Contents

For Rose and Sam

A Note for Readers

The chapters in this book are all more or less self-contained, so they can be read in any order. Each one finishes with a short guide to further reading for those who wish to find out more about the topics discussed. Most readers can safely ignore this book's many endnotes. They indicate the sources for the facts, arguments and claims mentioned in the main text.

INTRODUCTION

The Wonder of Science

The achievements of the sciences are extraordinary. They have produced explanations for everything from the origins of human culture to the mechanisms of insect navigation, from the formation of black holes to the workings of black markets. They have illuminated our moral judgements and our aesthetic sensibilities. Their gaze has fallen on the Universe's most fundamental constituents and its very first moments. They have witnessed our intimate private activities and our collective public behaviours. Their methods are so compelling that they can command consensus even when dealing with events that are invisible or intangible, in the distant past or the distant future. Because of this, the sciences have alerted us to some of the most pressing problems facing humanity, and the sciences will need to play central roles if these problems are to be solved.

This book – an introduction to the philosophy of science – steps back from the particular achievements of the sciences to ask a series of questions about the broad significance of scientific work. It is a book for anyone with an interest in what we mean by science, and in what science means for us. It does not assume any scientific knowledge, nor does it assume any familiarity with philosophy.

The philosophy of science, like all branches of philosophy, has existed since the time of the ancient Greeks. And like all branches of philosophy, it has a mixed reputation. The charismatic American physicist Richard Feynman, a recipient of the Nobel Prize in Physics in 1965, had little patience for the subject, allegedly remarking that 'philosophy of science is about as useful to scientists as ornithology is to birds'.[1]

Feynman's words – assuming he really said them – were ill-chosen. Ornithology is useless to birds because birds cannot understand it. If a bird could only learn what ornithologists know about how to recognize a cuckoo chick in its brood, then that bird could save itself a lot of misguided effort. Of course, Feynman didn't mean to suggest that philosophy was too complicated for scientists to comprehend; he just didn't see any evidence that philosophy could contribute to scientific work.

There are many good ways to respond to this challenge. One comes from a physicist whose stature is even greater than Feynman's. In 1944, Robert Thornton, freshly qualified with a PhD in the philosophy of science, began teaching modern physics to students at the University of Puerto Rico. He wrote to Albert Einstein for advice. Should he introduce philosophy into his physics course? Einstein wrote back with an unequivocal 'yes'. 'So many people today,' he complained, 'and even professional scientists – seem to me like somebody who has seen thousands of trees but has never seen a forest.' Einstein went on to describe the antidote to this myopia:

> A knowledge of the historic and philosophical background gives that kind of independence from prejudices of his generation from which most scientists are suffering. This independence created by philosophical insight is – in my opinion – the mark of distinction between a mere artisan or specialist and a real seeker after truth.[2]

For Einstein, the value of the philosophy of science, in combination with the history of science, lay in its ability to liberate the investigator's imagination.[3]

We will see in this book that scientists have been admirably ambitious in bringing their methods to some of the most profound topics the world presents us with. Psychologists, evolutionists and neuroscientists have grappled, for example, with the nature of ethics and the reality of free choice. Once they venture down these investigative pathways, it is impossible for them to avoid engagement with philosophy. Scientists cannot make plausible pronouncements about the repercussions of evolutionary theorizing for human morality, they cannot assess the fate of free will in the face of work in neuroscience, unless they have well-formulated views about what morality, or freedom of the will, involves. In other words, whether they like it or not, scientists end up running into just the same conceptual issues that have puzzled philosophers for centuries.

This does not mean that philosophers have nothing to learn when scientists begin to colonize territory that has traditionally belonged to the humanities. On the contrary, recent philosophical work on topics like morality and free will has been greatly enriched by its interactions with the best scientific research on evolution, the mind and social behaviour. In areas like these, philosophy and the sciences have repeatedly come together in constructive ways. They have learned from each other.

We should not suppose that the value of the philosophy of science is fully measured by the degree to which it helps scientists. It also has general cultural significance. The sciences look everywhere, but do they see everything? Will they eventually teach us all that is worth knowing? Or are there alternative forms of understanding that must be arrived at in other ways, perhaps by engaging with works of literature, perhaps by abstract reflection? Philosophical questions like these concern the reach of science, and they help us to understand how the sciences and the arts make different kinds of contributions to human knowledge.

The philosophy of science also has direct political relevance. We cannot understand how governments should respond to threats from climate change without first determining how we should reason when our evidence is uncertain and the stakes are momentous. We cannot decide whether homeopathic treatments should be funded by public health budgets without asking about the markers of genuine science and the markers of pseudo-scientific quackery. We cannot assess how democratic states should make use of technical scientific advice without asking whether apparently neutral pieces of scientific information already come laden with moral and political values.

It turns out, in other words, that the issues addressed by the philosophy of science – the issues we will explore in this book – matter in the most practical ways, for the most important questions of all.

PART ONE

What we Mean by ‘Science’

CHAPTER 1

How Science Works

Science and pseudo-science

There are sciences. Physics is one, chemistry another. There are also disciplines that involve the generation of knowledge and insight, but that few of us would immediately think of as sciences, such as history and literary studies. All this is fairly uncontroversial. But there are cases where we are unsure about what counts as science, and these cases are sometimes politically and culturally explosive.

Consider the trio of economics, intelligent design theory and homeopathy. The only thing that unites these three endeavours is that their scientific status is regularly questioned in ways that provoke stormy debate. Is economics a science? On the one hand, like many sciences, it oozes both mathematics and authority. On the other hand, it is poor at making predictions, and many of its practitioners are surprisingly blasé when it comes to finding out about how real people think and behave.[1] They would rather build models that tell us what would happen, under simplified circumstances, if people were perfectly rational. So perhaps economics is less like science, and more akin to *The Lord of the Rings* with equations: it is a mathematically sophisticated exploration of an invented world not much like our own.

The theory of intelligent design has been promoted by organizations like the prominent US think tank, the Discovery Institute, and developed by theorists including the biochemist Michael Behe and the mathematician/philosopher William Dembski. It aims to compete with the theory of evolution as an account of how species became well adapted to their surroundings. It suggests that some organic traits are too complex to have been produced by natural selection, and that they must instead have been produced by some form of intelligent oversight: perhaps God, perhaps some other intelligent agent. The theory is positioned as a science by its adherents, but many sensible commentators think this is merely an attempt to insert a contentious interpretation of religion into schools, and that understood as a piece of science the theory is hopeless.[2]

Mainstream doctors sometimes value homeopathic remedies, in spite of the fact that their track record of validation by large-scale clinical studies is poor. One camp says that these are quack treatments with no scientific credentials, whose apparent effectiveness derives from nothing more than the placebo effect.[3] Another camp responds by telling us that the dominant method by which scientific investigation establishes the credentials of medical interventions gives us generic wisdom regarding what works in typical circumstances for average patients. This approach, it is said, ignores the need for doctors to prescribe what is right for a unique individual, in idiosyncratic circumstances.[4]

These questions about the markers of proper science are important. They affect the power held by people whose advice can determine our financial and social well-being; they affect what our children are taught at school; they affect what forms of research our tax contributions can be used to fund; and they affect how our doctors advise that we maintain our health. These questions are also old: while today we might be concerned by the scientific status of enterprises like economics, intelligent design and homeopathy, previous thinkers have been troubled by the scientific status of Marxism, psychoanalysis and even evolutionary biology. What we need, it seems, is a clear account of what makes something a science, and what makes something pseudo-science. What we need, it seems, is Karl Popper.

Sir Karl Popper (1902–94)

It is still the case that if you ask a scientist to reflect on the general nature of science, you will probably be referred to the pronouncements of Karl Popper. Popper was born in Vienna in 1902, a time when Viennese cultural life was blessed with an extraordinary richness. He began to attend the University of Vienna in 1918, where he exposed himself to the conspicuous intellectual movements of the moment. He became involved with left-wing politics, he adopted Marxism for a time, he listened to a lecture on relativity theory by Einstein, and he briefly served as a volunteer social worker in one of the psychotherapist Alfred Adler's clinics. In 1928 Popper was awarded a PhD in philosophy, and by 1934 he had published his first book, *Logik der Forschung* (later translated into English as *The Logic of Scientific Discovery*).[5] The broad conception of scientific progress laid out in that book would remain more or less intact in Popper's thinking until his death.

Popper, whose parents were of Jewish origin, was forced to leave Vienna in the 1930s. He moved to New Zealand, to a position at the University of Canterbury in Christchurch, where he spent nearly ten years before moving back to Europe. In 1946 he was offered a post at the London School of Economics, which he held until his retirement. The philosopher of science Donald Gillies, who first met Popper at the LSE in 1966, has recently painted a lively picture of some of Popper's idiosyncrasies:

> Waiting in the lecture hall for Popper to appear was not without some amusement, because a ritual was always performed before the great man entered the door. Two of Popper's research assistants would come into the room before him, open all the windows, and urge the audience on no account to smoke, while writing: NO SMOKING on the blackboard. Popper had indeed a very strong aversion to smoking. He claimed that he had a very severe allergy to tobacco smoke, so that inhaling even a very small quantity would make him seriously ill. When his research assistants had reported back that the zone was smoke-free, Popper would enter the room.[6]

Gillies goes on to explain that when Popper went to a specialist in allergies, the expert was unable to find any evidence of an allergy to tobacco smoke: 'Popper's comment on the result was: "This goes to show how backward medical science still is." '[7]

Perhaps the high point of Popper's reputation came in the late 1960s and early 1970s. He was knighted in 1965, and around this time a string of distinguished scientists described his work in tones of dazzled admiration. Sir Peter Medawar, a Nobel Prize-winner in medicine, said simply: 'I think Popper is incomparably the greatest philosopher of science that has ever been.' Sir Hermann Bondi, mathematician and cosmologist, took the view that 'There is no more to science than its method, and there is no more to its method than Popper has said.'[8]

Some more of Donald Gillies' recollections make it clear that Popper could provoke exasperation, as well as admiration. On Tuesday afternoons, the London School of Economics hosted the 'Popper Seminar', where visiting speakers were invited to present their philosophical views. In a standard academic seminar of this kind, the speaker might talk unmolested for thirty or forty minutes, before the chair invites questions from the audience. At the Popper Seminar, things were different:

> Usually the speaker was allowed to talk for only about 5 to 10 minutes before he was interrupted by Popper. Popper would leap to his feet, saying that he wanted to make a comment, and then talk for 10 to 15 minutes. A typical intervention by Popper would have the following form. He would begin by summarising what the speaker had said so far. Then he would produce an argument against what the speaker had said, and he would usually conclude with a remark like: 'Would you agree then that this is a fatal objection to your position?' As can be imagined, such an attack would often have a very disconcerting effect on the visiting speaker.

Gillies adds that: 'It is easy to see that while, from Popper's point of view, his seminar could be seen as a perfect example of "free criticism", it could have seemed to the speaker very much like a session of the committee on un-Popperian activities.'[9]

'What is wrong with Marxism, psychoanalysis and individual psychology?'

Popper's basic outlook on science derived from two underlying sources of discomfort. He had grown up in a place and a time of intoxicating intellectual excitement. He recalled that, 'after the collapse of the Austrian Empire there had been a revolution in Austria: the air was full of revolutionary slogans and ideas, and new and often wild theories'.[10] Various grand intellectual systems of exceptional ambition – Einstein's relativity theory, Karl Marx's theory of history, diverse psychoanalytic understandings of the mind – were in common currency. And yet Popper felt that there was a deep difference between relativity theory, which he venerated, and (for example) the psychoanalytic theory, of which he was deeply suspicious.

He set himself the task of clarifying his intuition: 'What is wrong', he asked himself, 'with Marxism, psychoanalysis and individual psychology? Why are they so different from physical theories, from Newton's theory, and especially from the theory of relativity?'[11] Popper's view was that while Einstein had proposed a theory that was heroically vulnerable to destruction if experiment should show it false – and yet it had nonetheless enjoyed spectacular experimental successes – the psychoanalytic theory of mind was couched in such non-committal terms that it was immune to experimental refutation. 'I felt', he said, 'that these other theories, though posing as sciences, had in fact more in common with primitive myths than with science; that they resembled astrology rather than astronomy.'[12]

The problem with the predictions of newspaper astrology columns is not that they don't come true: the problem is that they are formulated in such a way that they cannot but come true, and because of that they say nothing of value. My own *Daily Mail* horoscope for the week I write these words tells me that, 'You have faced more downs than ups in recent weeks, but now things are about to change. With both the Sun and Venus, planet of harmony, entering your birth sign this week, you can stop worrying about the past and start planning for the future. This is also the time to bring to the boil something that has been on the back-burner for too long.'[13] How often would we think it sensible to advise someone to 'stop planning for the future and start worrying about the past'? If something has indeed been on the back-burner for 'too long', doesn't that make it trivially true that it is time to address it? And how on earth are we supposed to quantify the relative number of 'ups' and 'downs' we have had over the course of weeks? It is hard to see how we can argue with any of these platitudes.

Similarly, Sigmund Freud recalls how a female patient, whom he described as 'the cleverest of all my dreamers', told him of a dream that seemed to refute his own theory of wish-fulfilment. That theory says that in dreams our wishes come true:

> One day I had been explaining to her that dreams are fulfilments of wishes. Next day she brought me a dream in which she was travelling down with her mother-in-law to the place in the country where they were to spend their holidays together. Now I knew that she had violently rebelled against the idea of spending the summer near her mother-in-law and that a few days earlier she had successfully avoided the propinquity she dreaded by engaging rooms in a far distant resort. And now her dream had undone the solution she had wished for: was not this the sharpest possible contradiction of my theory that in dreams wishes are fulfilled?[14]

This woman dreamed, not of something she wanted to do, but of something she abhorred: a holiday with her mother-in-law. In spite of apparent refutation, Freud argued that his theory was intact: 'the dream showed that I was wrong. *Thus it was her wish that I might be wrong, and her dream showed that wish fulfilled*.'[15] A dream that seems to jar against Freud's theory is explained away with the argument that the woman wanted Freud to be wrong, and the dream allowed this desire to be fulfilled. It is hard not to share Popper's discomfort in the face of examples such as these. Freud's ability to cook up interpretations of the evidence that bring it into line with his theory hardly seems a strength of his psychoanalytic approach; instead, the elastic ability of his theory to stretch around whatever evidence may confront it seems more like a weakness.

The problem of induction

One set of Popper's concerns derived from this urgent sense that we should be able to give a 'criterion of demarcation', which will tell us how to sort science from pseudo-science. The second set of concerns came instead from Popper's deep scepticism of what philosophers call *inductive inference*. The eighteenth-century Scottish philosopher David Hume is usually credited with being the first to pose what we now call 'the problem of induction'. To understand this problem, we first need to understand the nature of *deductive* – as opposed to *inductive* – inference.

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An inductive inference can be defined as any pattern of argument that we regard as reasonable, but which does not claim deductive validity. Our inference about Colin is not deductively valid, and it does not pretend to be. It does not deal in certainty, for clearly it is possible for 10,000 people to have experienced no side effects, and for poor Colin to be the first to react badly. Such circumstances can easily be imagined without contradiction – perhaps Colin has an exceptionally rare genetic mutation – and it is partly because of this that we cannot be sure that Colin will be free from adverse reactions. Even so, we do take it that our evidence, derived from testing thousands of people, makes it reasonable to conclude that Colin is unlikely to suffer adverse reactions. What makes this inductive inference reasonable?

One might try to justify our inference by appealing to further pieces of scientific research. We might point out, for example, that for Colin to react in a way that is different from every one of the 10,000 individuals we tested previously, he would need a very unusual sort of body. We might go on to claim that it is reasonable, although not a certainty, to think that Colin's body is typical, because human conception and development run along well-understood lines. The processes by which human bodies are typically made have been studied in painstaking detail by physiologists and developmental biologists, and this research gives us knowledge about how Colin's body probably works, his likely genetic constitution, and so forth.

This appeal to background scientific knowledge does not solve Hume's problem. It simply reveals the depth of our reliance on inductive inference. Scientists have studied a limited number of embryonic unfoldings in humans, other mammals, and a broad variety of additional species. We assume that the processes that went into the construction of Colin were most likely similar to the processes that have been observed in the laboratory. Our inference about Colin's constitution is based on extrapolation, and Hume's challenge was to explain why this form of extrapolation should be thought reasonable.

The problem of induction can be put forward as a pithy dilemma: we want to know what, if anything, makes it sensible to extrapolate from a limited sample to a broader generalization. We cannot try to answer this by claiming deductive validity for our

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beneficent, human anatomy in general will turn out to be well designed.

What will the astrologer say if everything seems to go fine for me next Tuesday? What will the intelligent design theorist say if an anatomist points out the apparently perverse layout of the male urinary system, which requires the urethra to pass inside the prostate gland, causing misery for men when the prostate becomes enlarged and the urethra becomes constricted? If we want to use a Popperian criterion to determine the scientific status of theories, we need to focus on how the theorists responsible for them handle failed predictions. Unfortunately, there doesn't seem to be any clear recipe that will tell us what sort of response is 'scientific', and what sort of response is 'unscientific'.

We do not want to say that a theory is scientific only if the theorists who put it forward are prepared to reject it the moment its predictions appear to be contradicted by experiment. It is perfectly reasonable for a theorist to dig in and say that, while the experiment might seem to be bad news for the theory, she believes fault to lie with the experimental set-up itself. That is exactly how the scientific elite responded to the apparent demonstration of faster-than-light neutrinos at Gran Sasso. But if particle physicists are allowed to evade refutation by suggesting that the blame for a failed prediction does not lie with their theories, but lies instead with other factors external to those theories, then what is to stop the astrologist, or the intelligent design theorist, from pointing the finger at something other than the view that our lives are influenced by the stars, or something other than the view that organic traits are the products of conscious design, when I fail to have an accident on a Tuesday, or when my prostate swells to constrict my urethra? Cannot they, too, offload the blame for failed prediction on an error of calculation, or a hidden assumption, or a misunderstanding of the theory itself? What, precisely, is the difference between an intelligent design theorist telling us that we cannot fathom God's peculiar intentions for my urinary anatomy and a physicist's insistence that the apparatus at Gran Sasso must have been malfunctioning in some as-yet-undetermined way? Don't all of these theorists use similar tactics to preserve their theories from refutation?

we have discovered many 'missing links', each of which adds further support to Darwin's view of common descent. But how are we to apply this sort of criterion *prospectively*, if what we want to do is sort the scientific wheat from the pseudo-scientific chaff right now?

'The inquiring mindset'

Popper is of little help if we want a practical, prospective criterion of demarcation. In spite of everything that we read about the importance of the 'scientific method', it remains unclear what that method is. The basic mathematical tools of statistical inference form a fairly constant part of the scientist's toolkit. There are also, of course, plenty of scientific *methods*: there are techniques of observation and analysis specific to individual sciences. We can use randomized controlled trials for understanding the efficacy of medicines; we can use X-ray crystallography for understanding the structure of molecules. But when we try to pinpoint some recipe for inquiry that all successful sciences have in common, we run into trouble.

Yet another Nobel laureate, Sir Harry Kroto, suggested in the *Guardian* a few years ago that we may have to settle for a loose account: 'The scientific method is based on what I prefer to call the inquiring mindset.'[27] The scientist approaches nature in a spirit of curiosity, she asks honest questions of nature. She proposes a hypothesis, and seeks out evidence, often through a well-designed experiment, that will adjudicate on the truth of that hypothesis. But while this does indeed help us to explain what makes science an admirable activity, it does not isolate a method that distinguishes the sciences from other branches of inquiry. Historians, too, can propose bold hypotheses, before delving into an historical archive in the spirit of honest inquiry. The same goes for other researchers in the humanities.

Kroto added to his very capacious remark on 'the inquiring mindset' that this favoured attitude 'includes all areas of human thoughtful activity that categorically eschew "belief", the enemy of rationality. This mindset is a nebulous mixture of doubt, questioning, observation, experiment and, above all, curiosity, which small children possess in spades.'[28] Kroto is right, of course, to stress that the sciences, as traditionally understood, do not have

— DAVID C. STOVE,
Four Modern Irrationalists (Oxford: Pergamon, 1982).

Meanwhile, a far more sympathetic assessment of Popper's work comes in:

— DAVID MILLER,
Critical Rationalism: A Restatement and Defence (Chicago: Open Court, 1994).

For a sophisticated form of Popperianism that aims to bring Popper's basic views into alignment with the history of science:

— IMRE LAKATOS,
The Methodology of Scientific Research Programmes (Cambridge: Cambridge University Press, 1980).

23. T. Kuhn, 'The Road since Structure' in *The Road since Structure: Philosophical Essays 1970–1993* (Chicago, IL: University of Chicago Press, 2000), p. 104.
24. C. Darwin, *On the Origin of Species* (London: John Murray, 1859).
25. See the 'Historical Sketch', which Darwin himself added to later editions of the *Origin*, and which acknowledges some of these precursors.
26. J. Secord, *Victorian Sensation* (Chicago, IL: University of Chicago Press, 2001).
27. P. Bowler, *The Eclipse of Darwinism* (Baltimore, MD: Johns Hopkins University Press, 1983).
28. H. Fleeming Jenkin, 'Review of The Origin of Species', *North British Review*, 46 (1867): 277–318.
29. T. Lewens, 'Natural Selection Then and Now', *Biological Reviews*, 85 (2010): 829–35.
30. R. A. Fisher, *The Genetical Theory of Natural Selection* (Oxford: Clarendon, 1930).

CHAPTER 4: BUT IS IT TRUE?

1. For an exploration of a similar position from a Jewish perspective, see P. Lipton, 'Science and Religion: The Immersion Solution' in J. Cornwell and M. McGhee (eds), *Philosophers and God: At the Frontiers of Faith and Reason* (London: Continuum, 2009), pp. 31–46.
2. Important papers on underdetermination include L. Laudan, 'Demystifying Underdetermination' in C. W. Savage (ed.), *Scientific Theories*, Minnesota Studies in the Philosophy of Science, 14 (Minneapolis, MN: University of Minnesota, 1990), pp. 267–97; L. Laudan and J. Leplin, 'Empirical Equivalence and Underdetermination', *Journal of Philosophy*, 88 (1991): 449–72.
3. C. Clark, *The Sleepwalkers: How Europe Went to War in 1914* (London: Penguin, 2013), pp. 47–8.
4. A full transcript of Boas's unpublished lecture is attached as an online appendix to H. Lewis, 'Boas, Darwin, Science, and Anthropology', *Current Anthropology*, 42 (2001). I am grateful to Jim Moore for first drawing my attention to it.
5. Originally published as P. Duhem, *La Théorie Physique. Son Objet, Sa Structure* (Paris: Chevalier and Rivière, 1906).
6. P. Duhem, *The Aim and Structure of Physical Theory* (Princeton, NJ: Princeton University Press, 1954 [1914]).
7. H.-J. Shin et al., 'State-Selective Dissociation of a Single Water Molecule on an Ultrathin MgO Film', *Nature Materials*, 9 (2010): 442–7.
8. The preceding exposition is entirely based on H. Chang, *Is Water H_2O?* (Dordrecht: Springer, 2012).
9. A. Kukla, 'Does Every Theory Have Empirically Equivalent Rivals?', *Erkenntnis*, 44 (1996): 145.
10. Here I am influenced by K. Stanford, 'Refusing the Devil's Bargain: What Kind of Underdetermination Should We Take Seriously?', *Philosophy of Science*, 68 [Proceedings] (2001): S1–S12.