it will be sunny tomorrow is certainly falsifiable though it is not of great value within science. Recall that the hypotheses that Popper prizes above all others are bold conjectures that make novel predictions. In fact, Popper believed that hypotheses can be compared to see which is more falsifiable: for example, take the hypothesis (1) that all metals expand on heating; it is more falsifiable than the hypothesis (2) that copper expands on heating, because the former hypothesis is inconsistent with more observation statements, in particular, it is inconsistent with observation statements about particular bits of iron and silver not expanding when heated as well as those that just concern copper. In this case, the set of all potential falsifiers of (2) is a subset of the set of all potential falsifiers of (1), and hence (1) is more falsifiable than (2).

Popper thought that theories could be ranked according to their degree of falsifiability and that this is the true measure of their empirical content. The more falsifiable a theory is the better it is because if it is highly falsifiable it must make precise predictions about a large range of phenomena. This seems to accord with an intuitive idea of what makes a particular scientific theory a good one. Scientists ought to aim to develop theories that are as falsifiable as possible which means the theories need to be both precise and have a broad content. For example, a hypothesis such as 'metals change shape when heated' is falsifiable and broad in scope but not precise enough to be highly falsifiable, while a hypothesis such as 'this piece of copper expands on heating' is pretty precise but of narrow scope.

Ideally, from the falsificationist point of view, science ought to consist of hypotheses that apply to a wide range of phenomena, but also make precise quantitative predictions. This is the situation with many of our best scientific theories, for example, Newton's mechanics gives precise predictions for a wide range of phenomena, from the motions of comets in the heavens to the paths of cannon balls near the surface of the Earth. Popper also argued that new theories ought to be more falsifiable than the theories they replace. This certainly fits with many episodes in the history of science; for example, Newton's theory was more precise than Kepler's which it succeeded, the theory of relativity improved upon the predictions of both Newtonian mechanics and Maxwell's electromagnetic theory, and so on. It seems that some of the basic ideas of falsificationism do accord with some of our intuitions about science.

3.3 The context of discovery and the context of justification

The attentive reader may have noticed a striking difference between naïve inductivism and falsificationism, namely that the former offers an account not just of how to test a scientific theory, but also an account of how scientists ought to generate them. So recall that Bacon's new inductive logic tells us how to begin our investigation of some range of phenomena, and the production of generalisations and laws is supposed to be an automatic outcome of the mechanical operation of the method. For a long time in the history of science it was widely believed that laws ought only to be admitted if they were actually derived from experimental data, and Newton himself claimed that he did not engage in speculation but simply deduced the laws of mechanics from the results of observations. However, as was explained at the end of the last chapter, we now know that in most of the interesting cases this is just not possible. Even Newton's laws cannot be simply read off the data, and claims of the sort he made are now not taken seriously. If there is one thing that has been learned from the twentieth century debates about scientific method it is that the generation of scientific theories is not, in general, a mechanical procedure, but a creative activity. If this is right, then when we are

thinking about scientific methodology, perhaps we ought to make a distinction between the way theories are conceived and the subsequent process of testing them. In Popper's work, this distinction was absolutely pivotal because he thought that philosophy of science was really only concerned with the latter.

Popper was one of the first philosophers of science to emphasise that scientists may draw upon diverse sources of inspiration, such as metaphysical beliefs, dreams, religious teachings and so on, when they are trying to formulate a theory. He thought that none of these were illegitimate because he thought that the causal origins of a hypothesis were irrelevant to its status within science. The kind of speculation and imagination that scientists need to employ cannot be formalised or reduced to a set of rules. In a way this makes the sciences closer to the arts than they might otherwise seem. On the other hand, the sciences differ from the arts in being subject to testing by experience and this must be the final arbiter of any scientific dispute. Popper thought that the task of philosophy of science was to undertake the logical analysis of the testing of scientific theories by observation and experiment rather than to explain how theories are developed:

> [T]he act of conceiving or inventing a theory seems to me neither to call for logical analysis nor to be susceptible to it . . . the question of how it happens that a new idea occurs . . . may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge.
>
> (Popper 1934: 27)

In Popper's view then, there are two contexts in which we might investigate the history of science and the story of how certain theories come to be developed and accepted, namely the context of discovery and the context of justification. This view accords with an intuition about the autonomy of ideas from the people that have them. It is no argument against vegetarianism to point out that Hitler was a vegetarian; similarly it is no argument against Newtonian mechanics to point out that Newton was an alchemist, and had an obsessive interest in the apocryphal books of the Bible. On the other hand, it is no argument for pacifism to point out that Einstein was a pacifist. In general, the evidence in favour of a hypothesis is independent of who believes it and who doesn't, and whether an idea really is a good one is not at all dependent on whether it is a genius or a fool who first thinks of it. It seems plausible to argue that an evaluation of the evidence for a hypothesis ought to take no account of how, why and by whom the hypothesis was conceived. Some such distinction between the causal origins of scientific theories and their degree of confirmation is often thought to be important for the defence of the objectivity of scientific knowledge.

If we assume the distinction between the production of scientific theories and their subsequent testing, then we need not be troubled by the problems Bacon's theory of scientific method faced with the impossibility of freeing ourselves of all presuppositions when making observations, and the need for scientists to use background theories in the development of new ones. In fact, Bacon himself distinguished between 'blind' and 'designed' experiments and suggested that the latter were more useful in science because they will allow us to chose between two rival hypotheses that equally account for the data we have so far. The idea is that scientists faced with a choice between two seemingly equally good rival theories ought to construct an experimental situation about which the hypotheses will predict different outcomes. This is just the sort of thing Popper emphasised, and some people have argued that the standard accounts of Bacon's methodology of the sort I gave in Chapter 1 misrepresent his views and neglect the fact that Bacon anticipated what would later be called **hypothetico-deductivism.** This is the name given to the popular view that science is fundamentally about thinking up hypotheses and deducing consequences from them, which can then be used to test the theory by experiment. As I mentioned in Chapter 1, such experiments are often called 'crucial experiments', and a famous example is the experiment performed by French scientists in the eighteenth century to decide between Newton's theory of gravity and the theory preferred by those who followed René Descartes (1596–1650). The former predicted that the Earth would not be a perfect sphere but would be flattened at the poles by its own gravitational forces; the latter predicted the Earth would be elongated at the poles. The French sent expeditions to determine the dimensions of the Earth, and it was more or less as Newton's theory predicted. It is alleged that there are many such examples in the history of science, and hypothetico-deductivists believe that such experiments are of central importance

for understanding scientific methodology. However, it has been argued that crucial experiments are, in fact, impossible; this will be the subject of the next section.

3.4 The Duhem problem

According to the account of falsificationism I have given so far, scientific theories are tested as follows: scientists deduce a prediction from a hypothesis and then if observation is not consistent with the prediction when the relevant experiment is performed the hypothesis is falsified. The way of thinking about falsification suggests the following schema to represent the relationship between a theory *T* and the observation statement that falsifies *T*:

$T \vdash e$ This says that *T entails e*, where *e* is something that can be decided by observation

$\neg e$ This says that *e* is false

$\neg T$ This says that *T* is false

For example, suppose *T* is the theory that all metals expand on heating, *e* is the statement that a particular sample of copper expands on heating. Clearly, *T* entails *e*, and so if *e* is false then *T* is false; the above argument is deductively valid.

However, in reality it is never possible to deduce any statement about what will be observed from a single hypothesis alone. Rather, hypotheses have to be conjoined with other assumptions about background conditions, the reliability of measurements, the initial conditions of a system and so on. This feature of the testing of scientific theories was recognised by Duhem who said: 'an experiment in physics can never condemn an isolated hypothesis but only a whole theoretical group' (Duhem 1906: 183). Consider the experimental test of Newtonian gravitational theory by the observation of the path of a comet. The law of gravitation alone will not predict any path for the comet. We need to assign values to variables representing the mass of the comet, the mass of the other bodies in the solar system and their relative positions and velocities, the initial position and velocity of the comet relative to the other bodies in the solar system, and the gravitational constant. We also need to employ Newton's other laws of motion. This will allow us to derive a prediction of the comet's future path that we can then test by observing its actual motion using a telescope. Suppose that the comet does not follow the path that Newtonian theory predicts; where do we locate the problem? It could be that the law of gravitation is false, or that one of Newton's other laws is false, or that we have one of the values for the mass of the other bodies in the solar system wrong, or that a mistake was made in observing the comet, or that the laws of optics which we think explain how the telescope works and why it is reliable might be wrong, and so on. Clearly, the falsification of a theory by an observation is not as straightforward as the above schema suggests.

Duhem discusses a real example that was widely considered to be a crucial experiment in optics. In the eighteenth century, there were two rival theories of the nature of light; one, due principally to Newton, according to which light consists of a stream of fast moving tiny particles, and the other, due principally to Christiaan Huygens (1629–1695), according to which light consists of a wavelike disturbance propagating through a unknown medium that permeates all space. Newton's theory predicted that the speed of light in water is greater than the speed of light in air. Eventually an experiment was devised such that light from the same source would pass through both water and air, and by the clever use of a rotating mirror the situation could be arranged so that the light would form two spots, one greenish the other colourless. If light travels faster in water than in air then the colourless spot ought to be to the right of the greenish one, and vice versa if light travels slower in water than in air. So we have a case where a statement describing something observable, namely 'the colourless spot appears to the right of the greenish one', can be deduced from a theory and we can try to falsify it. When the experiment was performed it was determined that the speed of light in water is in fact less than in air, and this was widely taken to refute Newton's theory, and to support the rival wave theory.

However, as Duhem points out, the situation is not so simple. Newton's theory, from which it follows that light travels faster in water than in air, includes a whole host of assumptions other than that light consists of particles. For example, Newton assumed that the particles of light attract and repel each other but that these forces are negligible unless the particles are very close together. It is all these

hypotheses together that are inconsistent with the result of the experiment. So a more realistic schema for falsification would be as follows:

$(T\&A) \vdash e$	This says that T together with some set of auxiliary assumptions *entails* e
$\neg e$	This says that e is false
$\neg (T\&A)$	This says that the conjunction of T and the auxiliary assumptions is false

Now $\neg (P\&Q)$ is logically equivalent to $\neg P$ or $\neg Q$. (This should be obvious; if it is false that P and Q are both true, then either P is not true or Q is not true, or both.) So how do the scientists know whether T or one of the assumptions in the set A has been falsified by the experiment?

Duhem recognised that this problem was not widely appreciated. Whether or not people are thinking in falsificationist terms, people, perhaps even some scientists, often think that scientific hypotheses can be taken in isolation and tested by experiment, to be either retained or discarded on that basis. In fact, says Duhem:

> [p]hysical science is a system that must be taken as a whole; it is an organism in which one part cannot be made to function except when the parts that are most remote from it are called into play, some more so than others, but all to some degree.
> (Duhem 1906: 187–188)

Furthermore, why can't we take an instance of falsification to be a refutation of the laws of logic rather than as refuting our hypothesis? A philosopher who argues that ultimately we could chose to abandon logic, rather than reject a physical theory in the face of falsifying evidence, is the American philosopher W.v.O. Quine (1908–2000). Quine argued that it would be quite reasonable to reject a law of logic, or change the meaning of our terms, if it was more convenient than rejecting a particular theory. Quine therefore rejects the distinction between analytic and synthetic truths that Hume, Kant and the logical positivists believed to be fundamental to epistemology (see 2.1, 5.3.1, and 6.1.3). A trivial example of such a change in the meaning of a term is that of the change in meaning of 'atom' which once meant something indivisible and now refers to a particular type of collection of smaller particles. When physicists discovered that atoms were divisible, they redefined 'atom' rather than abandoning the term altogether.

Whether or not Quine is right in his more radical conclusions, it is clear that Popper must grant that there is no such thing as a completely conclusive refutation of a theory by experiment. In fact, Popper admits this and argues that as well as a set of observation statements that are potential falsifiers of the theory, there must also be a set of experimental procedures, techniques and so on, such that the relevant group of scientists agree on a way in which the truth or falsity of each observation statement can be established. Hence, falsification is only possible in science if there is intersubjective agreement among scientists about what is being tested on any given occasion. Popper argues that, in proper scientific inquiry, whenever a high-level theoretical hypothesis is in conflict with a basic observation statement, it is the high-level theory that should be abandoned. Although Popper concedes that falsification of a high-level theory by an observation statement is not a matter of the evidence *proving* the theory to be false, he does argue that it is conclusive as far as the practice of science is concerned; intersubjectively testable falsification is, he says, final. If a hypothesis has enjoyed some empirical success in the past but is subsequently falsified, it must be abandoned and a new hypothesis should be proposed. The latter should explain whatever success was enjoyed by its predecessor, but it should also have extra empirical content that its predecessor does not have. It is in this way that true science avoids the deplorable state of affairs that occurs when a pseudo-scientific theory is falsified and its adherents simply introduce a new version of the theory to which arbitrary assumptions have been added to save it from falsification.

Some people have argued that because falsification is never completely conclusive there is not really the asymmetry between falsification and confirmation that Popper thought there was. This is a mistake because it is still the case that if the scientific community accepts the truth of a statement reporting the observation of a negative instance of some theory, for example, that some particular metal does not expand when it is heated, it is logically inconsistent for the community to believe the generalisation as well. On the other, hand there is nothing inconsistent in accepting the truth of a positive

instance of the same generalisation and at the same time believing the generalisation to be false.

3.5 Problems with falsificationism

There are several problems with Popper's account of falsificationism. Some of these are specific to the details of the theory Popper first elaborated and so may be avoided by a more careful formulation or by revising some of the details. However, some are quite general and challenge the fundamental idea that it is possible to give an account of the scientific method without endorsing any kind of inductive inference. Below, some of the main criticisms of falsificationism are briefly explained.

(1) Some legitimate parts of science seem not to be falsifiable

These fall into three categories.

(a) Probabilistic statements

Science often seems to issue statements about the probability of some occurrence. For example, modern physics tells us that the half life of uranium 235 is 710,000,000 years, which means that the probability of one atom of uranium decaying in 710,000,000 years is one-half or that it is highly probable that if one starts with 1 kg of uranium then in 710,000,000 years 500 g of it will have decayed. However, such statements cannot be falsified because an experiment may produce an improbable outcome and that is consistent with the original statement – improbable things are bound to happen sometimes. Any statement about the probability of a single event is not falsifiable, so, for example, the probability that a particular coin toss will land heads is 1/2, but we cannot falsify that hypothesis by tossing the coin because the fact that the probability is 1/2 is consistent with the coin landing heads or tails on that occasion. This problem does not arise for probabilities that are defined over large populations; hence, the statement that the probability that a particular coin will land heads

50 per cent of the time during a million tosses would be considered refuted if the coin landed tails 90 per cent of the time. I won't say any more about probabilistic statements and theories except to point out that probability is a bit of a philosophical minefield for anyone, and that Popper did develop a detailed theory of probability whose merits we cannot assess here.

(b) Existential statements

Although Popper is right that a universal generalisation can be falsified by just one negative instance, many statements in science are not of this form. For example, scientific theories assert the existence of things like black holes, atoms, viruses, DNA and so on. Statements that assert the existence of something cannot be falsified by one's failure to find them. Of course, if a theory asserts the existence of something that we repeatedly fail to find in various circumstances then one has inductive grounds for thinking it won't be found in the future; however, falsificationism is supposed to allow us to do without inductive grounds for beliefs completely. This raises the question of the relationship between falsificationism and scientific realism. Popper is clear that belief in unobservable entities has often been an important influence on the ideas of scientists and has helped them generate highly falsifiable theories, such as the atomic theory of the elements that are central within physical science. However, his views on induction imply that one can never have positive grounds for believing in theoretical entities no matter how empirically successful the theories that posit them are. This contradicts the idea many people have that we have good reasons to believe that the entities to which our best current scientific theories seem to refer do in fact exist. We shall return to this issue later.

(c) Unfalsifiable scientific principles

It is arguable that some unfalsifiable principles may nonetheless be rightly considered part of scientific knowledge. So, for example, the status of the principle of conservation of energy, which states that energy can take different forms but cannot be created or destroyed, is such that it is inconceivable to most scientists that an experiment

could falsify it; rather, an apparent violation of the principle would be interpreted as revealing that something is wrong with the rest of science and it is likely that a new source, sink or form of energy would be posited. It has also been argued that the second law of thermodynamics, which states that the entropy of any closed system always increases, is of such generality that it is beyond falsification. Similarly, consider the principle that there is no 'action at a distance', in other words that all physical causation is mediated by local interactions. What this means is that whenever a distant event causes one somewhere else, there is a chain of intermediary causes and effects linking the two. For example, the vibration of strings in a piano causes your ear to vibrate and you to hear music; in this case a series of vibrations in the air is the link. This principle is unfalsifiable because whenever an apparent counter-example is found the principle simply requires that some as yet unknown medium exists. This was the case with Newton's theory of gravity, which was always regarded by Newton himself as incomplete precisely because it posited a gravitational force acting between all bodies without explaining how this force was propagated through space. Later, the idea of a field was introduced to solve the problem and this concept was extended to electromagnetic theory which deals with phenomena where similar forces (electrostatic attraction and repulsion) seem to act at a distance, such as the action of a magnet on a compass needle. The pursuit of local theories has certainly been fruitful in the history of science, and the use of other unfalsifiable, and even metaphysical, principles has also had success at various points.

There are also methodological principles that are arguably central to science but not falsifiable. So, for example, many scientists intuitively regard simple and unifying theories as, all other things being equal, more likely to be true than messy and complex ones. For example, suppose the population of sparrows is noticed to be falling in various regions. Scientists investigating the cause of these separate phenomena will usually seek a unifying explanation, say destruction of hedgerows, which simultaneously explains why sparrows, and perhaps other birds, in different places are all in decline. This principle is followed in everyday life: if a doctor observes a sudden rise in the number of patients presenting with a particular set of symptoms, he or she will probably assume that a single pathogen is responsible;

if a detective hears reports of a sudden increase in armed robberies in a certain area, he or she will probably look for a single new active gang of robbers; of course, all of them may be wrong but simplicity is only claimed to be one among a number of other *fallible* methodological principles. Some people claim that we have inductive grounds for believing in scientific theories that are simple, unified and so on, because in general the search for simple and unifying explanations has been fairly reliable in producing empirically successful theories, but they would add that we should never make simplicity an absolute requirement because sometimes nature is complex and untidy.

Of course, Popper would reject any talk of our having positive grounds for believing in scientific theories, but the problem for him is that there are many examples of scientists claiming to have been sure they were on the right track when they found a particularly simple or beautiful theory. We ought to apply the requirement of reflective equilibrium to falsificationism just as we did to naïve inductivism, so if it turned out that Popper's theory failed to be compatible with actual scientific practice that would amount to a powerful argument against it. Einstein's special relativity is a wonderful example of a scientific advance that brought unity and simplicity to a messy situation. Often in the case of physics, but also in other sciences, the mathematical formulation of a theory is at the heart of these considerations, and in order to address them properly we need to deal with specific cases closely. However, there is a more fundamental principle of simplicity that is often claimed to be essential to science, namely Occam's razor, which is roughly the prescription not to invoke more entities in order to explain something than is absolutely necessary. (This kind of simplicity is called ontological parsimony.)

We shall discuss the status of these principles in more detail later. For now, note that a falsificationist could argue that it is possible to falsify metaphysical principles by, as it were, proxy. Duhem observed that although a metaphysical theory can never imply a particular scientific theory, it can rule out certain scientific theories. For example, the Cartesian metaphysical picture of a world completely filled with matter, with no empty space whatsoever, is inconsistent with Newtonian mechanics, so arguably the success of the latter counts against the former. This idea could be developed as a response

to the present objection to falsificationism, but we shall leave this issue for now.

(d) Hypothesis of natural selection

At one time, Popper was critical of the theory of evolution because he thought the hypothesis that the fittest species survive was tautological, that is to say true by definition, and therefore not falsifiable, yet evolutionary theory is widely thought to be a prime example of a good scientific theory. Most philosophers of biology would argue that the real content of evolutionary theory lies not in the phrase 'the fittest survive', but in the idea of organisms passing on characteristics, subject to mutation and variation, which either increase or decrease the chances of their offspring surviving long enough to reproduce themselves, and so pass on those characteristics. This is supposed to account for the existence of the great diversity of species and their adaptation to the environment, and also to the similarities of form and structure that exist between them. This theory may be indirectly falsifiable but the status of evolutionary explanations is too large a subject for us to enter into here.

(2) Falsificationism is not itself falsifiable

Popper admits this but says that his own theory is not supposed to be because it is a philosophical or logical theory of the scientific method, and not itself a scientific theory, so this objection, although often made, misses its target.

(3) The notion of degree of falsifiability is problematic

The set of potential falsifiers for a universal generalisation is always infinite, so there can be no absolute measure of falsifiability, but only a relative one. Earlier on we discussed the notion of degree of falsifiability where one theory's empirical consequences are a subset of those of another theory. However, often the situation is much more complicated. For example, Einstein's theory of gravitation is supposed to be more falsifiable than Newton's, yet as we have seen empirical consequences can be derived from these theories only if they are conjoined with background theories and assumptions. So we only have reason to believe that high-level and sophisticated theories have the empirical consequences that we think they have to the extent that we believe the background theories and assumptions are themselves likely to be true. The Duhem problem means that judgements about the degree of falsifiability of theories are relative to whole systems of hypotheses, and so our basis for such judgements is past experience and this lets induction in by the back door.

As we will see in the next chapter, this problem becomes more acute if we consider the arguments that some philosophers claim show that all observations are theory laden. If this is correct, then when there is wholesale change in scientific theories there will be a change in what counts as an observable phenomenon and it will be impossible, in general, to compare the empirical content of theories from a point of view that is neutral with respect to them.

(4) *Popper cannot account for our expectations about the future*

In the second quotation in section 3.2 above Popper says that we are not entitled to believe that our best theories are even *probably* true. His position is ultimately extremely sceptical, indeed he goes further than Hume, who says induction cannot be justified but that we cannot help but use it, and argues that scientists should avoid induction altogether. But is this really possible, and is it really plausible to say that we never get positive grounds for believing scientific theories?

Our scientific knowledge does not seem to be purely negative and if it were it would be hard to see why we have such confidence in certain scientifically informed beliefs. After all, it is because doctors believe that penicillin fights bacterial infection that they prescribe it for people showing the relevant symptoms. The belief that certain causes do indeed have certain effects and not that they might not is what informs our actions. For example, according to Popper, there is no positive inductive support for my belief that if I try to leave the top floor of the building by jumping out the window I will fall hard on the ground and injure myself. If observation of past instances

really confers no justification on a generalisation then I am just as rational if I believe that when I jump out of the window I will float gently to the ground. I take it that this is an unacceptable consequence of Popper's views for there is nothing more obvious to most of us than that throwing oneself out of high windows when one wishes to reach the ground safely is less rational than taking the stairs. If we adopt Popper's nihilism about induction we have no resources for explaining why people behave the way they do and, furthermore, we are obliged to condemn any positive belief in generalisations as unscientific.

Of course, just when and how we can be justified on the basis of experience in believing general laws and their consequences for the future behaviour of the natural world is the problem of induction. Most philosophers, however, think that solving this problem is not a matter of deciding whether it is more rational to take the stairs but *why* it is more rational to do so. Popper's response to this challenge is to introduce the notion of *corroboration*; a theory is corroborated if it was a bold conjecture that made novel predictions that were not falsified. Popper says that it is rational to suppose that the most corroborated theory is true because we have tried to prove it false in various ways and failed. The most corroborated theory is not one we have any reason to believe to be true, but it is the one we have least reason to think is false, so it is rational to use it in making plans for the future, like leaving the building by the stairs and not by jumping. Popper stresses that the fact that a theory is corroborated only means that it invites further challenges.

However, the notions of boldness and novelty are historically relative; the former means unlikely in the light of background knowledge and therefore highly falsifiable, and novel means previously unknown, or unexpected given existing corroborated theories, so once again induction based on past experience is smuggled into Popper's account. Furthermore, there is an infinite number of best corroborated theories, because whatever our best corroborated theory is, we can construct an infinite number of theories that agree with what it says about the past, but which say something different about what will happen in the future. The theory that gravity always applies to me when I jump into the air except after today is just as corroborated by all my experience up to now as the alternative that tells me not to jump off tall buildings; again we seem to have no choice but to accept the rationality of at least some inductive inferences despite what Popper says.

(5) Scientists sometimes ignore falsification

If we demand of scientists that they be prepared to state in advance under what conditions they would abandon their most cherished assumptions, then we will be disappointed. We have already discussed the case of the principle of conservation of energy but there are many examples in the history of science where, instead of abandoning a theory, scientists thought up modifications or extra assumptions to save it. Popper admits this but argues that extra assumptions made to save a theory from refutation are acceptable if they entail further predictions. He distinguishes between ad hoc and non-ad hoc modifications of a theory to save it from refutation, and argues that modifications proposed after a falsifying instance must explain the partial empirical success of the old hypothesis, and have further empirical content, otherwise they will be ad hoc and therefore unacceptable within science.

For example, in the nineteenth century Newton's mechanics together with the known facts about the mass, positions and motions of the planets, predicted that the orbit of Uranus should be different from what was actually observed. Instead of regarding their theory as falsified, most scientists of the time assumed that one of the above parameters was wrong, and some proposed the existence of another planet to accommodate the data. This was acceptable according to Popper because this modification increased the empirical content of the science by predicting that this planet ought to be observable. In due course Neptune was indeed observed within one degree of arc of the position that had been predicted, and subsequently this process was repeated as measurements became more precise and Pluto was discovered.

On the other hand, there are certainly extreme cases where most people will agree that a theory has only been saved from refutation by a gratuitous assumption whose only role or justification is to save the theory. For example, in the early twentieth century someone called Velikovsky proposed a theory according to which there had

been a series of cataclysms in human history. The theory predicts that there ought to be some record or trace of these events in written or oral history, but no such records are found. This is a clear case of apparent refutation which Velikovsky accommodates by postulating that the cataclysms are so traumatic that collective amnesia prevents people from recording them. This modification is ad hoc because it adds no extra empirical content to the theory. Similarly, if the Bible is literally true, then the Earth is only about six thousand years old and the fossils of dinosaurs, which appear to be much older, seem to refute the biblical theory. However, it is always open to the fundamentalist to argue that the fossils were in fact put in place by God and made to seem much older than six thousand years old in order to test our faith. Both these ways of saving a theory from refutation seem to have a similar structure. The point about them is that there is no independent way of testing the assumption which saves the theory; it merely reconciles the theory with the potentially falsifying evidence.

Unfortunately, it turns out that there are cases in the history of science where a falsifying observation is tolerated for decades despite numerous attempts to account for it. For example, the early atomic theory of Niels Bohr (1885–1962) is actually inconsistent, yet it was widely adopted as a working model. Mercury's orbit was known to be at odds with Newtonian theory for many years yet this never led to the theory being abandoned; finally, Einstein's theory of gravitation predicted the right orbit for the planet and the Newtonian theory was regarded as falsified. It is arguable that Newtonians wouldn't give the conditions under which they would reject the basic assumptions of Newtonian physics, and so it seems lack of commitment is not essential to good science after all. More generally, it often seems to be the case that where scientists have a successful theory, the existence of falsifying observations will not be sufficient to cause the abandonment of the theory in the absence of a better alternative.

3.6 Conclusions

Popper has drawn our attention to features of good science that are now widely emphasised: a critical attitude to the received wisdom, an insistence on empirical content that is precise and wide in scope, and the use of creative thinking to solve problems with bold conjectures that open up radical new possibilities for experiment and observation. The ideas of ad hocness, novel prediction and corroboration must surely play a part in explaining the difference between right and wrong reasoning in science. Lakatos tried to improve upon Popper's falsificationism and avoid some of the problems we have discussed. However, although many scientists insist that theories ought to be falsifiable by experiment, and actively trying to falsify theories may sometimes be important and productive, it seems that we cannot explain the scientific method and the justification of scientific knowledge without recourse to induction of some form or other. Science is about confirmation as well as falsification. At least, that is what many people believe and some of Popper's ideas can help them formulate a more sophisticated inductivism.

The distinction between the context of discovery and the context of justification is used by a sophisticated inductivist to separate the question of how scientific theories are developed from the question of how to test them against their rivals. Sophisticated inductivism is not refuted by those episodes in the history of science where a theory was proposed before the data were on hand to test it let alone suggest it. Instead, the model of hypothetico-deductivism can be adopted. Theories may be produced by any means necessary but then their degree of confirmation is a relationship between them and the evidence and is independent of how they were produced. Since Bacon, there have been many more theories of inductive logic and confirmation including Mill's methods, Whewell's account of consilience, and Carnap's and Reichenbach's mathematical theories of probability. However, in the next chapter we will consider a rather different view of the scientific method.

Alice: Come on, you can't pretend we never have any positive reason to believe things. I don't know how to justify induction but sometimes it definitely is justified. Do you really think that I have no reason to believe that the next time I catch a train it will be late?

Thomas: I don't know. Maybe we have to form definite beliefs about things to live our lives, but that doesn't mean they are true.

Alice: Well, anyway, science is like everyday life in that respect. If scientists were completely sceptical all the time they wouldn't get anywhere. Sometimes they need to be committed to a theory even if it's got a few problems they can't quite see how to solve.

Thomas: But now I don't really see the difference between science and any other belief system. How can it be okay for scientists to ignore evidence that doesn't suit their prejudices?

Alice: If a theory has lots of other evidence in its favour and it works then it would be crazy to abandon it without something to replace it.

Thomas: Well if it's all a matter of what the competition is like then what we count as so-called scientific knowledge depends on what we happen to have to compare it with, so the same theory could count as knowledge one day and then not the next, just because someone else invented a better theory.

Alice: It doesn't work like that because usually new theories build on old ones so the knowledge in the old theory is preserved as science progresses.

Thomas: But not always. What about when there are revolutions in science?

Further reading

Falsificationism

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Harding, S. (ed.) (1976) *Can Theories be Refuted? Essays on the Duhem–Quine Thesis*, Dordrecht, The Netherlands: D. Reidel.
Lakatos, I. and Musgrave, A. (eds) (1970) *Criticism and the Growth of Knowledge*, Cambridge: Cambridge University Press. See especially the papers by Lakatos and Feyerabend.
Quine, W.v.O. (1953) *From a Logical Point of View*, Cambridge, MA: Harvard University Press. This contains the classic 'Two dogmas of empiricism' but is hard.

4

Revolutions and rationality

The scientific method is supposed to be rational, and to give us objective knowledge of the world. To say that scientific knowledge is objective means that it is not the product of individual whim, and it deserves to be believed by everyone, regardless of their other beliefs and values. So, for example, if it is an objective fact that smoking causes cancer, or that all metals expand when heated, then it ought to be believed equally by atheists and theists, by conservatives and liberals, and by smokers and non-smokers, if these people are to be rational. Our search for the scientific method has led us from the naïve inductivism of Bacon, which is an account of how to develop scientific theories, to the falsificationism of Popper, which is exclusively concerned with the testing of scientific theories once they have been proposed.

As we saw at the end of the previous chapter, a more sophisticated form of inductivism combines the distinction between the context of discovery and the context of justification, with the view that evidence in science does give us positive reasons for believing both scientific theories, and the generalisations about the future behaviour of things that we can derive from them. Sophisticated inductivism, like falsificationism, departs from naïve inductivism by giving an important role to non-rational factors in the development of science. After all, as we have seen, scientists might be inspired by their religion, their dreams, their metaphysical beliefs or even by blind prejudice when they are developing new theories. For this reason, the context of discovery is outside the domain of rationality; however, the context of justification is subject to the constraints