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Philosophy and the Sciences for Everyone

ROUTLEDGE

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Preface

For centuries, philosophy and the sciences have gone hand in hand. Throughout the seventeenth and the eighteenth centuries, ‘natural philosophy’ provided the blueprint for modern physics, chemistry, astronomy, no less than botany and medicine. Newton, for example, called his masterpiece *Mathematical Principles of Natural Philosophy,* and philosophical reflections about the nature of space and time played a central role in Newton’s physics. In the eighteenth century, Kant’s philosophical speculations about the origins of the universe led him to the nebular hypothesis, later developed by Laplace as one of the first attempts at a modern scientific explanation in cosmology. But what good is philosophy for the sciences? And what can contemporary philosophers learn from the sciences? While still at the beginning of the twentieth century, philosophy had a profound influence on the discoveries of Einstein, Bohr, and other pioneers of the time, it might seem that the dialogue between philosophy and the sciences has come to an end. After all, we live in an era where scientific research is so specialized, diversified, and run on such a large scale that – the sceptics argue – there is very little that philosophy can contribute to contemporary science.

This book is our modest attempt at proving the sceptics wrong. The dialogue between philosophy and the sciences has never been richer, more pervasive, and timely. What is the origin of our universe? What are dark matter and dark energy, and what reasons do we have for believing in their existence? Is the universe such as to allow life to evolve? Has the human mind evolved as a series of ‘mini-computers’, adapted to solve problems that our ancestors faced? And to what extent, is our mind – in its functioning – like a computer? What makes us conscious human beings? And what role do technology and environment play in understanding how our minds work? These are central, timely, and cutting-edge questions in contemporary science that will occupy us in the course of this book – as a journey from the cosmos, to consciousness, and computers.

Of course, this selection of topics is not intended to be a comprehensive or exhaustive introduction to the many ways in which philosophy and the sciences (broadly construed) are still engaged in a mutually beneficial dialogue (we would need a much bigger book for that). Instead, we selectively focus on some timely topics in the philosophy of the physical sciences, and the philosophy of cognitive sciences, with the hope of probing each of them a little bit deeper than a whistle-stop tour through the sciences, broadly construed, would have allowed.

Hence the book is structured in two parts. In the first part we focus on three key issues in contemporary philosophy of cosmology. We start in Chapter 1 with a general introduction to philosophy of science. We take you through the famous relativist debate about Galileo and Cardinal Bellarmine. You will learn about what makes scientific knowledge 'special' compared with other kinds of knowledge, the importance of demarcating science from non-science, and how philosophers such as Popper, Duhem, Quine, and Kuhn came to answer these questions. In Chapters 2, 3, and 4 we turn to a particular branch of philosophy of science, called philosophy of cosmology. This is a burgeoning field at the key juncture of philosophy of science and cutting-edge research in cosmology. Chapter 2 is dedicated to the origins of our universe and provides a general overview of the history of cosmology and of the philosophical problems (laws, uniqueness, observability) that stood in the way of cosmology becoming a science in its own right (from being a branch of metaphysics, back in the eighteenth century). In Chapter 3, we discuss the current cosmological model, which talks about dark matter and dark energy: we ask what dark energy and dark matter are, what the evidence for them is, and which rival theories are currently available. This will provide us with an opportunity to explore a well-known philosophical problem known as underdetermination of theory by evidence. Next, in Chapter 4, we ask the question of why our universe seems to be such as to allow life to have evolved, according to the anthropic principle. We clarify what the anthropic principle says, and how these philosophical reflections may or may not find a counterpart in inflationary cosmology and the hypothesis of a multiverse.

After these chapters on philosophy of cosmology, we turn our attention to philosophy of cognitive sciences. This is a thriving area where philosophers of mind, cognitive scientists, psychologists, and linguists are joining forces to provide a better grasp of how the human mind has evolved and how it functions. We start in Chapter 5, with a fascinating journey through evolutionary psychology and the debate about nativism: we look at examples coming from ecology, such as beavers' colonies, to understand how the human mind might have adapted to solve specific tasks that our ancestors faced. In Chapter 6, we zoom in on the actual functioning of the human mind as a computer able to perform computations, and we look at the scientific ideas behind the mind–computer analogy. Chapter 7 takes us through cutting-edge research in psychology on the nature of consciousness, and pressing issues such as the role of consciousness in the vegetative state and other syndromes. Finally, in Chapter 8, we review the state of the art in the blossoming area of 'embodied cognition'. This is a recent trend that has brought to the general attention the importance of going beyond the 'neurocentric' view of how our mind works, and re-evaluating the central role of technology and environment in developing our cognitive capacities.

This book has been written for anyone who may be interested in learning about philosophy of science, not from the point of view of the history of the subject and its internal debates (there are plenty of excellent introductions already available on this). Instead, this book gives you an introduction to philosophy of science by exploring cutting-edge debates between philosophers and scientists on timely topics, such as dark matter and dark energy, mind and machines, consciousness, and evolutionary psychology. As such, we aim to offer an accessible, non-technical introduction to each topic, without presupposing too much background knowledge in either philosophy or science. Each chapter has a summary, list of study questions, further readings (both introductory and advanced) as well as internet resources. Key terms are emphasized in bold and defined in the glossary at the end of the book.

In the spirit of fostering dialogue between philosophy and the sciences, each chapter has been jointly written by a philosopher and a scientist. The process of jointly writing each of these chapters has been a rewarding journey for all of us, and I'd like to thank all the contributors for seeing this journey through: David Carmel (Psychology), Andy Clark (Philosophy of Cognitive Sciences), Jane Suilin Lavelle (Philosophy), John Peacock (Physics and Astronomy), Alasdair Richmond (Philosophy), Peggy Seriès (Informatics), Kenny Smith (Linguistics), and Mark Sprevak (Philosophy). Special thanks to James Collin for precious help with the copy-editing of the volume. I hope you will find the journey through the book as rewarding as we did!

This book is born out of a free and open-source MOOC ('massive open online course'), called 'Philosophy and the Sciences' and offered through the University of Edinburgh, following the success of our first MOOC 'Introduction to Philosophy' and associated book *Philosophy for Everyone*. The MOOC 'Philosophy and the Sciences' is to be launched in October 2014, with eight-week video lectures, forum discussions, and online self- and peer-assessment. I'd like to thank the University of Edinburgh for the institutional support in making possible this cross-College interdisciplinary collaboration. I want to thank especially the Principal, Professor Sir Timothy O'Shea, for enthusiastically supporting the project from the start; the Vice-Principal, Jeff Hayward; the Head of the College of Humanities and Social Science, Dorothy Miell; the Head of the College of Science and Engineering, Lesley Yellowlees; the Head of the School of Philosophy, Psychology, and Language Sciences, Andy McKinlay, and the School Administrator, Debbie Moodie; and the whole MOOCs Vice-Principal's Office, with Amy Woodgate, Lucy Kendra, Scott Imogen, and Nicol Craig for kindly assisting us every step of the way in the MOOC. The course will have several iterations, so if you have come to this book in some non-MOOC-related way, you may be interested in enrolling in our MOOC. A very warm welcome to everyone from the team of Philosophy and the Sciences!

Michela Massimi

1 What is this thing called science?

A very brief philosophical overview

Michela Massimi and Duncan Pritchard

What is science? Evidence, knowledge claims, and their justification

Scientific inquiry is widely considered to be a paradigmatic way of acquiring knowledge about the world around us. But what is science? And what makes scientific knowledge 'special', compared with other kinds of knowledge (see Achinstein 2010; Chalmers 1999; Goldacre 2009)? Here is one possible answer to this question: science just is what people who are professional scientists (e.g. in university science departments, or in the scientific research wings of large corporations, and so on) do. So, for example, astrology, which is not practised by professional scientists (but by e.g. newspaper columnists), is not a science, whereas astronomy, which is practised by professional scientists, is. A moment's reflection should reveal that this isn't a particularly helpful account of what science is.

For example, couldn't someone undertake a scientific inquiry and yet be an amateur, and so not be part of any professional scientific community? Moreover, do all the inquiries undertaken by professional scientists as part of their work count as scientific inquiries? Note that even the contrast between astronomers and astrologists isn't all that helpful in this regard once we inspect it more closely. There are *professional* astrologers after all, and such people may be regarded by themselves and those around them (e.g. their clients) as bona fide scientists. We clearly need to dig a little deeper.

In order to bring our question into sharper relief, consider the well-known Bellarmine–Galileo controversy about the validity of Ptolemy's geocentric system vs Copernicus's heliocentric system. This historical episode is well documented, and it has been the battleground of important discussions about what epistemologists call *epistemic relativism*, namely the view that norms of reasoning and justification for our knowledge claims seem to be relative (see Rorty 1979; Boghossian 2006). Epistemic relativism contends that while there might well be facts of the matter about whether or not our planetary system is indeed heliocentric, it does not follow that heliocentrism is the most rational view to believe. The epistemic relativist would contend that to assess the disagreement between Galileo and Bellarmine on whether or not the Earth moves, one would need to assess the epistemic standards and norms at work in assessing such claims. But the problem is that Galileo and Bellarmine used two seemingly incompatible norms or epistemic principles to evaluate the truth of their respective claims. While Galileo relied on the observational evidence coming from his telescope, Cardinal Bellarmine relied on the testimony of the Bible. In other words, Galileo and Bellarmine appealed to two different epistemic principles for the justification of their respective beliefs (Boghossian 2006, chs 5 and 6). Galileo appealed to the epistemic principle, which might be called *observation* whereas Bellarmine resorted to the epistemic principle of *revelation*. The former roughly says that given the telescopic evidence that Galileo had available at his time, if it seemed to Galileo that the Earth moved around the sun, then Galileo was justified in believing that the Earth moved around the sun. The latter principle, by contrast, says that given the testimony of the Bible as the revealed word of God, if it seemed to Bellarmine that the Earth was at rest in the centre of the universe, then Bellarmine was justified in believing that the Earth was at rest in the centre of the universe.

The epistemic relativist relies here on a powerful argument, known as the 'no neutral ground' argument (see Siegel 2011). The 'no neutral ground' argument claims that there was no common ground or neutral standard that Galileo and Bellarmine shared at the time and which could be used unambiguously to discern who was right and who was wrong. More precisely, to ascertain whether one of them was in fact wrong, one would need to offer reasons and arguments for proving that the epistemic principle of observation is in fact superior to Bellarmine's epistemic principle of revelation. Can such reasons and arguments be found?

A Galilean supporter may easily invoke here the reliability of the telescope and telescopic evidence in justifying Galileo's beliefs. The telescope was a scientific instrument that could be deployed to test the Copernican hypothesis and confront it directly with *observational* evidence. The evidence from the Bible for the geocentric hypothesis was of an altogether different kind: it was *textual* evidence, based on the authority of the Bible as the revealed word of God. So it may seem that the superiority of observational evidence over textual evidence can speak in favour of the superiority of Galileo's observation over Bellarmine's epistemic principle of revelation.

Not so fast. For one thing, it is not immediately obvious why *textual* evidence should be per se inferior to *observational* evidence. Think of the human sciences, and disciplines such as archaeology or anthropology where textual evidence (or oral evidence by members of a community) is routinely used to justify claims that we believe to be correct about the past, or about cultural practices. For sure, there are contexts in which *textual* evidence is the primary kind of evidence available to justify knowledge claims (in archaeology or anthropology) that we are inclined to think of as valid and scientific. But there is more. Back at the time of Galileo, the observational evidence delivered by the telescope was itself the object of acrimonious controversy. Not everyone at the time believed that the telescope was reliable or that telescopic evidence should have the upper hand over textual evidence from the Bible. Indeed, the scientific status of telescopic evidence was as much at stake in this debate as was the belief in heliocentrism.

To start, Galileo did not have a full-blown optical theory to explain how his telescope worked or whether it was reliable, although he did have a *causal explanation* about how the lens of the telescope worked in making the celestial objects appear more similar to the way they are in nature. However, Galileo's opponents endorsed the opposite causal explanation about the working of the telescope, whose lens – they thought – magnified and distorted the actual size of celestial objects. Galileo's foes, from Christopher Clavius to Lodovico delle Colombe and Cesare Cremonini, objected to the reliability of the telescope on the ground that it did not seem to magnify the stars, by contrast with other celestial objects: the size of the stars appeared to be the same to the naked eye and to the telescope. At stake in this debate was the issue of whether or not the halos of the stars visible to the naked eye should be taken or not taken into account in the estimate of their actual size: Aristotelians such as Horatio Grassi thought that it should, while Galileo thought that it should not, because it was illusory. The debate was sparked when Grassi (under the pseudonym of Lothario Sarsi) published this objection in his 1619 *Libra astronomica,* which Galileo rebutted in *The Assayer*. The final verdict went to Galileo because the scientific community eventually embraced Galileo's causal explanation of how the telescope worked and why it was reliable. To use Rorty's expression, we all stand on the grid that Galileo established with his victory.

This historical example illustrates the epistemic relativist's 'no neutral ground' argument, and the difficulty of identifying a common ground or a common measure to assess and evaluate knowledge claims in their historical context. But there is a further, stronger argument that relativists can use against the claim of universally valid norms of reasoning in science. This is called the 'perspectival argument' (see Siegel 2011), and it says that given the contextual and historically situated nature of our scientific knowledge, it follows that what we can know (what is both true and justified to believe) depends inevitably on the perspective of the agent. Leaving aside for now the issue of how we should think of or define a perspective – either in terms of the system of beliefs endorsed by the agent (see Sosa 1991); or in terms of the hierarchy of scientific models defining a scientific perspective (see Giere 2006) – the important issue for our discussion here is that if the perspectival argument is correct, then our knowledge claims are bound or determined by the perspective of the agent so that again there are no universal norms or standards to evaluate those knowledge claims *across different perspectives.*

We cannot enter here into the details of the debate surrounding epistemic relativism and its far-reaching implications for science and scientific knowledge (see Kusch 2002). Instead, we simply want to draw attention to what is at stake in this debate, namely the notions of truth and scientific progress. If the epistemic relativist is correct in the 'no neutral ground' argument and the 'perspectival argument', one may legitimately conclude that scientific inquiry should not be regarded as an endeavour to gain an increasingly better, and more likely to be true knowledge of the universe we live in. For there are no universal norms of reasoning or standards to which we can appeal in evaluating knowledge claims and we are trapped into the strictures of our respective perspectives. If this is indeed the case, how can science progress? And is there any goal at all at the end of scientific inquiry? Truth, as correspondence with the way things are in nature, would be a natural candidate goal for scientific inquiry: we expect our scientific theories to improve on their predecessors, to show why their predecessors were successful to the extent that they were, and to extend the range of phenomena that could be explained and predicted over and above those of their predecessors. In other words, if truth is the ideal goal of scientific inquiry, we could take the history of science as a progressive sequence of scientific theories, which were more and more likely to be true, while also fallible and revisable. The view of science that takes truth as the final aim of scientific theories is known as **scientific realism** and it goes right against epistemic relativism in claiming that there must be universal norms of reasoning and standards through which we can assess knowledge claims and discern scientific ones (e.g. Galileo's belief in heliocentrism) from pseudo-scientific ones (such as Bellarmine's belief in geocentrism). In the rest of this chapter we will look at a prominent attempt to identify a universal scientific method able to discern science from pseudo-science, in Karl Popper's view. Next, we will look at how some of the aforementioned relativist intuitions found their way again into the debate on the scientific method (or lack thereof) in the works of Duhem, Quine, and Kuhn.

From inductivism to Popper's falsification

Philosophers of science are interested in understanding the nature of scientific knowledge and its distinctive features, compared with other forms of knowledge (say, knowledge by testimony). For a very long time, they strove to find what they thought might be the distinctive method of science, the method that would allow scientists to make informed decisions about what counts as a scientific theory. The importance of demarcating good science from pseudo-science is neither otiose nor a mere philosophical exercise. It is at the very heart of science policy, when decisions are taken at the governmental level about how to spend taxpayers' money.

Karl Popper was, undoubtedly, one of the most influential philosophers of the early twentieth century to have contributed to the debate about demarcating good science from pseudo-science. In this section we very briefly review some of his seminal ideas, especially since such ideas will prove important for understanding methodological discussions about cosmology in the next two chapters.

Popper's battleground was the social sciences (Ladyman 2002; Thornton 2013). At the beginning of the twentieth century, in the German-speaking world, a lively debate took place between the so-called *Naturwissenschaften* (the natural sciences, including mathematics, physics, and chemistry) and the

Geisteswissenschaften (the human sciences, including psychology and the emergent psychoanalysis), and whether the latter could rise to the status of proper sciences on a par with the natural sciences. This is the historical context in which Popper began his philosophical reflections in the 1920s. Popper's reflections were influenced by the Vienna Circle, a group of young intellectuals including Philipp Frank for physics, Hans Hahn for mathematics, Otto Neurath for economics, and the philosophers Moritz Schlick (who joined the group in 1922) and Rudolf Carnap (who joined in 1926). The philosophical view adopted by the Vienna Circle is known as logical empiricism: knowledge comes in two kinds; the first kind is knowledge of logical truths (truths independent of experience); the second is empirical knowledge, whose truths are based on experience (see Gillies 1993). Popper's influential book *The Logic of Scientific Discovery* was first published in 1934 (the English translation came much later, in 1959) in the Vienna Circle series edited by Schlick; and it dealt precisely with the problem of how to demarcate good science from pseudo-science. Before Popper, the received view about scientific knowledge and the method of science was **inductivism**: on this view, scientific theories are confirmed by inductive inferences from an increasing number of positive instances to a universally valid conclusion. For example, Newton's second law seems confirmed by many positive instances from the pendulum, to harmonic oscillators and free fall, among others. We can think of scientific theories as sets of sentences, i.e. laws of nature; and laws of nature, as taking the form of true universal generalizations, 'For all objects x, if Fx then Gx' (e.g. Newton's second law would read as follows: if an external force acts on a body of mass m, then the body will accelerate). And we can think of true universal generalizations as being confirmed when a sufficiently large number of positive instances (and no negative instances) have been found for them. Inductivism was at work in the logical empiricists' criterion of **verification**: namely the idea that any claim or statement is scientific if there is a way of *empirically verifying* it (i.e. if there is a way of finding positive empirical instances confirming that claim or statement).

The problem with inductive methodology – according to Popper – is that it is too liberal as a method for demarcating good science from pseudo-science. Political theories such as Marxism or Freud's psychoanalysis would equally meet the requirements of inductivism. A Freudian psychoanalyst could appeal to plenty of positive instances of people's dreams that can confirm the validity of Freud's analysis of the Oedipus complex, for example. But is this per se sufficient to license the scientific status of Freud's psychoanalysis? People that read horoscopes can similarly claim that there are positive instances in their monthly working schedule confirming the horoscope's warning that it is going to be a very demanding month for Aquarians! Does it mean that horoscopes are scientific? Positive instances are where one wants to find them. Thus, to demarcate good science from pseudo-science, Popper thought, we need to probe a little deeper.

The problem – as Popper saw it – is that theories such as psychoanalysis do not make specific predictions, and their general principles are so broadly construed as to be compatible with any particular observations, whereas scientific theories such as Copernicus' heliocentric theory or Einstein's relativity do make novel predictions, i.e. predictions of new phenomena or entities. As the historian Koyré once said, the amazing thing about Copernican astronomy is that it worked, despite the overcast sky of Copernicus' Poland! Using Copernican astronomy, Galileo could predict the phases of Venus, a novel phenomenon not predicted by Ptolemaic astronomy and observed by Galileo himself with his telescope. Or consider Einstein's general relativity, which predicted light-bending, a phenomenon indeed observed by Arthur Eddington's expedition to Brazil in 1919. What makes Copernicus' or Einstein's theory 'scientific' is not just having positive instances, but instead, being able to make very specific and precise predictions about previously undreamt-of phenomena – predictions that *may turn out to be wrong.*

Popper's conclusion was that scientists should be looking for instances that are risky predictions, namely **potential falsifiers** (predictions that if proved wrong, would reject the theory). Having no potential falsifiers is the hallmark of dubious scientific standing. Pseudo-scientific theories have a tendency to *accommodate* evidence, as opposed to *predicting* novel, risky phenomena. But no matter how many positive instances of a generalization one has observed or accommodated, there is still no guarantee that the next instance will not falsify it. No matter how many white swans we might have observed, nothing excludes the possibility that the next observed swan will be black, as indeed explorers found in Australia. Hence, Popper's conclusion that the distinctive method of science does not consist in confirming hypotheses, but in falsifying them, looking for one crucial piece of negative evidence that may refute the whole theory.

According to Popper, science proceeds by a method of **conjectures and refutations**: scientists start with bold (theoretically and experimentally unwarranted) conjectures about some phenomena, deduce novel undreamt-of predictions, and then go about finding potential falsifiers for those predictions. Currently accepted scientific theories have passed severe tests and have survived, without being falsified as yet. If a theory does not pass severe tests, and/or if there are no sufficient or suitable potential falsifiers for it, the theory cannot be said to be scientific. The history of science is full of theories that enjoyed a relative period of empirical success until they were eventually falsified and rejected: from the caloric theory of Lavoisier (which regarded heat as an imponderable fluid) to Stahl's phlogiston theory in the eighteenth century, and Newton's ether theory. Science has grown across centuries by dismantling and rejecting previously successful theories – scientific progress is characterized and made possible by falsification.

To conclude, falsificationism is the distinctive method of science, according to Popper. It is a deductive (instead of inductive) method, whereby scientists start with bold conjectures, and deduce novel predictions, which then they go about testing. If the predictions prove wrong, the conjecture is falsified and replaced with a new one. If the predictions prove correct, the conjecture is corroborated and will continue to be employed to make further predictions and pass more tests, until proven wrong. However, reality is much more complex than Popper's simple deductive scheme. In daily laboratory situations, scientists never test a scientific hypothesis or conjecture by itself. Nor can they deduce any empirical consequence out of any bold conjecture either. This problem, known as the Duhem–Quine thesis, is the topic of our next section.

The Duhem–Quine thesis: or, the problem of underdetermination of theory by evidence

Before Popper developed falsificationism as the method of science, the French physicist Pierre Duhem (1906/1991) at the turn of the century had already realized that no scientific hypothesis can be tested in isolation, but only in conjunction with other main theoretical hypotheses plus some auxiliary ones. Consider Newton's law of gravity. Scientists never test the hypothesis of gravitation by itself, but always in conjunction with other theoretical hypotheses H_1, H_2, H_3 (e.g. Newton's three laws of motion) plus some auxiliary hypotheses A_1, A_2, A_3 (e.g. A_1 says that the mass of the sun is much bigger than the mass of other planets; A_2 says that no other force apart from the gravitational one is acting on the planets; A_3 says that planetary attractions are weaker than attractions between the sun and the planets). Now, suppose we deduce from this set of main and auxiliary hypotheses some observable evidence *e* and we proceed to test whether *e* occurs or not in nature:

$H \;\&\; H_1 \;\&\; H_2 \;\&\; H_3 \;\&\; A_1 \;\&\; A_2 \;\&\; A_3 \rightarrow \text{evidence } e$

Suppose we find that *e* does not occur (or that the measured value for *e* is not what one would expect from this set of hypotheses). This would only indicate that there must be something wrong with the whole set of hypotheses:

$H \;\&\; H_1 \;\&\; H_2 \;\&\; H_3 \;\&\; A_1 \;\&\; A_2 \;\&\; A_3 \rightarrow \text{evidence } e$
Not *e*

Then not-$(H \;\&\; H_1 \;\&\; H_2 \;\&\; H_3 \;\&\; A_1 \;\&\; A_2 \;\&\; A_3)$

But we do not know whether it is H or H_1 or H_2 or H_3 or A_1 or A_2 or A_3, or any combination of any of these main and auxiliary hypotheses, which is actually refuted by the negative evidence. Duhem concluded that confirmation is not a process exhausted by comparing a single hypothesis with some observational evidence. The same (or very similar) observational evidence can in fact be entailed by more than one theory (and sometimes even incompatible theories) so that evidence may underdetermine the choice between theories: evidence may not provide us with strong reasons for accepting one theory over rival ones (obtained by tweaking one or more of either main or auxiliary hypotheses). This is what philosophers of science (see Stanford 2013) call the problem of **underdetermination of theory by evidence**.

The American philosopher W. V. O. Quine in a famous (1951) article further developed Duhem's idea. Quine arrived at a conclusion similar to Duhem's by criticizing what he considered as two dogmas of logical empiricism, namely **reductionism** and the **synthetic/analytic distinction**. Carnap's physicalism is a good example of reductionism: it claimed to reduce the whole system of science to the language of physics so as to guarantee intersubjective agreement. Quine argued that the logical empiricist's criterion of verification underpinned the reductionist claim that any theoretical statement can be reduced to an observational statement (i.e. a statement cashed out in a language that eschewed theoretical terms, e.g. 'electron', and used only terms referring to phenomena that could be easily observed and empirically verified). It underpinned also the analytic/synthetic distinction, since analytic statements are not amenable to being empirically verified, by contrast with synthetic statements. Since any attempt to define analyticity failed, and the analytic/synthetic distinction does not really stand, Quine concluded that we should dismiss the logical empiricist's criterion of verification, and replace it with a holistic approach, whereby we take each statement as related to the entire web of our knowledge. In Quine's words (1951, pp. 42–3):

> The totality of our so-called knowledge or beliefs, from the most casual matters of geography and history to the profoundest laws of atomic physics or even of pure mathematics and logic, is a man-made fabric, which impinges on experience only along the edges. A conflict with experience at the periphery occasions readjustments in the interior of the field … but the total field is so underdetermined by its boundary conditions, experience, that there is much latitude of choice as to what statements to re-evaluate in the light of any single contrary experience. No particular experiences are linked with any particular statements in the interior of the field, except indirectly through considerations of equilibrium affecting the field as a whole.

Given this holistic picture, the process of confirmation or refutation of a hypothesis can no longer be regarded as a one-to-one comparison between the hypothesis and a piece of evidence. Instead, it takes place through a variety of direct and indirect routes across the entire web of knowledge. Of course, Quine claimed, there are peripheral areas of the web (say biology) that are more directly exposed to experience, and hence more suitable to being confronted with it directly. There are, on the contrary, internal areas, such as logic or mathematics, which are less exposed to direct empirical evidence. But this does not mean that those areas (logic or mathematics) are analytic, i.e. that their truths are not grounded on matters of fact, or that they cannot be refuted by experience. By contrast with logical empiricism, Quine believed that even the most fundamental principles of mathematics are amenable to being refuted by experience. The extent to which different statements are subject to the

of 'scientific paradigm', he thought a scientific paradigm (or what he later called a 'disciplinary matrix') would typically include the dominant scientific theory, the experimental and technological resources, no less than the system of values of the community at a given time (e.g. how the community may value judgements of simplicity, accuracy, plausibility, and so on). In addition, a scientific paradigm includes also what Kuhn called 'exemplars', i.e. 'the concrete problem-solutions that students encounter from the start of their scientific education, whether in laboratories, on examinations, or at the ends of chapters in science texts' (1962/1996, Postscript, p. 187). Any scientific community in periods of normal science acquires its identity by working on an accepted textbook (be it Ptolemy's *Almagest*, or Newton's *Principia*) and solving well-defined problems or puzzles within a well-defined textbook tradition. No attempt to test, falsify, or refute the accepted paradigm takes place during periods of normal science.

Only when a sufficiently large number of anomalies – which cannot be done away with – accumulate, does the accepted paradigm undergo a period of crisis. In periods of crises, a new paradigm may come to the fore, and the crisis resolves into a scientific revolution when the scientific community decides to abandon the old paradigm and shift consensus around the new paradigm. Kuhn stressed how theory choice in these cases is not determined by the alleged superiority of the new paradigm over the old one. The consensus-gathering process is not determined by the new paradigm being more likely to be true or correct than the old one, but by the increase in the puzzle-solving power of the new paradigm. The new paradigm should be able to solve more puzzles than the old one, and thus Kuhn redefined scientific progress in terms of increased puzzle-solving. But this shift of focus from Popper's falsification to Kuhn's puzzle-solving has far-reaching implications for the rationality of theory choice.

Kuhn famously claimed that scientific paradigms (say, Ptolemaic astronomy and Copernican astronomy) are incommensurable. **Incommensurability** meant lack of a 'common measure' to evaluate two paradigms (not to be confused with non-comparability or non-communicability) – in other words, lack of a common measure for rational choice between paradigms. Different paradigms use different scientific concepts, methodologies, resources, and even systems of values, so that – Kuhn concluded – paradigm shifts resemble psychologists' *Gestalt switches* rather than rational, objective decision-making processes. Kuhn's (1962/1996, p. 121) radical conclusion was that 'although the world does not change with a change of paradigm, the scientist afterward works in a different world'. A lot could be said about incommensurability and its implications for our views about science, but we will have to leave those reflections for some other occasion (see Bird 2011). We will go back to incommensurability and the rationality of theory choice in Chapter 3, when we review the prospects of a possible paradigm shift in contemporary cosmology. (Is there any crisis looming for our currently accepted paradigm in cosmology?)