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1 Karl Popper and demarcation

> Philosophy, they say, cannot by its very nature have any significant consequences, and so it can influence neither science nor politics. But I think that ideas are dangerous and powerful things, and that even philosophers have sometimes produced ideas.
>
> Popper (1963, p. 6)

Box 1.1 Questions

What is science?

What is the difference between science and pseudo-science?

What is the difference between good science and bad science?

On what grounds should papers submitted to scientific journals be rejected or accepted?

Are Christian Science, Creation Science, Scientology, astrology, traditional Chinese medicine, or chiropractic sciences? Why or why not and why does it matter?

Is psychology a science? Good science or bad science?

How does knowledge grow?

We are constantly faced by choices concerning what to believe, choices of great practical and personal importance (Box 1.1). Will taking an anti-depressant pill help me if I am depressed? Should I prefer the use of these herbs or those drugs for my hay fever? Are the economic policies for controlling inflation of one political party more effective than those of another? Are some forms of exercise rather than others more effective for loosing weight, gaining muscle or excelling in sporting performance? What methods might help me overcome anxiety? What are the best methods for learning a second language? What principles might enable different groups of people to live together more harmoniously, or a given group to work more smoothly, or a relationship to run better?

The choice of what to believe is not just a practical matter. A desire to understand the world seems intrinsic to homo sapiens. I remember as a teenager being fascinated to find

I needed only my high-school algebra to follow arguments concerning the fundamental nature of space and time as portrayed by special relativity. The sheer fact that such elegant and counter-intuitive laws had survived severe tests counted for me as amongst mankind's greatest achievements. (It still does.) Further, the details of how evolution works, of how genetics work, of how the brain works, of how the mind works are surely worth knowing for no other reason than we gain understanding of ourselves and the universe. The burgeoning popular science section of bookshops attests to people's simple desire to understand, that is, to know the best explanations around. But what counts as a satisfactory explanation of phenomena in the world? When is one such explanation better than another? This is a philosophical problem, but not an empty one, it is a problem of real substance regardless of whether our aim is practical or it is understanding itself.

The Austrian Karl Popper (1902–1994) formulated the problem in terms of what distinguishes science from non-science (including pseudo-science). He called it the problem of demarcation. He was not interested in merely categorizing or labelling something as science or not, he was interested in the substantial problem of how we can best pursue knowledge of the world. His works inspired many scientists, including various Nobel Laureates, who publicly declared how beneficial Popper's philosophy had been to them as scientists. For most of his academic career Popper held a chair in logic and scientific method at the London School of Economics. He was also a highly influential political and social philosopher. He continued producing work right up until his death at age 92. For his achievements, Sir Karl was knighted in 1965.

Background: logical positivism

Popper was born in Vienna in1902, then an intellectual and cultural centre of the Western world. A prominent philosophical movement during Popper's time in Vienna was the so-called 'Vienna Circle' (Wiener Kreis), a group of scientists and philosophers who met regularly to discuss the implications of the major revolutions in science happening at that time, especially those triggered by Einstein's work (see Edmonds and Eidinow, 2001, for a very readable biographical account). The discussions were led by Moritz Schlick (1882–1936); attendance required his personal invitation.[1] Among the members were mathematician Kurt Gödel (of Gödel's theorem fame) and philosophers like Rudolf Carnap and Carl Hempel, and the economist Otto Neurath. They formulated a philosophical approach called *logical positivism*. It is worthwhile knowing a little bit about logical positivism to understand what Sir Karl was partially opposing.

Logical positivism was itself a reaction against a style of philosophy then popular in the German world which emphasized pompous, difficult and obscure writing. In fact, members of the Vienna Circle wondered whether such writing actually said anything at all. In order to sort meaningful statements from meaningless nonsense, the logical positivists proposed that meaningful statements were either definitions, and thus necessarily true (like 'a triangle has three sides'); or else *verifiable* empirical statements (statements about the world). Empirical statements were only meaningful if they satisfied the *verification criterion*, that is, if one could specify the steps that would verify whether the statement was true.

1. Schlick had an early death because of a vengeful and mentally unstable student. Schlick had failed the student and later slept with his wife. The student confronted Schlick on the steps of the University and shot him in the chest. Although Schlick was not a jew, the killer was subsequently hailed as a jew killer in the popular press.

For example, the statement 'my desk is three foot tall' can be verified by measuring its height with a ruler, so the statement is meaningful. But how could one verify that 'The essence of Spirit is freedom'? Or 'The world does not really exist, it is just an idea'? Or 'The world really does exist'? Or 'Free will is an illusion'? Or 'God exists'? Or 'God is loving kindness'? For the logical positivists such unverifiable statements were just *metaphysical* nonsense. Much conventional philosophy could be swept away as dealing with pseudo-problems! One could get on with the real work of developing mathematics and logic (which deal with statements which are necessarily true) and natural science. Science could be construed as sets of empirical statements, dealing only with possible observations that could be directly verified, and theoretical statements which acted as definitions linking theoretical terms (like 'electron') to observations. If you have ever read a lot of old German texts (or indeed many things written today in English – e.g. see Sokal and Bricmont, 1998), you may have some sympathy with the urge to throw much in the dustbin labelled 'gibberish' (Box 1.2).

Box 1.2 Operational definitions

A notion inspired by logical positivism, and used extensively in modern psychology, is the idea of an *operational definition*, introduced by the Nobel Laureate physicist Percy Bridgeman in 1927. An operational definition defines the meaning of a concept in terms of the precise procedures used to determine its presence and quantity. For example, an operational definition of intelligence could be the score obtained on a certain IQ test. An operational definition of unconscious knowledge could be above baseline performance on a knowledge test when the person verbally claims they are guessing. An operational definition of the degree of penis envy in women could be the number of pencils returned after an exam. Experimental psychologists habitually produce operational definitions. They are invaluable but often the 'definition' is actually not a definition at all but rather a way to measure more-or-less imperfectly the thing we want to measure. More-or-less imperfectly measuring is different from defining; if the measurement is a definition the outcome is never imperfect. Defining being 'successfully anaesthetized' as 'having received a standard dose of anaesthetic' might strike some unfortunate people as having completely missed the point. When a psychologist has his back against the wall in an argument to the effect he is not measuring what he claims, there is the temptation for him to say, for example, 'But what I MEAN by emotional intelligence is a score on this test'. (The word 'mean' is always emphasized in this tactic.) The temptation should normally be resisted in psychology. It is rarely the case that what we *mean* by a concept is exhausted by some particular way of measuring it. An operational definition should not be an excuse to stop thinking critically about the validity of one's measure.

The logical positivists had two problems in determining whether a sentence was verifiable. There is the problem of how to verify statements about specific individuals and their observable properties, like 'Emma the swan is white'; and there is the problem of verifying generalizations like 'All swans are white'. The first problem was meant to be solved by direct observation, and the second by a putative logical process called *induction*. Induction can be contrasted with *deduction*. Deduction is the process of drawing inferences such that if the premises are true the conclusion is guaranteed to be true. For example:

All swans are white
Sam is a swan
____
Conclusion: Sam is white

Induction is the process of inferring universal rules given only particular observations:

Sam the swan is white;
Georgina the swan is white;
Fred the swan is white;
. . .
Emma the swan is white
____
Conclusion: All swans are white (?)

The conclusion here cannot be guaranteed to be true, hence the inference is not deductive. But still one instinctively feels the conclusion has increased in plausibility by the repeated particular observations. We feel very sure the sun will rise tomorrow for no other reason than it has done so repeatedly in the past. You might say, we also have a theory of gravitation that predicts the sun will continue to rise. But why do we feel confident in that theory other than for the fact that it has worked on so many particular occasions in the past? Inductivists, including logical positivists, believe that science proceeds by induction. Science is objective because it is based on actual observations rather than just speculation, and it goes from those particular observations logically – inductively – to general rules.

With the Second World War and Hitler's dismantling of the university system in Germany and neighbouring countries, many Jewish intellectuals, including members of the Vienna Circle, sought refuge in the United States. In fact, logical positivism went from being a minority philosophical position in Europe before the war to being the dominant force in American philosophy by about 1960 (see Giere, 1999, Chapter 11). Since then many people have defined their philosophy of science either as being in the same tradition or as reacting against logical positivism. Popper, however, is the person who credits himself with killing it off (Box 1.3).

Box 1.3

Consider the following theory: In human beings, the hippocampus is required for spatial navigation.

You find Sam who, due to a recent unusual viral illness, has destruction of all his hippocampus and no other brain structure. His spatial navigation is very bad.

Have you established the theory is true?

What about if you found eight teenage drivers who all destroyed their hippocampi (and no other brain structure) in car accidents. Their spatial navigation is very bad.

Have you established the theory?

Your ninth teenage driver with complete destruction of the hippocampus has excellent spatial navigation.

What can you conclude about the theory?

The problem of induction

Popper denied all aspects of logical positivism just described. The positivists rejected metaphysics as meaningless; Popper argued that metaphysics could be not only meaningful but also important. The positivists wished to view science as a method of moving towards certain knowledge: knowledge based on a firm foundation of observation and induction. Popper denied that either observation or induction provided means for moving towards certain knowledge. He denied further that certainty was even the goal of science. The core feature of Popper's philosophy was fallibilism: we may be wrong in anything we believe. We will see how this principle can be turned into a practical philosophy of science that allows knowledge to progress!

Induction had received a crippling, if not fatal, attack earlier by the Scottish philosopher David Hume (1711–1776). Hume argued that we are never justified in reasoning from repeated instances of which we have experiences (e.g. different swans being white) to other instances of which we have had no experience yet (other swans we are yet to observe the colour of). Induction is never justified. No matter how often we have seen a white swan, it never follows that the next swan we see will be white, yet alone that all swans are white. (A famous example because when the British went to Australia they finally found swans that were black!) A common response is to accept that, yes, from particular observations no generalization follows with certainty – but surely the *probability* of the generalization is increased with each supporting observation. Each time we see a white swan, is it not more likely that all swans are white? Hume pointed out that this does not follow. No matter what observations up to now support a generalization, it may be the case that every instance after *now* fails to support it. No matter how often your car has started early in the morning, one day it will not, and one day will mark the point that the car never starts again. Indeed, you might feel that the more days your car has started, the *less* likely it is it will start the next day, because that is the effect of age on cars! One morning the chicken at the Colonel's ranch is greeted by someone who is not offering breakfast. But surely, you might reply, induction has empirically proved itself because using induction in the past to infer generalizations has often been successful. This argument, Hume pointed out, assumes that induction is true in order to argue for it: No matter how often induction has worked in the past, there is no reason to think it will work ever again. Not unless you already assume induction, that is.

Popper (e.g. 1934, 1963, 1972) accepted all these arguments. The claim that an explanatory universal theory is true is never justified by assuming the truth of any number of observation statements. One stunning intellectual event happening during Popper's formative years was the replacement of Newtonian physics by relativity and quantum physics. No theory had received more 'inductive support' – over several hundred years – than Newtonian physics. But in the space of a few years it went from a theory widely regarded as being the established truth to one recognized as literally false. The moral for Popper was clear: We can never actually establish the truth of our theories. Establishing truth is not what science does. No matter how strongly we or others believe a theory, the theory is and always remains just a guess, even if it is our best guess. Induction does not exist.

What is the problem that people wished to explain by postulating induction? Typically, Popper points out, the situation is that we have several theories which compete as explanations and we must choose between them. Bertrand Russell (1872–1970), the grandfather of the logical positivists, thought that without working out how induction could be valid we could not decide between a good scientific theory and the 'mere obsession of a madman'. Popper argued that we do not need induction to solve the problem of theory choice. He pointed out that while assuming the truth of a particular observation or test statement never allows us to justify the claim that a universal theory is *true*, it sometimes allows us to justify the claim that the universal theory is *false*. For example, accepting that 'Sam the Swan is black' allows us to conclude that the universal claim 'All swans are white' is false. Although we cannot *establish* our theories, we may be able to successfully criticize them. There is a genuine asymmetry here in our ability to falsify versus establish a theory, given we accept singular observations. Popper exploits that asymmetry to the full in developing his philosophy of science. Rationality consists in critical discussion, trying to find the weaknesses in a theory or argument, weaknesses that may be shown by, for example, observations – observations used to criticize the theory.

The role of critical discussion

How does knowledge grow? According to Popper, there is only one practicable method: critical discussion. Knowledge does not start from unprejudiced pure observation. There is no such thing: All observation involves some theory, some prejudice. Consider the ridiculous nature of the task of just telling someone: ‘Observe!’. Observe what? For what purpose? Science is not built from naked observations. One starts with a theory, a conjecture. Then one tries to refute it. If the theory resists refutation, new lines of criticism can be attempted. In the light of a successful refutation, a new conjecture can be proposed to deal with the new problem situation.

Popper points out that in most societies in human history and around the world we find schools of thought which have the function not of critical discussion but to impart a definite doctrine and preserve it, pure and unchanged. Attempting to change a doctrine is heresy and will probably lead to expulsion from the school. In such a tradition, the successful innovator insists that he is just presenting what the master’s original doctrine really was before it was lost or perverted! The critical tradition, in contrast, was founded by explicitly establishing the method of proposing a conjecture then asking your students, after having understood it, to try to do better by finding the weaknesses in the proposal. In the critical tradition, the aim is not to preserve a doctrine but to improve it. Popper (1994) suggests that remarkably this method was invented only once in human history. Whether or not Popper’s historical conjecture is true is irrelevant for his main point, though it remains an intriguing speculation. Popper suggests that it was Thales (c.636–c.546 BCE) who established the new tradition of free thought in ancient Greece. The students of Thales openly criticized him and presented bold new ideas to overcome those criticisms (for details, see Popper, 1963, Chapter 5). The growth in knowledge in the space of a few years was breathtaking. Xenophanes (570–480 BCE) spread the critical tradition, expressing the philosophy that all our knowledge is guess work in this poem (Popper’s translation, in his e.g. 1963):

> But as for certain truth, no man has known it
> Nor will he know it; neither of the gods,
> Nor yet of all the things of which I speak.
> And even if by chance he were to utter
> The perfect truth, he would himself not know it;
> For all is but a woven web of guesses

Though we can never know if we have the truth, we can always try to improve on what we have. This critical tradition died in the West, according to Popper, when an intolerant Christianity suppressed it. It smoldered in the Arab East, from where it finally migrated and ignited the Renaissance and modern science. Once again there was an explosion in knowledge. The scientific tradition just is this critical tradition.[2]

2. In developing your own critical skills, bear in mind the advice of Donald Broadbent: Stand on the shoulders of those who have gone before you and not on their faces.

Popper's historical conjecture about the critical tradition arising only once is a reminder not to take the tradition for granted. It would be easy to see claims of the importance of criticism as platitudes. But participating in a tradition of one's students and colleagues criticizing oneself is not psychologically easy. Further, regardless of the intrinsic difficulties, external attacks and erosions are all around, from religious, party political and corporate authoritarianism. To take an extreme example, in 1957, Mao said, 'Let a hundred flowers bloom; let the hundred schools of thought contend', meaning let everyone voice their opinions so the best ideas may survive. Those who voiced opinions critical of Mao were silenced.

What is science?

Popper rejected the logical positivists attempt to distinguish meaningful statements from nonsense but instead sought to distinguish science from non-science, or metaphysics. But that does not mean that Popper believed in a scientific method as a specifiable formula to be followed for developing knowledge. 'As a rule, I begin my lectures on Scientific Method by telling my students that scientific method does not exist. I add that I ought to know, having been, for a time at least, the one and only professor of this non-existent subject within the British Commonwealth' (1983, p. 5). Popper despised compartmentalizing knowledge into subjects in any case. What is really important is just interesting problems and attempts to solve them. For example, there is the problem (which I find interesting) of explaining why hypnotized people behave as they do. Solving that problem may involve thinking about it in ways typical of the subjects of philosophy, cognitive and social psychology, or neuroscience. But none of *those* 'subjects' has any separate reality (beyond being useful administrative divisions for organizing a university); I may blend their ideas in my own conjectures concerning hypnosis. All relevant knowledge should be brought to bear on interesting problems. Nonetheless, 'Scientific Method holds a somewhat peculiar position in being even less existent than some other non-existent subjects' (p. 6).

There is according to Popper no method of discovering a scientific theory, no method of inferring a theory from 'pure' observation. Science consists of freely, creatively *inventing* theories: Science is made by people. There is also according to Popper no method – like induction – of determining the truth of a theory, no method of verification. There is not even any method of determining whether a theory is probably true. This follows from Hume's critique of induction. Many philosophers refuse to accept this conclusion (see e.g. Salmon, 2005, for a defense of the Popperian thesis, see Miller, 1994, Chapter 2. For an accessible introduction to arguments concerning induction, see Chalmers, 1999, Chapter 4). We will see later in the book that there is an approach, the Bayesian approach, aimed precisely at determining the probability of a hypothesis. But the Bayesian approach never actually answered Hume's critique as such; instead, the argument is *assuming that the world follows a certain specified type of model* (i.e., *assuming* generalizations of a certain type hold and will continue to hold)*, then the probabilities of different versions of the model can be calculated*. Popper rejected such approaches, not only because he accepted Hume's critique but also because he rejected the relevance of the subjective probabilities the Bayesians used. This issue is very much a current live debate and is explained further in Chapter 4.

Popper thought how people invent their theories is not relevant to the logic of science. The distinction between the process of conceiving a new idea (which Popper, 1934, called the psychology of knowledge) and the process of examining it logically (the logic of knowledge) is more commonly known as the distinction between the *context of discovery* and the *context of justification*. These latter terms were introduced by Hans Reichenbach (1891–1953) in 1938. Reichenbach had founded the 'Berlin Circle' and a type of logical positivism. He had been dismissed from the University of Berlin in 1933 due to Nazi racial laws and eventually established himself in the United States. Reichenbach illustrated the distinction between the context of discovery and the context of justification by the distinction between psychological and historical facts concerning Einstein the man, on the one hand, and the logical relation of his theory of general relativity to relevant evidence, on the other (see Giere, 1999). Giere suggests that Reichenbach was motivated by the cultural climate at the time to deny that the characteristics of a person, such as being a Jew, has anything to do with the scientific validity of a hypothesis proposed by the person. The hypothesis stands or falls on its own logical merits independently of who thought it up or how they thought it up (see Box 1.4).

Box 1.4 Is there a distinction between the contexts of discovery and justification?

The distinction between the context of discovery and the context of justification has good face validity. For example, Kekule is said to have thought of the idea of a ring of carbon atoms as the structure of benzene by dreaming of snakes biting their tails. The process of dreaming is part of the context of discovery or the psychology of knowledge and irrelevant to the evaluation of his hypothesis. It is the hypothesis itself – of a ring of atoms – and its relation to laboratory evidence that belongs to the logic of knowledge or the context of justification. Nonetheless, the usefulness of the distinction between the contexts of discovery and justification has been controversial. Thomas Kuhn (1969), for example, rejected the distinction. He did not believe that there was a special logic of scientific knowledge to make a distinctive context of justification. Kuhn also thought the processes by which a particular scientist within a discipline comes to discover new knowledge is an integral part of what makes the practice scientific. We will discuss Kuhn's position in more detail in the next chapter. Even within Popper's philosopher the distinction can sometimes be blurred. As we will see, central to Popper's account of science is that scientists adopt a 'falsificationist attitude'. Such an attitude is surely as much part of the psychology of knowledge as its logic. The psychological problem of what factors facilitate scientific discovery is a theoretically interesting and educationally important problem on which progress has been made; for example, in the work of Peter Cheng on the role of diagrams (e.g. Cheng & Simon, 1995) and of Roger Shepard on thought experiments (Shepard, 2001). Diagrams and images aid *psychologically* by embodying the underlying *logic* of the scientific problem. However, while the contexts of discovery and justification can be intertwined, Salmon (2005, Chapter five) believes that the distinction is still 'viable, significant, and fundamental to the philosophy of science' (p. 85) if not to scientists themselves. As scientists, we are concerned with the logical relations between theory and evidence, stripped of accidental irrelevancies concerning the discovery of the theory or of the evidence. Just what is relevant and what is irrelevant to these logical relations is an issue every chapter of this book will bear on.

There are no set methods for creating theories and there are no methods at all for showing a theory to be true. According to Popper, theory testing is not inductive, but deductive: Accepting certain observation statements can show a theory is false (the one black swan showing that not all swans are white). This is how observations make contact with theories; this is how, therefore, our knowledge acquires its empirical character. Science can only work in this way if a theory is falsifiable to begin with: The theory says certain things cannot happen. Non-science or metaphysics is, by contrast, non-falsifiable. That is a logical property distinguishing science from metaphysics. This distinction does not render metaphysics meaningless in any way; it just is not science. Metaphysical knowledge can still grow by critical discussion; but scientific knowledge can also grow by the feedback provided by actual observations as part of that critical discussion. In sum, falsifiability is Popper's demarcation criterion between science and metaphysics.

Popper sees science as the process of proposing falsifiable theories then rigorously attempting to falsify them. It is only when theories are falsified that we get feedback from Nature and a chance to improve our knowledge. But one must be clear: Theories that survive rigorous attempts at falsification are NOT proved or established. They are 'corroborated' as Popper puts it; they survive and prove only their mettle not their truth. They can be held only tentatively. Because they are held only tentatively, in a genuine scientific tradition, the researchers will have a 'falsificationist attitude'. That is, on Popper's view, it is not enough for empirical knowledge to grow that the theories be simply falsifiable as such; the community as a whole must be actively trying different ways of falsifying the proposed theories. In this way, the community becomes part of the critical tradition.

Box 1.5 Why do people often prefer the food they were brought up on, but not always?

Consider the following two factor theory of liking:

Factor 1: We are programmed to like familiar things (e.g. foods, people, animals, tools, etc.) because our knowledge and skills are likely to apply to them. They are not dangerous, we can deal with them. Thus, there is a mechanism that automatically makes us like things as we come across them more often.

Factor 2: But we also get bored with familiar things, because there is little to learn from them and we have a drive to learn.

These two factors act in opposition to each other.

So increasing people's exposure to a new thing can

1. Increase people's liking because the familiarity means it is safe (first factor operating)
2. Decrease peoples liking because they get bored (second factor operating)
3. First increase then decrease liking because the first factor operates initially before boredom becomes stronger
4. First decrease then increase liking because boredom operates initially before the first factor becomes stronger

The theory is a good one because it explains all these outcomes.

Discuss.

Psychoanalysis, Marxism and Relativity

Popper was impressed by two opposing types of experiences in 1919: on the one hand, with Marxism and psychoanalysis; on the other hand, with Einstein. As Popper puts it,

> Admirers of Marx, Freud and Adler [a student of Freud's] were impressed by the ability of the theories to explain everything that happened within their domain. They saw confirming instances everywhere; whatever happened always confirmed it. Its truth appeared manifest; people who did not see the truth refused to because of their class interest or

Popper believed that there were genuine problems in philosophy, and that he Popper had even solved some (including the problem of demarcation between science and non-science). Yet, he comments, 'nothing seems less wanted than a simple solution to an age-old philosophical problem' (Popper 1976, pp. 123–124). Wittgenstein held there were no genuine philosophical problems only puzzles created by the inappropriate use of language. In their one and only meeting at a seminar at Kings College Cambridge, 5 October 1946, a meeting lasting 10 minutes, Popper gave a list of what he considered to be genuine philosophical problems, including the problem of the validity of moral rules.

Different people attending the seminar had different memories of their impassioned exchange, culminating in Wittgenstein storming out of the room (see Edmonds and Eidnow, 2001, for the full story).

> because of their repressions which were crying out for treatment. A Marxist could not open a newspaper without finding on every page confirming evidence for his interpretation of history (Popper, 1963, p. 45).

The method of looking for verifications is unsound; indeed, it seemed to Popper to be the typical method of pseudo-science. 'All swans are white' is equivalent to 'All non-white things are non-swans'. Thus every time you see a non-swan you *verify* both statements. Verifications can come cheaply if you are interested in just any old verification. By contrast, knowledge will progress most rapidly by the method of criticism, by the method of looking for falsifying instances in order to improve one's theory.

Popper briefly worked for Alfred Adler and describes a related anecdote.

> Once in 1919 I reported to [Adler] a case which did not seem particularly Adlerian, but which he found no difficulty in analyzing in terms of his theory of inferiority feelings, although he had not even seen the child. Slightly shocked, I asked him how could he be so sure. 'Because of my thousand-fold experience' he replied; whereupon I could not help saying, 'And with this new case, I suppose, your experience has become one thousand-and-one-fold' (Popper, 1963, p. 46).

What do these confirmations mean if every conceivable case could be interpreted in the light of Adler's (or Freud's) theory? How would Adler or Freud or Marx ever get any indication that they were wrong? If any behaviour by a person can be 'explained', the theory cannot be criticized by the use of observations; it looses its empirical character. Consider an analyst giving a patient an interpretation. If the patient accepts the interpretation, this analyst may conclude that it was the right one. But if a patient rejects the interpretation, particularly with some vigor, the analyst might conclude that the interpretation hit pretty close to home. What would give the analyst the slightest inkling that she were wrong? And if we cannot learn from our mistakes, how can we improve our theories? Popper's attempt at demarcation between science and non-science was not a mere attempt at classification, but an analysis of how knowledge can best grow.

As an example, Popper (1983, pp. 159–174) discusses Freud's approach to dealing with objections to Freud's theory of dreams and whether it illustrates the critical attitude. For example, consider Freud's central tenant that the content of dreams represents in a disguised way the fulfillments of wishes. An apparent objection to this theory is that some dreams are nightmares. Nightmares do not seem to represent the fulfillment of wishes at all. Does this count as a falsification? Popper has always insisted that the difference between falsifiable and unfalsifiable is not sharp, nor need it be. In terms of the growth of knowledge the point is whether the theory was used to motivate falsifiable predictions which are in turn used to improve the theories. In fact, on Popper's analysis, Freud does not show this critical approach; he attempts to evade rather than use criticism. Freud points out that disguised wish fulfillment *could* occur in the context of an anxiety dream or nightmare, and leaves the matter there. On Popper's analysis, what is important to Freud is possible verifications of the theory. A means for knowledge about the problem of dream meaning to genuinely grow is sadly sidestepped.

Grünbaum (1984; see also 1986) argued that Freudian psychoanalysis is actually falsifiable, contra Popper. Part of the problem is Popper's equivocation on falsifiability as a purely syntactic property (i.e. what matters is the literal form of the statements of the theory) and falsifiability as depending on the attitude of the community of people using the theory (which we discuss below). Grünbaum focused on the fact that clear predictions *can* be derived from the statements of psychoanalysis. For example, therapeutic outcomes should be better for people undergoing psychoanalysis rather than other therapies. (On these grounds, Grünbaum argued that psychoanalysis was both falsifiable and falsified.) Further, if other therapies do remove symptoms, then because they do not address the actual causes, new symptoms should appear (this prediction is also largely falsified). Freudian theory can also make epidemiological predictions: For example, because paranoia is theorized to be due to repressed homosexual desires, an increase in tolerance to homosexuality should lead to a decrease in paranoia in the community (a possible example alluded to by Popper, 1983). But Popper's point still remains: Were such predictions used to test and develop the theory? In general, was every hypothesis formulated such that it could be subjected to severe tests and then subjected to those tests whenever it could be? The ability to generate some predictions does not situate the approach within the critical tradition and it does not imply the falsificationist attitude of its practitioners, an attitude which is essential to rendering the theoretical claims actually falsifiable. [3]

In 1919, Popper went to a lecture by Einstein which impressed him greatly. Einstein said if in a particular situation (viewing a star close to the sun during a solar eclipse) light were not observably bent, his general theory of relativity would be untenable. (The prediction was tested in May 1919 by Eddington and the effect was found.) Einstein, in contrast to either Freud, Adler or Marx, was willing to put his theory on the line by specifying in advance what observations would falsify his theory. Likewise, Popper advised in general that if someone proposes a scientific theory they should answer, as Einstein did, 'Under what conditions would I admit that my theory is untenable?'

A philosopher strongly influenced by Popper was the Hungarian Imre Lakatos (1922–1974). Lakatos reported,

> I used to put this question to Marxists and Freudians: 'Tell me what specific social and historical events would have to occur in order for you to give up your Marxism?'. I remember this was accompanied by either stunned silence or confusion. But I was very pleased with the effect (1999, p. 26).

In fact, Lakatos later considered the question as such too simple and a more sophisticated version was needed; we will consider the subtleties in the next chapter. Nonetheless, the question is always one worth asking yourself for any theory you hold. It is regarded as a key question in experimental psychology; if you do not consider it thoroughly with respect to your own views, someone else certainly will. The more clearly specified the conditions are that would falsify a theory, the more highly such a theory is now regarded in experimental psychology. Whenever you read an explanation someone else has proposed, ask yourself: How could I find out if the theory were wrong?

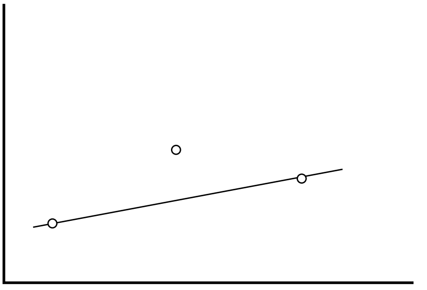
To be clear, Popper believed that the relative non-falsifiability of Freudian and other views was perfectly consistent with them often seeing things correctly. Popper (1963) regarded much of what Freud and Adler said to be of considerable importance and as one day potentially forming the basis of a proper psychological science (see Erdelyi, 1985, for an attempt to relate psychoanalysis to ideas in experimental psychology). In fact, according to Popper, almost all scientific theories originate from something like myths. Starting from myths and metaphysics is perfectly fine, if not unavoidable. Eventually though, we want those myths to generate empirical sciences wherever that is possible. As Popper said, 'Those among us who are unwilling to expose their ideas to the hazard of refutation do not take part in the game of science' (1934/1959, p. 280).

Degrees of falsifiability

A potential falsifier of a theory is any potential observation statement that would contradict the theory (e.g., 'Peter the swan is black' is a falsifier of the hypothesis that 'all swans are white'). One theory is *more falsifiable* than another if the class of potential falsifiers is larger. Popper urges us to prefer theories which are more falsifiable. For example, one needs fewer

3. On my reading of Popper, see section below on Falsifiability. If the purely syntactic reading is taken, Popper contradicts himself regularly and often within the space of a sentence. Hence, I do not believe the common reading captures Popper's meaning.

Figure 1.1



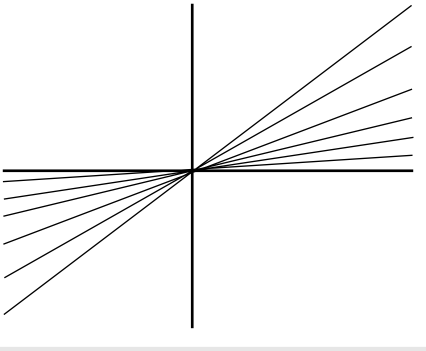
Linear versus quadratic

data points to rule out a straight line relationship than a quadratic. The three observations in Figure 1.1 are sufficient to falsify a proposed linear relation between the two variables, but cannot even in principle falsify a postulated quadratic relation. Put another way, a quadratic function can fit more data patterns than a linear function can; the latter has more falsifiers.

Scientists prefer simple theories. But what is simplicity? Simple theories are better testable. Straight lines are simpler than curves. There is no doubt more to simplicity than this, but perhaps falsifiability captures part of what it is for a theory to be simple.

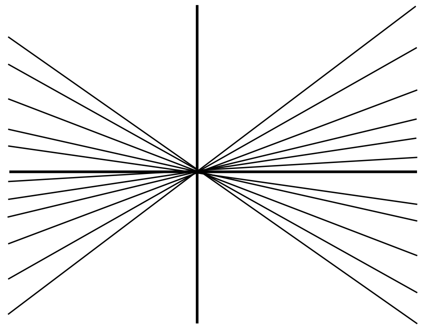
'A is positively correlated with B' allows all the positive regression lines (Figure 1.2) and just rules out all the negative ones; that is, 50% of possible lines are excluded. 'A is correlated with B' rules out practically nothing. All positive and negative regression lines are permitted (Figure 1.3). 'A is positively correlated with B' has more falsifiers than 'A is correlated with B'. The former is more falsifiable and would constitute a better form of theory than the latter. The latter appears to be not falsifiable at all and hence incapable of being a scientific hypothesis according to Popper! This may strike you as odd given scientific psychology is packed full of hypotheses of the form 'A is correlated with B'. In a famous paper, Meehl

Figure 1.2



A is positively correlated with B

Figure 1.3



A is correlated with B

(1967) powerfully criticized much psychology on just these grounds (Meehl's arguments are discussed further in Chapter 3).

Similarly, 'Group A will score differently from Group B' also rules out virtually nothing; it is a very weak theory. 'Group A will score higher than B' is better: It rules out 50% of the possible difference scores. 'Group A will perform 30% better than Group B' rules out a lot! It would be a prediction of a very falsifiable theory.

One style of theorizing in psychology is to predict simply that the score of one group will be higher than that of another group. This appears very weak on Popperian grounds, and indeed it is. But psychological theories can redeem themselves by making predictions for contrasts between a range of conditions and experiments. For example, a theory that predicts simply the score for condition A will be greater than that for B is less falsifiable than a theory that predicts not only that the score for condition A will be greater than that for condition B, but also the score for condition C will be less than that for D, and the score for condition E greater than that of either condition A or B. Further, a theory that makes predictions not only about behaviour but also about brain regions gains in falsifiability; as does a theory that makes predictions not only about learning to read but also about learning social norms. A theory can gain in falsifiability not only by being precise (e.g. predicting a particular numerical difference between conditions, or a difference to within a smaller band of error) but also by being broad in the range of situations to which the theory applies. The greater the universality of a theory the more falsifiable it is, even if the predictions it makes are not very precise. In fact, Popper (e.g. 1983) warned social scientists of trying to ape the physical sciences by being 'precise' when it is uncalled for. He suggested the following moral: 'Never aim at more precision than is required by the problem in hand' (p. 7).

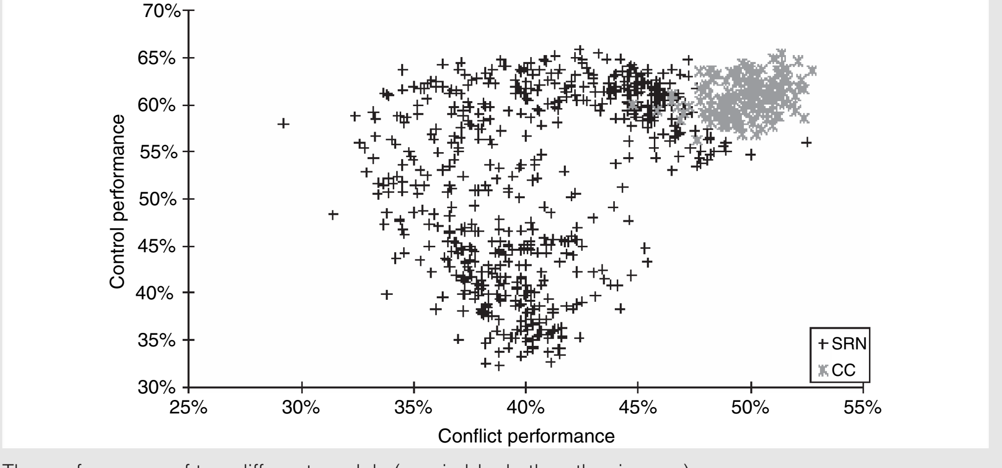
Revisions to a theory may make it more falsifiable by specifying fine-grained causal mechanisms. As long as the steps in a proposed causal pathway are testable, specifying the pathway gives you more falsifiers: There are more components of the theory which, by failing tests, can falsify the theory. But a theory with more verbiage is not for that reason more falsifiable; probably the reverse is true.

Psychologists sometimes theorize and make predictions by constructing computational models. A computational model is a computer simulation of a subject, where the model is exposed to the same stimuli subjects receive and gives actual trial-by-trial responses. For example, neural networks (or 'connectionist models') are simulated collections of artificial neurons connected together in a certain way, which can learn according to specified learning rules (see Plunkett et al., 1998, for an introduction). A computational model has a number of 'free parameters', that is, numbers that have to be fixed, like the number of artificial neurons used, the learning rate determining the size of the change between the connections on each learning trial, and so on. In order for the model to perform, the free parameters have to be set to particular values. But we cannot directly observe the values of these parameters by looking in the brain of the subject. However, one can choose a full range of values for each parameter then measure the performance of the network on each combination of parameter values.

For example, Boucher and Dienes (2003) contrasted two models, trained on the same stimuli as subjects were. (The models differed in terms of the way in which they learned.) In Figure 1.4, the axes are dependent variables in an experiment people participated in; specifically, the performance on two different tests measuring different aspects of what had been learned. Performance on these two tests for each model was determined for the full range of allowable parameter values. Each mark in the graph is the actual performance of a model with a certain combination of parameter values (e.g. with 20 artificial neurons, a learning rate of 0.5, and so on). For one model, its performance for each combination of parameter values is marked with a black cross; different black crosses correspond to different parameter values. Call this the black model. The performance of the other model for each combination of parameter values is marked with a grey x. Call this the grey model. Notice that the models occupy different size regions of the space. The grey model occupies less area than the black model. The grey model is easier to falsify.

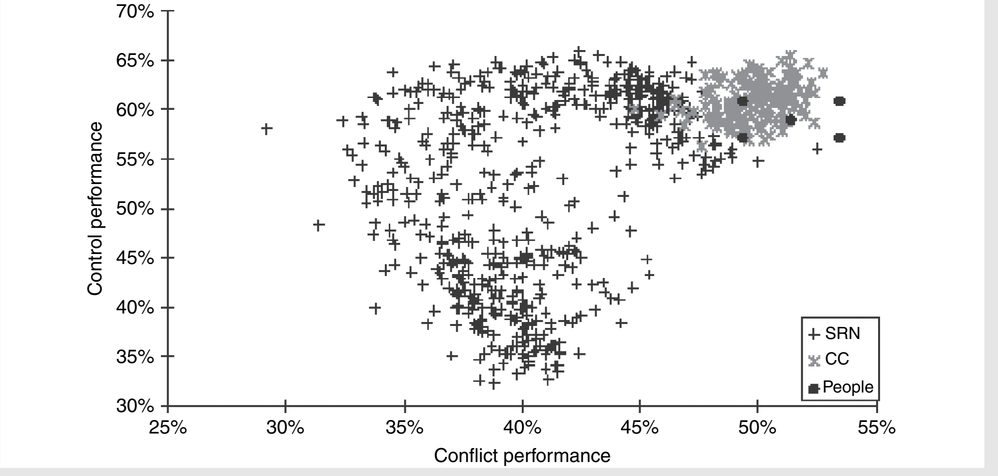
Figure 1.5 shows the performance of people superposed on Figure 1.4. The people were given the same learning phase and the same two tests as the models. The five dots making a square represent people's data, with the central dot indicating the mean of all people on both tests. (The four other dots define limits called the 95% confidence intervals; this is explained in Chapter 3). In fact, the human data fell in the region of space occupied by the

Figure 1.4



The performance of two different models (one in black, the other in grey)

Figure 1.5



Human performance compared to the grey and black models

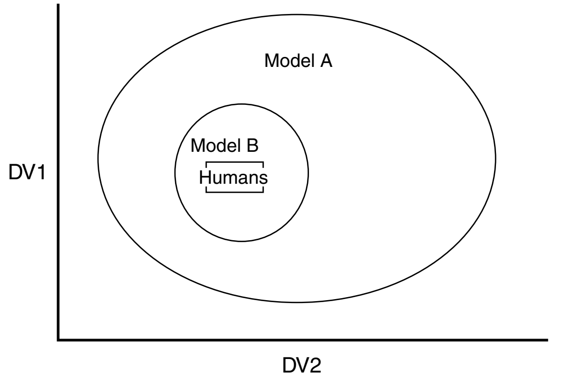
grey model: The grey model was easy to falsify, but it survived this attempt at falsification. The grey model was not proven true but it was corroborated: It proved its mettle.

With computational models it can be difficult to predict how the model will do just by thinking about it. The model has to be run and its behaviour observed and analysed. Let us say two models, A and B, happen to produce data schematically as illustrated in Figure 1.6. The figure also shows where the hypothetical human data fell. Notice the human data does not falsify either model; either model can reproduce the human data, given a suitable combination of parameter values. However, model B is more falsifiable than model A. As the human data is consistent with both models, model B is to be preferred. The point is obvious but often disregarded: Often modellers just try to find any set of parameter values that fits the data (i.e. such that model performance reproduces human performance). A typical procedure – showing a verificationist attitude – would be to report the exact parameter values that allowed model B to fit the human data as closely as possible and the same for model A. If the best-fitting model of each fitted about as well, the modeler may conclude that there is no reason to prefer one model than another. Popper's ideas – as relevant today as they were in 1934 – indicate the inadequacy of simply finding best-fit models. One needs to see what if any observations would rule the model out.

In order to express our preference for more falsifiable theories, Popper would say that even though both models A and B pass the test in Figure 1.6, model B has been more strongly *corroborated*. Model B has passed a more severe test than model A because it was more falsifiable to begin with.[4]

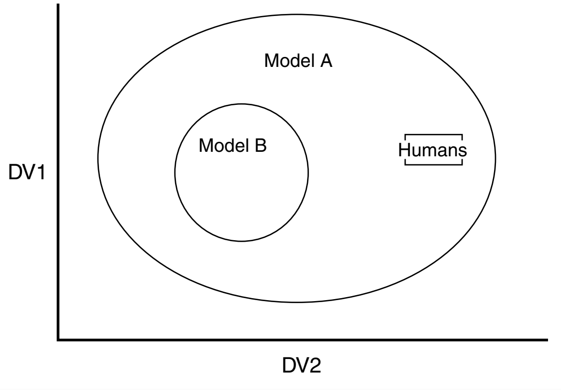
4. The degree of corroboration depends on the severity of the test passed. Popper suggested that test severity could be determined by working out the probability of the evidence obtained given the combination of theory and background knowledge compared to the probability of the evidence given background knowledge alone. In Figure 1.6 the data are more probable given model B than model A: Model A allows anything so any particular data region is not very likely. If the data are very likely given the model and unlikely otherwise, the test is a good one. This notion is very close to the notion of the 'likelihood', which is the topic of Chapters 4 and 5. Likelihood and Bayesian theorists would also prefer model B to model A for the data illustrated in Figure 1.6.

Figure 1.6



Hypothetical comparison between model A and model B

Figure 1.7



Another hypothetical comparison between two models, A and B

Of course, if the data were inconsistent with model B, model B would have to be rejected (as in Figure 1.7). Model A would be tentatively accepted until a better (more simple, more falsifiable) theory could be discovered. Notice that model A in Figure 1.7 does not rule out very much; it does not really *explain* why people behaved as they do. *A theory that allows everything explains nothing*. If model A, much like psychoanalysis in its domain, was consistent with any behaviour, it would give us no explanation at all for why people behaved as they did. In short, *the more a theory forbids, the more it says about the world*. *The 'empirical content' of a theory increases with its degree of falsifiability*. A theory that allows almost anything has very little content. Such phrases of Popper ring in the ear of every experimental psychologist.

The more falsifiable a theory is, the more open it is to criticism. So the more falsifiable our theories are, the faster we can make progress, given progress comes from criticism. We also should prefer more falsifiable theories, according to Popper, because they are bold and more interesting. We are not interested in truths just because they are true or even highly probable. A tautology (i.e. a statement guaranteed to be true like 'a triangle has three sides') or a near tautology ('People cannot remember the words that they were told to forget *because* there is a "forget" label on the word representation blocking recall') is precisely not what we are after in science, even if the statement is true.[5]

Popper (1934) argued that good science shows itself not just by the simple literal form of its theories, but also by the nature of the history of its theories leading to the current proposals. Science aims at the maximum falsifiability it can achieve; hence successive theories should be successively more falsifiable, either in terms of universality or precision. Popper urged the methodological rule: Only revise theories, introduce new theories, or introduce those auxiliary hypotheses that increase the degree of falsifiability of the system. A revision or addition that decreases falsfiability is called 'ad hoc'. For example, one starts with the hypothesis that 'All swans are white'. Then one finds Peter the Swan who is black. One might revise one's hypothesis to 'All swans are white except Peter'. This amendment to the theory is ad hoc, it decreases the theory's falsifiability and so is unsatisfying. For an historical example, consider the theory that 'God created the Earth, with all current species in it, 6000 years ago'. The fossil record provides apparently falsifying evidence. So in the 19th century the clearly ad hoc auxiliary hypothesis was proposed that 'God put the fossil record there to test our faith' (Box 1.6).

Popper proposed that revisions and amendments should always increase falsifiability. Clearly, this is desirable, if possible, but often unrealistic in the short term. For example, simply not diminishing falsifiability is obviously a step forward if the theory is no longer

Box 1.6 The function of female orgasm

Theory of researcher 1: Female orgasm makes the female lie down and perhaps sleep, increasing sperm retention. This leads to greater female fertility.

Theory of researcher 2: Female orgasm causes suction into the uterus from the vagina. Such 'upsuck' increases female fertility.

But a Cosmo survey of new couples found that those women who regularly have intercourse orgasms have only a tiny and non-significant increase in fertility above those women who rarely have orgasms.

(Imaginary results. I don't know what the real figures are.)

Response by Researcher 1:
The effect on sperm retention is small and hard to detect but still real. Also it may be small or non-existent for the young, say for people less than 25 or so.

Response by researcher 2:
Upsuck doesn't increase the number of children but their genetic variety.
When the orgasm occurs seconds before to minutes after the male orgasm, it sucks up sperm leading to a greater chance of fertilization.
If the orgasm occurs more than a minute before male ejaculation, acidic vaginal mucus is sucked up, acting as contraceptive.
Upsuck is used by a woman in a pair bond to make sure her partner sires at least one child, and her illicit lover sires another. (For discussion of real data, see Baker and Bellis, 1994, and Lloyd, 2005.)
A research council only has enough money to fund one of the researchers.
Which one should it give its money to and why?

5. This is one way reason why Popper rejected the Bayesian approach discussed in Chapter 4: Many Bayesians assumed scientists pursue the most probable hypotheses; Popper argued they do exactly the reverse.

actually falsified. Even more strongly, if data rule out, for example, a straight line, surely one can accept a curve. Nature may be simple, but she is only so simple! In any case, it may often be impossible to say whether falsifiability has increased or decreased. But there is a maxim that can always be followed: Make sure that any revision or amendment to theory can be falsified. That way theory development is guaranteed to keep its empirical character. (The amendment 'God put the fossil record there to test us' is not falsifiable at all.) For example, a theory may improve not because of an increase in falsifiability, but because our choice of observation statements is better motivated, for example we use measures with better construct validity. The claim that the new measure has better construct validity should itself be falsifiable and severely tested. Consider a researcher with a good falsificationist attitude, declaring his theory would be falsified if such and such is observed using measure X. Having dutifully falsified and rejected the theory, he could be later criticized for using measure X. Such criticism might not involve an increase in falsifiability, because it just recommends a different measure, but it may represent scientific progress if the claim that the new measure is a better measure that has survived severe tests.

In psychology, attempts to save theories in ways that do not suggest new tests are often called 'post hoc' ('ad hoc' is not typically used in the psychology literature). Whenever you propose a revision or an amendment to your theories to account for your data always ask yourself, 'How could I test this idea?'. If you cannot think of a way of testing your idea in principle, you should think harder about how to explain the data. Of course, one always strives for the simplest most falsifiable revision or amendment one can (Box 1.7).

Box 1.7 Marriage guidance therapy

Therapist A can predict whether or not a couple argues in any 5-minute period of observation based on whether one or both people felt insecure in the preceding 5 minutes. He finds some cases where couples argue but neither rated feeling insecure beforehand. He concludes that his theory is still correct, but these apparently discrepant cases arise when the insecurity is unconscious.

Allowing for unconscious insecurity, he finds all data from a further 20 couples fit his theory: On 80% of the occasions they argue that they state being insecure before the argument, and thus the remaining 20% can be attributed to unconscious insecurity.

Has A established his theory?

Therapist B can predict arguments based on style of conversation just beforehand. He divides conversational style into two types and proposes that one type invariably precedes an argument. He finds a number of cases that do not fit. He devises a new way of categorizing conversational styles: 30 styles, roughly equiprobable across all 20 couples in his sample. He proposes one and only one of the 30 styles always precedes an argument. This new generalization holds true for a further 20 couples.

Has B established his theory?

A's therapy is based on increasing feelings of security. B's therapy is based on changing the conversational style that precedes arguments. If you had to choose between them, which therapy would you choose and why?

Popper's early view of scientific progress as specifically the replacement of one falsified theory by another theory even more falsifiable (but not yet falsified) is just one, somewhat ideal, path that progress could take (see e.g. Feyerabend, 1975, for the argument that often science should *not* proceed in this way). We will consider Popper's later and more flexible view of scientific progress in discussing Verisimilitude below.

Falsifiability

Is it actually possible to distinguish falsifiable from non-falsifiable systems? A problem arises because, according to Popper (1934/1959), observations are always 'theory impregnated'. Thus, falsification is not so simple as pitting theory, on the one hand, against observation, on the other. Theories are needed to determine what an observation is. Even when making the simplest observation, of length or of time, there is the question of whether it is an accurate clock or a rigid rod? One can only refer to our theories to answer this. Consider a measurement of how extroverted a participant is in testing the theory that extroverts are more alert during the evening rather than the morning. We need a theory of extroversion and also of why our scale measures extroversion. Similarly, a measurement of working memory span depends on a theory of working memory. A measurement of how 'anxious' a rat is (in testing a drug for treating human anxiety) depends on a theory relating the rat's behaviour to human anxiety.

Popper (1934/1959) pointed out that a theoretical system can always escape falsification by doubting the observations ('The extroversion scale has limited validity'), or changing a definition ('a non-white swan is not a swan', 'an extrovert who is alert in the morning is no extrovert'). This is possible because observation statements are never given directly from experience. Every statement uses universal names; thus, every statement has the character of a theory. To use an example of Popper's, 'Here is a glass of water' uses the universal terms 'glass' and 'water'. We rely on theories, prejudices and expectations in describing our experience with these terms. Thus, the statement cannot strictly be verified or justified by any observational experience. Experiences, however, clearly can *motivate* observation statements. According to Popper, observation statements are finally accepted only by decision or agreement. Finally, there comes a point where everyone concerned feels the observation statement is sufficiently motivated that no one wishes to deny it. Considerable work may be needed to reach that point; and even then the decision to accept an observation may be overturned by new considerations. For example, in accepting a questionnaire as a good measure of a personality dimension, we might consider whether we accept a statistical tool called 'factor analysis' as a useful tool in isolating a personality dimension, whether past factor analyses have been conducted in an optimal way, whether the scale has been sufficiently tested for its reliability (e.g. stability over time) and whether its validity has been appropriately tested (e.g. whether it has been shown to correlate with other more direct measures of the personality dimension).

In the end we must *decide* which observation statements we will accept. The decision is fallible and amounts to tentatively accepting a low-level empirical hypothesis which describes the effect: For example, accepting an observation statement amounts to accepting a hypothesis that 'Peter is an extrovert', or 'This extrovert was asleep at 7 am' and so on.

Given these considerations, Popper (1934/1959) argued that the question of whether a theoretical system as a set of statements is strictly falsifiable or not is 'misconceived' (p. 82). It is only with regard to the methods applied in examining or testing the theory can we ask whether it is falsifiable or not. That is, the good scientist will consider carefully under what conditions an observation statement will be sufficiently well motivated that it would constitute a falsification of his theory. There should be no attempt to avoid this task. The decision is fallible, so the feedback from nature is noisy, but at least we are exposing ourselves to feedback! We are giving ourselves a chance to learn from our mistakes! Contrast Freud's attitude as described by Popper in the previous section.

Consider also astrology. Astrology can be used to make falsifiable predictions. But astrologers as a community do not seek to test and falsify theories, let alone do this in order to improve both specific and more general theory. Thus, astrology does not constitute a science. It is not part of the critical tradition.

Given you have accepted an actual observation statement, you are logically committed to rejecting any theory that forbade the statement. (You are not logically committed to accepting any general theory as true though; there is still this asymmetry even though basic statements are fallible and falsifications never certain.) The early Popper (i.e. 1934/1959) held that the honest scientist would not reverse this decision; to be honest, scientists must decide to avoid excuses for saving a theory.

Falsifiability: too strong a criterion or too weak?

In this section, we consider two criticisms of Popper's approach. The first is that no theory is falsifiable at all and the second is that all theories are falsified anyway. In a subsequent section, we consider the problem of theories that do not make strictly universal claims true but only probabilistic ones (i.e. like most theories in psychology).

The fact that no system of statements is falsifiable as such has just been considered in the previous section, but Popper's critics often raise the issue. Critics often focus on the fact that accepting an observation statement involves accepting various levels of theory as well as the theory under test. There is no general method of determining which of the theories should be rejected when an apparent falsification occurs. Theory at the most general level (e.g. Newton's law of gravitation; Eysenck's theory that extroversion is related to low cortical arousal) needs to make contact with a particular observational or experimental set-up. Contact is made by auxiliary hypotheses. For example, one might test Eysenck's theory that extroversion is related to low cortical arousal by testing introverts and extroverts with a memory task sensitive to arousal. Already we need at least three theories in addition to the proposal nominally under test: one theory that specifies our measurement of extroversion is actually measuring extroversion; another that specifies that our measurement of memory really is measuring the sort of memory we think it is; and then the auxiliary hypothesis linking such memory to cortical arousal. If the results come out the wrong way round, why not keep Eysenck's theory but reject, say, the theory specifying the relationship between memory performance and arousal? Maybe it was the auxiliary hypothesis that got things the wrong way round? One can only falsify the system of reasoning as a whole, including auxiliary hypotheses, theories of measurement, and also any prior observations used in generating predictions in the current case (e.g. we may need scale norms to determine what counts as a reasonable spread of extroversion scores). Given a falsification, how do we know which component of the system to reject? This widely recognized problem of scientific inference is called the Duhem–Quine problem.

Popper's answer to this problem appears in his earliest work. In order for criticism to occur at all, some part of our knowledge must be accepted for current purposes as unproblematic. Popper calls such knowledge 'background knowledge'. For example, we may take as background knowledge the claim that memory depends on cortical arousal in a certain way. That is, we must make a methodological decision, we must accept at least some of past research in order to make progress. Then we are in a position to test other proposals, for example that extroversion involves low cortical arousal. Isolating the right component of the system of knowledge to reject depends on hunches and critical discussion. Popper suggested that such discussion can involve, for example, the outcome of different tests in which we can vary what is being taken as background knowledge. It is widely acknowledged that such converging evidence with different background assumptions is very important in science, a process Whewell (1840) called consilience. Further, any part of the background knowledge involved in a test may be opened up to critical scrutiny later.

There need not be a general algorithm for how to solve the Duhem–Quine problem; each case can be taken on its own merits, which is precisely what scientists seem to do. In the end, experience motivates us to accept some test statements, and some theories are sufficiently well corroborated that we find ourselves with no reason to doubt them for current purposes. Once we accept some beliefs, we can criticize other beliefs. Popper's methodology does not say how this is done; it only accepts the obvious fact that we do do it. In sum, the Duhem–Quine problem does not obviously threaten the Popperian argument for the importance of the falsificationist attitude to the growth of knowledge. Nonetheless, there remains an interesting and fundamental problem: Can more be said in general about what parts of a system of knowledge people do or should find hard to reject or easy to give up? We will revisit this question in Chapter 2 in discussing the philosophy of Thomas Kuhn and Imre Lakatos.

Kuhn (1962), Lakatos (1970) and Feyerabend (1975) also delivered what is generally taken to be a fatal critique of Popper by pointing out that in the history of science 'all theories have been born falsified' (Lakatos, 1978). Even Newtonian mechanics, all through its history, had applications that did not quite work out. The precise motion of the moon was a major problem for almost 100 years before it was substantially solved (Kuhn, p. 39). The precise motion of the planet mercury was a completely unsolved problem within Newtonian physics. Yet few took these anomalies as reasons to reject the theory. Every falsifiable theory in psychology that has been around for any length of time has no doubt been challenged by recalcitrant data.

At no time did Popper state that any apparent falsification of a theory be accepted uncritically. Just because one should state the grounds on which one would reject a theory, it does not follow one should accept any grounds whatsoever. (This point seems almost universally misunderstood by writers on Popper.) It is no criticism of Popper to point out that scientists do not immediately give up a theory when apparent falsifications are presented to them. However, a genuine criticism of the early Popper is that having laid out the conditions under which one would give up a theory and seen such a set of results, 'we must reject the theory and not work on it again on pain of being irrational', in the striking phrase of Lakatos (1978). Yet there seems no principled reason for why one must always stick to the first analysis that such results would falsify the theory nominally under test. Previously un-thought-of considerations thrown up by the pattern of results or by a colleague or by a random thought may make it more reasonable to keep the general theory and doubt the observations. Indeed, flexibility in deciding what to accept would be most consistent with the overall principle of Popper's philosophy, namely fallibilism and the in-theory openness of all aspects of one's beliefs to criticism. Despite the severe yet aesthetic simplicity of his earlier formulations, Popper did come to have a more flexible approach. The later Popper even argued that we might accept as our current best theory a theory we hold to be false, a point we consider next.

Verisimilitude

Popper argued that given two theories that are apparently false we can still prefer one of them to the other if we think in terms of how closely each approximates to the truth; that is in terms of its *verisimilitude* or *truthlikeness*. It is quite natural for scientists to regard their theories, even their best theories, as mere approximations to the truth with later theories being better approximations than earlier ones. For example, Popper (1963) argued that we should be inclined to say of a theory t2 that it is *closer* to the truth than t1 in some sense if, other things being equal, (a) t2 makes more precise assertions than t1; or (b) t2 explains more facts than t1; or (c) t2 has passed more tests which t1 has failed to pass. Thus, we can prefer t2 to t1 even if there are some facts neither can explain (this may at first pass seem obvious; for some difficult problems, see Popper 1979, pp. 367–374, and Miller, 1994, Chapter 10). Our goal is to move in the direction of increasing verisimilitude which we can do even if we happen to accept all current theories are false, that is not completely true. Intuitively, Einstein's theory is a better approximation to the truth than Newton's, which is a better approximation than Kepler's, and so on. Similarly, Smolensky (1988) has argued that many connectionist models provide a closer approximation to the truth than their corresponding information processing theories.

Popper attempted to produce a more formal definition of verisimilitude but that attempt failed and will not be discussed here. In the end, Popper appeared happy with the common-sense or intuitive notion of closeness to truth (1983, pp. xxxv–xxxvii), though his initial work did spark some decades of intensive technical work by other philosophers on the question (Miller, 1994; Thornton, 2005).

While we can never know whether our theory is actually true, or even what degree of verisimilitude it possesses, the question is whether there is a method able to move us in the direction of increasing verisimilitude? Popper accepts that there is no method that can guarantee this. However, what we can do, and the best we can do, is subject the conjecture that we have moved closer to the truth to critical scrutiny and see if it survives. That is, just as Popper has always recommended, we should aim to construct theories of increasing falsifiability in the long run which can pass more and more severe tests. Whenever we have a theory more falsifiable than another and which has passed more severe tests than the other, then in the light of current discussion this theory is our best guess as the theory which is closest to the truth (even if it has also failed some test). The theory remains a guess of course, but the best current explanation for *why* it has fared so well is its closeness to the truth. And that explanation for why it has fared so well may easily be overturned at any time by a competing theory of the scientific problem that does better. Each successor theory specifies the way in which previous theories were close to the truth. And the best tentative explanation of why each successor theory does so well is *its* closeness to the truth. Of course, consistent with Popper's fallibilism, it follows that the process in no way *guarantees* that science will always actually move closer to the truth.

Objective knowledge

Popper (1972) made an important distinction between two different senses of the word 'knowledge'. Knowledge can be subjective, referring to the mental states of a particular person – what she knows and how strongly she believes it. But there can be knowledge that exists independently of anybody knowing it, that is, objective knowledge. Consider a theory someone has invented; naturally, it starts out life as something (partly) subjectively known by a person. But once invented it takes on a life of its own. The theory has properties that must be *discovered*, just like the properties of any physical object. Indeed, the theory might have properties that are the opposite of what people subjectively believe they are. The theory acquires its own independent reality, it is something people can be right or wrong about. It is real also because it can *affect* physical objects. Because theories do have certain properties, our mental states will change as we come to know those properties; and our mental states can change the world, for example in writing books expressing those theories, or starting a revolution based on the theories. Marx's ideas changed the world, as did Maxwell's equations. Popper used the term 'World 3' to indicate the world of ideas as objective entities, including not just scientific theories but also musical themes, plot developments, methods of arguing and the character of Homer Simpson, to name a few inhabitants. (World 1 is the world of the physical universe *per se* and World 2 is the world of conscious experience.)

Scientific knowledge belongs to World 3. The objective properties of theories include their relation to observation statements (do they contradict or are they consistent?), and the consequences and implications of the theory. The methods used by people in discovering the properties of a theory also have objective properties and belong to World 3 (e.g. the method of vigorously searching for falsifications). It can take some time to discover the properties of one's theory; hence, the later Popper urged scientists to have at least to some degree a 'dogmatic attitude', that is some persistence in sticking with a theory, despite initial problems, because it is only by such persistence one can fully explore the theory and decide whether to treat an apparent falsification as an actual one. Fully understanding whether or not a falsification has occurred may take years. This is a different way of answering the problem (of all theories in practice facing anomalies from the outset), raised by Kuhn, Lakatos and Feyerabend in the previous sections, a way that does not make use of the notion of verisimilitude. Note the dogmatism recommended by Popper still involves taking putative falsifications seriously; there should be a full recognition that they must be resolved.

Some dogmatism is not inconsistent with a critical attitude but necessary for it. Understanding a theory is based on the process of successively trying to criticize it and realizing why the criticisms do or do not work. When one sees why the initially obvious criticism does not work, one understands the theory better (Popper, 1972). Understanding the theory involves a process of conjecture and refutation repeated successively as one sticks with the theory. Current work in theoretical physics in string theory may be seen as trying to understand the theory as an objective entity in its own right as a preliminary to being able to test it against the world (see Greene, 1999 and Woit, 2006, for the Popperian complaint that such work has not yet rendered the theory falsifiable). Indeed, it is a satisfying aspect of a theory that its properties must be discovered. Part of the appeal of connectionist models is that predictions generated by a model cannot be produced by hand waving by the proponent of the model; the predictions have to be discovered by hard work. So some dogmatism is essential. It is when dogmatism is not accompanied by the critical attitude that knowledge will stagnate.

Probabilistic theories and falsification

One apparent problem for Popper is how to treat probabilistic hypotheses, that is almost all hypotheses in psychology. A probabilistic hypothesis does not state that a correlation will be perfect or that a person will always score greater in one condition than another; instead, the claim is that the correlation will be positive but middling, or that scores in one condition are more *likely* to be greater than scores in another condition. For example, we may predict that ginseng will on average improve running speed. But running speed depends on all sorts of factors. I may give ginseng to one person on one occasion and measure their running speed then compare to one other occasion without ginseng. If their speed was greater on the day without ginseng, I have not falsified the hypothesis at all. The hypothesis refers to the mean of a population – the set of ALL occasions we could be interested in measuring. A sample of one observation from the population does not definitely tell us what the mean of the population is. Nor does a sample of five. Or 20. Or a million. How can we ever falsify the hypothesis?

I may have the hypothesis that a coin is fair. I flip it 10 times and get 10 heads. Have I falsified my hypothesis? No, the hypothesis of a fair coin does not rule out a coin landing heads 10 times in a row; in fact, it predicts it will happen sometimes. Similarly, obtaining a million heads in a row does not strictly falsify the hypothesis that the coin is fair. In fact, the hypothesis predicts that sometimes (albeit extremely rarely) a fair coin will produce a million heads in a row. The hypothesis appears to have no falsifiers.

The problem though is no different from falsifying any theory. We have to set up a severe test and make a methodological decision. A test is severe if an outcome has very different probabilities assuming the hypothesis is true rather than false. We can conventionally decide that if the set-up allows an outcome very unlikely given the hypothesis, then the test is severe. Survival of the test then corroborates the hypothesis. Lakatos (1970) presumed that this Popperian analysis became the same as significance or hypothesis testing in statistics (see Chapter 3) as taught in most statistics textbooks for scientists. However, as we shall see in Chapter 3, the typical use of null hypothesis testing in the behavioural sciences has been roundly criticized for failing to be Popperian. The most natural application of Popper's thought to probabilistic hypotheses may in fact be a version of likelihood inference (see Chapter 5 for explanation of likelihood inference; and Taper and Lele, 2004, Chapter 16, for discussion with respect to Popper). For Popper the relative likelihood would not entail that hypotheses are supported in the sense of having increased probabilities (they are not inductively supported) but only in the sense they are corroborated. (Popper himself was comfortable using the term 'support' provided by relative likelihood; see e.g. Section 2 of the Addenda to his 1963/2002.) Do not worry if these points do not make much sense to you now; revisit them after reading Chapters 3, 4 and 5 and decide for yourself what (if any) philosophy of statistical inference would fit in best with Popper's ideas.

Are theories in psychology falsifiable?

Popper often said that the falsifiability of a theory can be discerned from its linguistic or logical form. For example, the statement 'all x are y' is on the face of it falsifiable because finding a single accepted case of an x that was not a y would show the statement wrong. On the other hand, the statement 'some x are y' is not falsifiable, given we do not have access to all the x's. No matter how many xs we find that are not y, 'some x are y' could still be true. But then to what extent are theories in psychology falsifiable? We saw in the last section that many claims in psychology are probabilistic, that is, consist of statistical hypotheses ('The population means for the two groups differ by more than 5 units'). Statistical hypotheses are not of a form like 'all x are y', but we may, as Popper suggests, be able to set up conventions to apply a falsificationist methodology (see Chapters 3 and 5). What is really important is falsificationist attitude, rather than syntactic form.

A statistical hypothesis on its own is an impoverished psychological theory. To be satisfactory, a statistical hypothesis should be strongly motivated by a substantial theory, that is, a unifying idea from which many predictions could be drawn. For example, one might use the idea of cognitive dissonance (a substantial theory) to predict attitude change in a particular context (a specific statistical hypothesis). Often in psychology (and more generally in cognitive science, Boden, 2006, and in the life sciences, e.g. Bechtel, in press) substantial theories take the form of mechanistic explanation, that is by postulating a mechanism by which something is achieved. The specification of a mechanism may consist not of lists of propositions but of analogies or models (cf Giere, 1999). While such representations can be far from the linguistic structures (e.g. ‘all x are y’) the logical positivists started with, and Popper continued his thinking with reference to, one can still apply a falsificationist attitude to such theories. There are consequences of mechanisms working in a certain way, consequences that may show in behaviour, reaction times, brain imaging or lesion studies. Despite what Popper himself has often said, the application of his philosophy, or the spirit of it, does not depend on psychological theories having a certain syntactic structure (cf. Lakatos, 1978). The better one can specify a mechanism such that possible observations can refute it, the more quickly we may learn about the actual mechanism in nature.

Indeed, psychologists rarely state their theories in terms of universals; but perhaps universals are sometimes implicit in how the theory is used. For example, universal statements are not explicitly used in the Boucher and Dienes (2003) paper that we discussed earlier despite the fact that the paper showed how the two models it considered differed in falsifiability. Maybe many theories in psychology could effectively be written in the form, ‘In certain contexts, people *always* use this mechanism’: ‘When my experimental procedure is set up in this way, *all* learning involves this sort of neural network.’ If predictions made by the network are falsified, either the specification of appropriate contexts or the model itself needs revision. That is, the practice of science can be Popperian regardless of explicit syntactic forms.

The strict description of scientific practice described by Popper may be regarded as an ideal in another way. In practice, scientists often do not either categorically accept or reject propositions (of theories or of observational claims). A scientist may hold a theory with a certain degree of conviction and likewise a statement of an experimental finding (and its relation to the theory) with some degree of conviction. He may believe to some degree both the theory and its apparently falsifying evidence. He can do this coherently because he believes neither completely. He may believe both with reasonable conviction yet also believe both are not simultaneously true. Of course, the more he believes the evidence (and the fact *that* it is falsifying) the less he believes the theory, and vice versa. Then, in practice, ‘falsifying’ evidence does not eliminate a theory in one hit; but the accumulation of different evidence may eventually drive conviction in the theory to low levels. Thagard (e.g. 1992) developed a computational model of theory choice in science involving such *continuous* degrees of acceptability as different constraints are satisfied or broken. The Bayesian approach to the philosophy of science can also directly capture these intuitions and is discussed in Chapter 4. Popper strongly rejected the Bayesian approach partly because the convictions of scientists (World 2) were not directly his concern; his concern was the logical relation between theory and evidence (World 3), what happens when scientists through critical discussion eventually do accept and reject relevant propositions. The Bayesians in turn believe that knowing how personal convictions in theories should be continuously altered is solving the logical problem of scientific inference. Lakatos, like Popper, rejected the Bayesian approach, but thought, like Bayesians, that theories were often gradually worn down by evidence and not directly falsified in one hit. We discuss Lakatos’ approach and the Popperian reply in the next chapter.

Sociology of knowledge

Popper believed that the society and tradition in which scientists are embedded was very important. Science relies on not only a first-order tradition of particular myth and theory about the world, but most importantly a second-order tradition of free criticism about those myths, which is a social enterprise. The objectivity of science depends on this social aspect of scientific method: not on the attempt of any individual scientist to be objective but on the friendly hostile cooperation of many scientists (Popper, 1945, p. 241). Thus, Popper thought, the inevitable passion and prejudice of the individual scientist functioned as a *challenge* to other scientists to engage critically. This critical tradition is – or should be – supported by the institutions of science, but can be lost and needs to be actively maintained. Corporate, party political and religious culture is often inimical to the critical tradition, for example, and there is similarly no reason why the institutions of science will always perform their function of supporting it.

Our social traditions also provide us with frameworks for thinking, that is, unexamined assumptions that constrain us. But, as Popper points out, we are never trapped. Simply becoming conscious of a previously unrecognized assumption allows us to criticize it; and that allows us to keep it or to break out into a roomier framework. Popper (1994) regarded discussions between people with different frameworks as not only possible but particularly productive (contra Kuhn, see Chapter 2), even if difficult (and perhaps not so pleasant as discussions between people with the same framework).

Because theories are human inventions, they can show the characteristics of the cultures and societies of the people who proposed them. A theory is simply a stab at the truth or at truthlikeness and, although Popper did not emphasize it, there is no reason on Popper's philosophy (pace Giere, 1999) why there should not be cultural influences on their content. But such influences do not constitute the reason why science may move us towards the truth; they belong to the context of discovery. Nor is science *science* because of power struggles between scientists or between scientists and politicians (though such struggles exist) but because social institutions maintain the tradition of free criticism allowing the objective properties of theories to be understood and criticized. We discuss these issues in more detail in Chapter 2.

Truth, instrumentalism and realism

Even in his earliest writings Popper (1934/1959, p. 278) held that the search for truth is the strongest motive for scientific discovery. We wish to explain the world, and only true explanations explain. If you ask 'Why is the bridge in pieces?' and I say 'because an airplane dropped a bomb on it', I have only explained why the bridge is in pieces if my explanation is true. So it seems obvious we want truth, and truth that is interesting and relevant to us. Remarkably, some philosophers have denied that there is such a thing as truth (we will consider the post-modernists in Chapter 2) or have held that truth may apply to everyday descriptive statements but not to theories. According to one view, instrumentalism, theories are *nothing but* means for making predictions. On this view, theories can be more or less useful, but not, like descriptive statements, more or less true (for a clear up-to-date discussion of the concept of truth, see Lynch, 2005).

Popper (e.g. 1983) argued, in contrast, that theories aim at truth, in the sense of corresponding to the way the world is, and do not aim at just being instruments. If an instrument (like a thermometer) fails a test (e.g. it does not measure temperatures well at high altitudes) we do not reject the instrument; we just use it within its limits of application. But scientists are concerned if a theory fails to pass a test. Likewise scientists are concerned if theories are mutually incompatible (like general relativity and quantum physics), but there is no need to worry about this for instruments, as long as the appropriate domain is used for each. There is no problem with instruments becoming more and more specialized, which is their historical tendency; but scientists like theories that become more and more general, which is, according to Popper, their historical tendency. Importantly, scientists wish to *explain* phenomena, and simply making predictions does not allow explanation nor interpretation (consider a black box that always made the right predictions but we had no idea why: it would not constitute a good scientific theory). Further, given that instrumentalism needs to distinguish statements of theory from everyday observation statements, to the extent that there is no principled distinction between 'pure' observation statements and theoretical statements, instrumentalism founders.

Aiming at truth in Popper's sense means there is a real world about which statements can be true; that is, Popper was a *realist*. A contrasting view is *solipsism*, namely the view that only oneself exists. (Bertrand Russell once said he had received a letter from a lady who was surprised she had not met more solipsists like herself.[6]) Another view is *idealism*, namely the view that only ideas exist, there is no external world beyond ideas. Sometimes people (following Hume) argue against realism because it cannot be justified; how could I ever know that a real world exists? But, as Popper points out, this argument backfires; how could one ever know that idealism or solipsism is true? They are equally incapable of justification.

Further, Popper believed there were positive reasons for believing in the reality of other minds. Popper knew he could not have created Bach's music or Mozart's. (He tried to copy Bach's style once and found he could not.) He was 'even less able, if possible, to draw an average comic strip, or to invent a television advertisement, or to write some of the books on the justification of induction which I am compelled to read' (1983, p. 83). While acknowledging that the argument of his own apparent incapability is inconclusive, Popper was prepared to accept the reality of other minds. And by simple extension to the physical world, he also accepted its existence: he regarded himself as, for example, 'incapable of creating out of my imagination anything as beautiful as the mountains and glaciers of Switzerland' (p. 84). Popper mused perhaps that there are people with megalomania who think otherwise about themselves. However, the issue of realism and anti-realism is still hotly debated. For more discussion on realism in science, see Chalmers (1999, Chapter 15); see van Fraasen (1980) for the anti-realist position; and see Salmon (2005) for a realist position. Greene (1999) is an excellent accessible discussion for the lay reader about the implications of modern physics for what we should take to be real.

Psychologists, despite claims sometimes made to the contrary, also generally believe in the reality of the domain of their subject – of minds, and of brains, thoughts, images, networks, social pressures, social identities, psychological contexts and so on. However, saying what exactly a theory claims is real is an interesting question without a general answer. Sometimes hard work has to go into exploring why a theory or model works well in an attempt to conjecture what in the theory corresponds to reality.

Consider, for example, a connectionist model in which people's learning is being simulated with a neural network with a certain number of simulated neurons. The modeller will often have no commitment to there existing exactly that number of neurons in the person involved in learning the task in question. The modeller is not a realist about that aspect of the model. The modeller may be a realist only about something very abstract like 'the style of computation' used in the model. On the other hand, another neural network modeller may wish to model specific neurons in specific pathways in the brain, and may be a realist about each simulated neuron in the model as such (but not a realist about some of their details).

6. Quoted in Sokal and Bricmont (1998, p. 54)

Both are trying to say something about how the mind, the brain, human learning really is, and that goal is part of what makes them scientists.

The issue of realism crops up in a number of places in cognitive science. Do thoughts really exist? Some say yes (e.g. Searle, 2004) and a few say no (see e.g. Churchland, 1988, Chapter 2). (Popper was a realist about thoughts and conscious experience in general.) Almost all branches of psychology, especially cognitive psychology, postulate that the mind consists of representations. Are such representations conjectured to be real by the theorist or are they just devices for predicting how people will behave? There are realist theories of representation (see Perner and Dienes, forthcoming, for an overview) but also people who say whether a system has a representation is just a question of whether an onlooker wants to interpret the system that way (e.g. Dennett, 1987). Specifying what in the world and in the mind is real is an important task of the science of the mind in general and each theory in particular. In my view, a scientist who gives up the notion of some real world also gives up being a scientist – they cease to have a subject matter (Box 1.8).

Box 1.8 Revisiting Box 1

What is science?

What is the difference between science and pseudo-science?

What is the difference between good science and bad science?

On what grounds should papers submitted to scientific journals be rejected or accepted?

Are Christian Science, Creation Science, Scientology, astrology, traditional Chinese medicine, or chiropractic sciences? Why or why not and why does it matter?

Is psychology a science? Good science or bad science?

How does knowledge grow?

Using Popper's ideas to critically evaluate a psychology article

Ask yourself the following questions as you read any research report. The secret to critical analyses – and hence to being both a good researcher and a good evaluator of research – is to constantly ask questions as you read. Of course this takes effort, though the habit of asking certain questions will with practice become second nature. And that will make all the difference to your ability to evaluate research in a penetrating way rather than uncritically.

First determine if the paper has a clear substantial theory from which follow predictions (i.e. specific statistical hypotheses e.g. predicting a non-zero correlation, difference, etc.). Statistical hypotheses do not in themselves constitute a substantial theory; they follow from one. Make sure you distinguish the two. Further, in order to test the substantial theory, auxiliary hypotheses are necessary. Auxiliary hypotheses allow the substantial theory nominally under test to make contact with data. For example, in terms of the 'two factor theory' of liking in Box 1.5, the researcher may postulate that because the stimuli were new, liking should increase over exposures. That is, an auxiliary hypothesis is that the stimuli are new – and hence people will not get easily bored with them. (In this case, the substantial theory is two-factor theory, and a statistical hypothesis is that 'With this population of materials and subjects, liking will increase from trial one to trial three'.) Are you prepared to accept the auxiliary hypotheses as safe background knowledge? Might you be just as willing to accept different auxiliary hypotheses that lead to opposite predictions? For example, you may be just as willing to assume that the stimuli are simple and also very similar to familiar stimuli – so boredom will be easily induced. In other words, is there a plausible way of deriving opposite predictions from the theory? If so, then the theory is not being put to a strong test. Frequently in papers the predictions given in the introduction could be reversed with different plausible auxiliary hypotheses. (Could you independently test which auxiliary hypothesis to use?) This often happens because researchers see the results before they write the paper; then they can readily convince themselves of what auxiliaries must be true for the theory to make the 'right' predictions. One way to motivate your mind is to imagine opposite results: How would you fit those results with the theory? By being clear about the work done by the auxiliaries, you should then be able to devise conditions where clear predictions really do arise from the theory, because you have established which auxiliaries can be accepted. The auxiliaries should be safe enough that falsification of the predictions can be transmitted back to falsification of the theory under test.

Popper regarded a severe test as being one in which the prediction was unlikely given the rest of your background knowledge. If you did not accept the theory under consideration, what predictions would you make? Think of other well-established theories in psychology. Could they generate the predictions without assuming the theory under test? If so, the test is not a strong test of the theory: The prediction was not unlikely given background knowledge.

How many predictions were derived from the theory? The more predictions the theory makes, the more falsifiable it is. If the theory simply predicts that there will be a difference of any size in a comparison, then it is not really falsifiable at all. In practice, there should be background knowledge to specify what a minimally interesting difference would be. In that case, the test is severe to the extent that the test has sufficient power to detect the minimally interesting difference. The concept of power will be described in Chapter 3 (and see Chapter 5 for another take on test severity). If the test is powerful (in the technical sense to discussed in Chapter 3) then a null result falsifies the prediction of a difference. If the test is not powerful, or if power is not stated, then a null result does not falsify the prediction of a difference. This point is not well understood amongst researchers. Often low power null results are taken as disconfirming a prediction of difference: They do not. Predictions of a difference will only be falsifiable if the statistical test is powerful. Similarly, predictions of no difference will only be strongly corroborated if the test is powerful.

If the predictions come out as expected, the theory survives the tests. Popper would say, the theory is corroborated. He would also be happy for researchers to say the theory has been confirmed – we need not quibble over the exact words used. But on Popper's account the theory is not more probable as a result of this confirmation. Talk of the theory being proved would also not be appropriate.

If the theory failed one or more tests, was it modified in testable ways? Note that modifying a theory or introducing new auxiliary hypotheses in the light of falsifying evidence is perfectly acceptable – indeed, it is how knowledge grows. But the modifications should not be ad hoc.

Finally, think if you have other ways of explaining the results. How would you know if your favourite explanation were wrong?

Final note

Popper's ideas were well received by practising scientists, more so than by philosophers, and in fact his ideas, or versions of them, have become part of the background assumptions of scientists. Consider, for example, the description of science given by the great bongo-playing Nobel laureate Richard Feynman (1918–1988), one of the greatest theoretical physicists of last century:

> The scientist does not try to avoid showing that the rules are wrong; there is progress and excitement in the exact opposite. He tries to prove himself wrong as quickly as possible (p. 16) . . . The more specific a rule is, the more interesting it is (p. 19) . . . In science we are not interested in where an idea comes from. There is no authority that decides what is a good idea. . . . there is no interest in the background of the author of an idea or his motive in expounding it. You listen and if it sounds like a thing worth trying . . . it gets exciting . . . (p. 22) (Feynman, 1998; see also Feynman, 1965 pp. 157–167 for a very Popperian account of how physics developed).

Feynman emphasized the fallibility of all scientific knowledge and the importance of doubt not certainty:

> If we did not doubt we would not get any new ideas . . . The freedom to doubt is an important matter in the sciences . . . it was born of a struggle. It was a struggle to be permitted to doubt, to be unsure . . . progress [is] made possible by such a philosophy, progress which is the fruit of freedom of thought. I feel a responsibility to proclaim the value of this freedom and to teach that doubt is not to be feared, but that it is to be welcomed as the possibility of a new potential for human beings (Feynman, 1998, pp. 27–28).

Review and discussion questions

1. What did the logical positivists believe?
2. Define the key terms of: induction, contexts of discovery and justification, demarcation criterion, falsifiability, ad hoc, auxiliary hypothesis, Duhem–Quine problem, and verisimilitude.
3. Does the existence of difficulties for theories that continue to be held pose a problem for Popper's philosophy of science?
4. Consider an empirical problem of interest to you. What is your favourite explanation for it? Under what conditions would you give up your theory?
5. If you are conducting any research, can you distinguish which of your beliefs relevant and valuable to that research are metaphysical and hence immune from the way data turn out, and which you hold ransom to the data, and hence are scientific properly speaking?
6. Is Darwin's theory of evolution scientific?

Further reading

For an example application of Popper's ideas in psychology, an analysis of the falsifiability of different theories of panic, see Roth et al. (2005). For a brief popular introduction to Popper's ideas, see Magee (1997). Chalmers' (1999) textbook on the philosophy of science, Chapters 5–7, deals with Popper in a clear way. Thornton (2005) gives a good concise summary of Popper's work and its criticisms. For more detail, Schilpp (1974) contains classic criticisms and a reply by Popper. Miller (1994) provides a detailed and robust defense of Popper's thought under the name of critical rationalism. For a discussion illustrating the social importance of a demarcation criterion, see Kitcher (1982). Finally, all of Popper's books are worth reading; his 1994 and 2001 publications are particularly accessible sets of essays.

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