Introduction

Philosophy of science is coming to resemble science: ever more specialized. Philosophers of physics deliberate the correct interpretation of quantum mechanics, whether spacetime is substantival or relational, and whether the time asymmetry of our world can be reconciled with the time symmetry of the laws of physics. Philosophers of biology grapple with determining the meaning of concepts such as gene, evolution, and fitness. Philosophers of chemistry ponder whether chemistry is really reducible to physics, and philosophers of climate modelling debate whether or not computer simulations can be trusted as much as experiments. While all these questions and debates are highly interesting and important, this *particularist trend* in the philosophy of science, as one might call it, has unfortunately gone to the expense of 'larger picture' questions about science: What is science? Is there a scientific method? How can we distinguish between science and pseudoscience? What constitutes a good scientific theory? Can science discover truths about the world? It bears some irony that today's philosophers of *science* have much to say about physics, biology, or chemistry but little about science. This book returns to one of those more ambitious philosophical questions about science, namely the question of what features characterize good scientific theories: what are *theoretical virtues* in science?

While this question clearly has a normative aspect, which I explore in this book, a successful answer will also have to take into account which properties of a theory scientists *actually* value when they decide to adopt a theory. I seek to establish this link to scientific practice primarily through various historical case studies in the venerable tradition of Duhem, Kuhn, Feyerabend, Lakatos, and others. Although any empirical or descriptive effort such as this one naturally comes with a certain inductive risk, it is an all-too-common fallacy to believe that due to the complexity and diversity we find in the sciences, any attempt to say something more general about science must be futile. Naturally, any empirical study of science will have to be carried out in one of the disciplines of science – such as physics, chemistry, and biology – as there can't be an empirical study of science per se. Crucially, though, true statements about theoretical virtues in any of those disciplines will automatically be true statements about theoretical virtues in science; to deny this would be to deny that any of those disciplines is a science, which would obviously be absurd. Such statements, of course, need not be true about *all* sciences or, even more implausibly, exhaust all there is to be said about theoretical virtues in science, but that need not be one's ambition. It's not mine, in any case.

Apart from attempting to answer the question What is a good scientific theory? this book also seeks to address the question Can our best scientific theories help us discover what is real? In fact, these two questions are intertwined: a good answer to the latter presupposes a good answer to the former, since we can judge whether a theory is likely to be true, if at all, only via the (internal and relational) properties of the theory. On the basis of my answer to the first question, I will argue that the 'mainstream' defence of realism, which is built on the idea that a theory's successful prediction of novel phenomena (or 'novel success' for short) is a theory's best evidence, is not justified. Instead, I think that realism can and should be defended along lines that are more in tune with the way in which theoretical properties are actually valued by scientists.

I will advance one 'central' and three 'auxiliary' arguments for realism. My central argument for realism is that a very virtuous theory – i.e., a theory possessing all of the standard virtues – is likely to be true. My three auxiliary arguments are as follows: (i) contrary to the standard view, there is an epistemic – i.e., knowledge-related – rationale for a controversial theoretical virtue, which is usually thought to be merely pragmatic; (ii) non–ad hocness is a sign of truth; and (iii) actual theory-choice decisions by scientists force us to accept that some theoretical virtues are indeed at least weakly epistemic. I will refer to the central and the three auxiliary arguments as my four *virtuous arguments for realism* and to the resulting position as 'virtuous realism'. Although all of these arguments are fairly independent, the three auxiliary arguments, as we shall see, support my central argument for realism.

I proceed in the following manner. In Chapter 1 of this book, I first introduce what many consider to be the standard theoretical virtues and the so-called explanatory defence of scientific realism. The success of the latter, it is well known, depends on whether theoretical virtues are truth-conducive. I will discuss this question with a particular focus on simplicity – perhaps the most controversial theoretical virtue. Many philosophers hold that simplicity cannot be truth-conducive because we would have to presuppose that the world is simple. But this is wrong-headed. Simplicity *can* be a reasonable epistemic concern without this presupposition. On the basis of what I call the *evidential-explanatory rationale* for simplicity, I will advance *my first virtuous argument for realism*. I call it the *argument from simplicity*.

In Chapter 2, I discuss an antirealist challenge that has been at the forefront of the realism debate in recent years, namely the so-called pessimistic meta-induction, and its cousin, the problem of unconceived alternatives, both of which appeal to the historical record of empirically successful but false theories. My main focus in this chapter, though, is on a challenge that has shed doubt on the very possibility of resolving the realism debate through any historical examples. The charge is that the 'base rate' of true theories, which is needed to compute the probability of a theory being true given its success, has been neglected and that we have no way of accessing it anyway. I take on this challenge and argue on the basis of the Kuhnian framework of theory choice and an important epistemological insight that a very virtuous theory is likely to be true when it is embraced by numerous scientists – even when the base rate is diminishingly small. I call this the *no-virtue-coincidence-argument*. It is *my second, and central, virtuous argument for realism.*

In order to fend off pessimistic meta-induction, realists have sought refuge in what has come to be known as the *divide et impera* move. That is, realists have restricted their commitments to those parts of theories which are responsible for empirical success and, more specifically, for *novel* empirical success, i.e., the successful prediction of novel phenomena. But is such a restriction warranted? Here I am doubtful. Such a restriction is only justifiable if a case can be made for the view, also known as *predictivism*, that novel success is better empirical evidence than non-novel success. However, the rationales that have been proposed to justify this view do not hold water, or so I shall argue in detail in Chapter 3. Novel success can be viewed as a form of a theory's fertility. There is another form of theoretical fertility which has received much less attention from philosophers. I shall explore this other kind of fertility, and arguments for realism based on it, in Chapter 4. My verdict here will also be negative, unfortunately.

The allegedly special epistemic status of both kinds of theoretical fertility has been motivated via an avoidance of ad hoc hypotheses. But what does ad hocness mean in the first place? This question will be the main focus of Chapter 5. The answer to this question seems intuitively clear: ad hoc hypotheses are those that are devised to save a theory from refutation. Such answer, however, tells us only about what motivates the introduction of such hypotheses, not about what is epistemically amiss with them. The latter we do need to know in order to understand why ad hocness is generally viewed as affecting a theory's empirical support in a negative way. My proposal is that ad hoc hypotheses are hypotheses that do not cohere either with the theories they amend or with the available background theories. Coherence, in turn, I believe can be understood as the provision of *theoretical* reasons for belief. This will form the basis for *my third virtuous argument for realism*. I call it the *argument from coherence*.

My *fourth virtuous argument for realism*, to be presented in Chapter 6, is based on the observation that, as a matter of historical fact, theoretical virtues have functioned as 'confidence boosters': scientists adopted (and did not just pursue) virtuous theories that resulted in ground-breaking discoveries despite the fact that these theories were contradicted by some of the available evidence. My argument for realism takes the form of a dilemma for the antirealist: either the scientists in question made ground-breaking discoveries *despite* making utterly irrational and methodologically wrong choices, or theoretical virtues are epistemic. I argue that we should try to avoid the first horn of the dilemma. This is my *argument from choice*.

In Chapter 7, I reflect on my chosen method of a historically informed philosophy of science. I argue that there are two fruitful roles for history and philosophy of science: (i) rational reconstruction of scientific practice and (ii) clarification of concepts used by scientists. Although rational reconstruction has a bad reputation in many quarters of philosophy and especially in history, I argue that there is a perfectly respectable way of doing it. Concept clarification, I argue, deserves more attention than it has received hitherto in the history and philosophy of science. Chapter 8 contains my conclusion. I end the book with an epilogue on the demarcation problem – that is, the problem of distinguishing science from non-science.