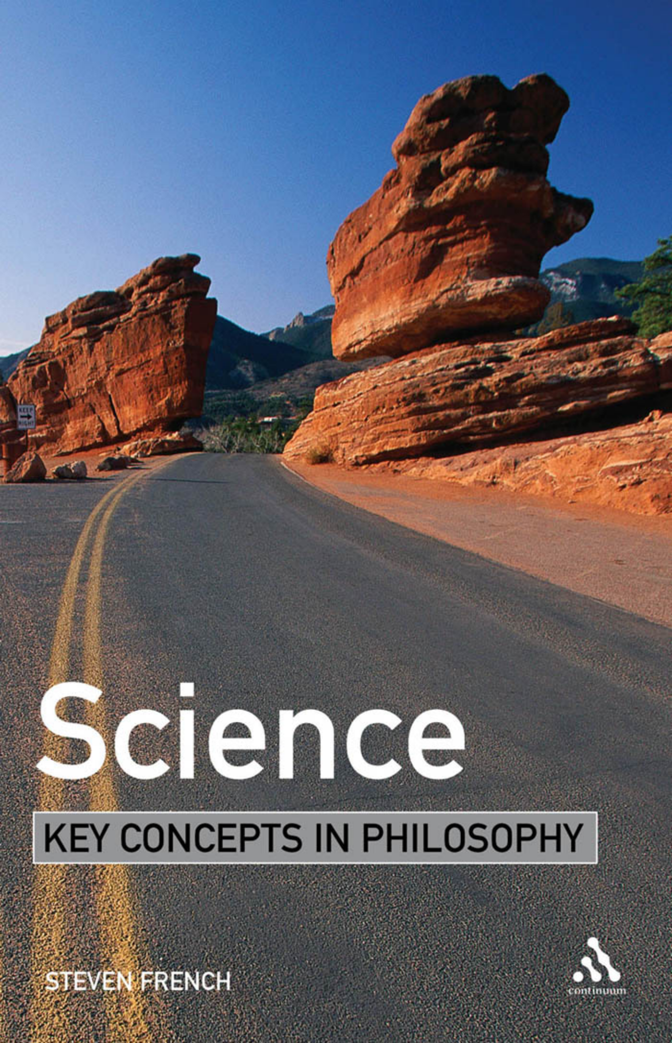

Science

KEY CONCEPTS IN PHILOSOPHY

STEVEN FRENCH

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CHAPTER 1

INTRODUCTION

It is always good to begin a book like this with a statement that surely everyone will agree with: as a cultural phenomenon, science has had more of an impact on our lives than any other. We could just list the technological spin-offs alone: genetic engineering, nuclear weapons, a cure for ovarian cancer, the laptop I'm writing this on, the microwave oven I cooked my dinner in, the iPod I listen to my (unfashionable) music on. . . . And of course the way in which such technologies are spun off from science is an interesting issue in itself, one which we do not have space here to tackle. But over and beyond the practical benefits, there is the profound way in which science has shaped and changed our view of the world and of our place in it: think of the theory of evolution and the way it has changed our understanding of our origins. Consider the further, related development of the theory of genetics and how that has transformed, not only our understanding of a range of diseases and disorders, but also our view of our behaviour, our attitudes, and of ourselves. Or think of quantum physics and the claim that reality is somehow fundamentally random; or Einstein's theory of relativity, according to which time runs slower the faster we move, and space and time are replaced by space-time, which is curved and distorted by the presence of matter.

Science is an amazing phenomenon, and has had a huge impact on human society over hundreds of years – so how does it work? How do scientists do the things they do? How do they come up with the theories? How do they test them? How do they draw conclusions from these theories about how the world might be? These are the sorts of questions we'll be looking at here.

How should we go about answering them? How should we go about discovering how science works?

One approach would be to pay attention to what scientists themselves say about their work; that is, to listen to the scientists. The problem with that is that scientists often have very different and sometimes downright contradictory views about how science works. Consider, for example, an apparently quite plausible statement: 'Science is a structure built on facts.'[1] Indeed, this is perhaps how many of us would begin to characterise science. It is surely what makes it distinctive and different from certain other human activities such as art, say, or poetry, or more controversially perhaps, religion. But now consider this admonition from Ivan Pavlov, famous for his experiments with the salivating dogs (which demonstrated how certain forms of behaviour can be triggered by appropriate stimuli): 'Do not become archivists of facts. Try to penetrate to the secret of their occurrence, persistently search for the laws which govern them.'[2] Now this might not seem to be in direct conflict with the previous statement; after all, Pavlov is simply urging us not to become obsessed with collecting facts, but to search for the laws underpinning them, and that can be taken to be quite consistent with the claim that science is built upon these facts (we might see facts as sitting at the base of a kind of conceptual pyramid with theoretical laws, perhaps, sitting at the top). W. L. Bragg, who did fundamental work with the use of X-rays to reveal the structure of materials (some of it performed near my place of work at the University of Leeds), went a bit further by insisting that 'The important thing in science is not so much to obtain new facts as to discover new ways of thinking about them.'[3]

This kind of view meshes nicely with the view that scientific 'facts' are rock solid in some sense, that they underpin the much vaunted objectivity of science. But then here is Stephen Jay Gould, the well-known professor of geology and zoology, defender of the theory of evolution and commentator on science: 'In science, "fact" can only mean "confirmed to such a degree that it would be perverse to withhold provisional assent". I suppose that apples may start to rise tomorrow, but the possibility does not merit equal time in physics classrooms.'[4] This suggests that the 'facts' are not to be taken as the bedrock of the structure of science. On Gould's view they are the sort of things about which we might give or withhold assent and in that giving and taking away, their status may change: yesterday's 'fact' might become today's misunderstanding, misinterpretation, or downright mistake. We shall return to this issue in Chapters 4, 5 and 6.

More radically, perhaps, Einstein maintained that 'If the facts don't fit the theory, change the facts.' What he meant here, is that in some cases our belief that a given theory is correct, or true in some sense, is so strong that if the 'facts' don't fit, we should conclude there is something wrong with them, rather than with the theory. And clearly there are examples from the history of science of theories that are so well entrenched that the first (and second and third . . .) reaction to an apparently anomalous experimental fact would be to question the fact (or the experimenter who produced it!). Some scientists and philosophers of science would abhor such an attitude, arguing that allowing theories to become so entrenched would be to sound the death knell of science itself.

That might seem a bit melodramatic, but we can surely understand the concern: how can science progress if certain theories become so well established that they are viewed as pretty well inviolable? I don't actually think this happens in practice; rather facts that don't fit with such theories are subjected to extra-critical scrutiny, but if they survive that, then the theory itself may come to be seen as flawed. Nevertheless, it is not as straightforward as Einstein, again, seemed to think, in the following assertion attributed to him: 'No amount of experiments can ever prove me right; a single experiment may at anytime prove me wrong.' This is a view – known as 'falsificationism' – which holds that the crucial role of facts is not to support theories but to refute and falsify them, since in that way science may progress – to which we shall return, again, in later chapters; for the moment, let's just note how Einstein appears to have contradicted himself! Another great physicist, Richard Feynman, expressed what he saw as the interplay between theory and experiment as follows:

> The game I play is a very interesting one. It's imagination in a straitjacket, which is this: that it has to agree with the known laws of physics. . . . It requires imagination to think of what's possible, and then it requires an analysis back, checking to see whether it fits, whether it's allowed, according to what's known, okay?[5]

Returning to our question of how science works, a better way, I would suggest, of getting to grips with it, is to look at scientific practice itself. Of course, this is complex and multi-faceted and just plain messy but rather than considering how scientists think science works, we should look at what they *do*. This raises the further question of how we should do that.

Some philosophers and sociologists of science have suggested that if we want to know how science works we should actually go into a lab, or a theoretician's office, and observe how science is actually practised. This is an interesting suggestion and some sociologists have indeed approached the observation of experimental scientists in the laboratory as if they were anthropologists observing the rituals and behaviour of some tribe with a culture very different from our own. Typically, such sociologists have insisted that they went in without presuppositions, or rather, that they recorded their observations as if they had no presuppositions about the work being carried out in the lab.

But of course that is nonsense; presuppositions cannot just be left at the door and even anthropologists do not do that. Furthermore, the procedure we adopt in examining scientific practice might depend on the questions we want to ask. As we shall see, our basic question asked here, about how science works, will be broken down into a further series: How are theories discovered? How are they supported, or not, by the evidence? What do they tell us about the world, if anything? What are the roles played by social and political factors in scientific practice? Except for the last, it is not clear how simply observing scientists in their natural habitats is going to cast much light on these issues.

And finally, most of us have neither the inclination nor the time to pursue such an approach (if you're interested in how a similar exercise might be carried out by a philosopher of science, consider the account by a well-known philosopher of science of his time spent in a high energy physics lab in Giere's book *Explaining Science*;[6] you might like to ask yourself to what extent this actually illuminates scientific practice). Instead, we look at case studies, some drawn from the history of science, some drawn from our own examination of the notebooks, records and papers of practising scientists. On the basis of such an examination, we can describe at least a certain aspect of scientific practice and, with that in hand, might start to formulate an answer to the above questions.

Now, I don't have the space to go into a huge amount of detail on these case studies here but I will draw on certain well-known (and perhaps not so well-known) episodes from current and past scientific practice to illustrate the points I want to make. Of course, you might feel that my descriptions of these episodes are too crude, too fragmentary or just too unclear to offer much in the way of illumination (and I'm sure my colleagues in the history of science will feel that way); that's fine and I hope if you feel that's the case then you will be encouraged to examine these case studies yourself, or even come up with some of your own. The claims I make in this book are by no means definitive; there is much more to do and develop and I hope the readers and students who use this book will add to these further developments.

There is one final point before we move on to the issues themselves, which is that some might insist that the really important question is not how science works, but how it *should* work. In other words, what philosophers of science and commentators in general should be concerned with is not merely describing what scientists do, how they come up with their theories and test them, etc., but actually specifying what they should be doing, by setting down certain *norms* of what counts as good science for example.

For many years, particularly in the first half of the twentieth century, this was taken to be an acceptable goal for the philosophy of science. Many philosophers and commentators on science saw themselves as in the business of spelling out what counted as good science, of delimiting it from bad or fake science and of effectively telling scientists what they should do in order to produce good science. Now you might say straightaway, 'What gives them the right?!' On what grounds can philosophers and others (but especially *philosophers*!) tell scientists how they should do their work? We can take the sting out of such questions and expressions of outrage by recalling that for many hundreds of years science was not regarded as distinct from philosophy, that it was indeed called 'natural philosophy' and that it was only in the late nineteenth and early twentieth centuries that the huge cultural impact of science, through technology and otherwise, as well as its transformative potential, began to be made apparent. It's a bit of a crude overstatement but not too far from the truth to say that it was only with the demonstration of science's capabilities for warfare, for the development of new weapons, new defences and so on, that governments and politicians in general began to take it seriously and as worthy of significant funding.

Setting aside the technological and material impact of science, and just considering the conceptual transformations it has promoted, or the changes to our worldview, even here science was not particularly regarded as something special or authoritative. We can go back and look at the great debates in the nineteenth century following the publication of Darwin's *Origin of Species* – debates which still echo down through the years – to see how science, or at least this aspect of it, came under attack. Or take an 'iconic' event in the history of twentieth-century science, one we shall return to in later chapters – the British astronomer Eddington's observation of the 'bending' of starlight around the sun which confirmed Einstein's claim that space-time could be curved and distorted by massive bodies (like stars). For reasons I shall touch on later, this apparent confirmation of a technically difficult and conceptually challenging theory in physics became *the* hot news of the day, making the headlines of the major newspapers and elevating Einstein in status from an obscure Swiss-German physicist to the crazy-haired representative of science in general. Yet Einstein's theories were rejected, often with derision, by many commentators (even scientists themselves were cautious and it is worth noting that he didn't get the Nobel Prize for his relativity theory but for his early work on an aspect of quantum physics). Indeed, a famous group of philosophers got together in the 1920s and published a tract declaiming Einstein's theories as clearly false since our conceptions of space and time were bound up with the very mental framework by which we came to understand and make sense of the world and in that framework space and time simply could not be 'curved'. Einstein himself was less bothered by such claims (he famously responded with the remark, consistent with the falsificationist attitude noted above, that 'If I were wrong, one experiment would be enough') than by the anti-Semitic attacks of certain Nazi sympathisers, but they illustrate how even what we now take to be major scientific advances were resisted and even rejected.

It is in this context that certain philosophers of science took on the role of defending science, of pointing out what they considered to be good science, using that to demarcate science from what they called 'pseudo-science' (we'll come back to this in later chapters, but astronomy would count as science and astrology as pseudo-science), and of laying out what they considered to be the norms of good scientific practice. On what were these norms based? Well, in part on what these philosophers of science took to be – in modern-day ad-speak – 'best practice'; so, Einstein's theory and Eddington's apparent confirmation of it typically feature in these accounts as exemplars of such practice, as we'll see later. But in part the norms of good science were shaped by certain broad values, to do with objectivity and rationality in general, themselves tied up with the testability of scientific theories.

However, it was the problems associated with defending these notions of objectivity and testability that led philosophers to drop out of the game of explicating how science should work and to concentrate on describing how it does. According to recent commentators, that has left a huge gap in the non-scientific public's ability to exercise some control of the agenda of science, leaving the field open to governments, multinationals and the like. Here's one such commentator who laments the loss of a normative element in these discussions:

> . . . scientists must acquire a competence in the consummate democratic art of negotiation – especially with a public who will bear the financial costs and sustain the eventual impacts of whatever research is commissioned. But perhaps more important, scientists must realize that the value dimensions of their activities extend not only to the capacity of their research to do good or harm but also to the opportunity costs that are incurred by deciding to fund one sort of research over another – or, for that matter, over a non-scientific yet worthy public works project. In short, part of the social responsibility of science is to welcome the public's participation in setting the priorities of the research agenda itself.[7]

I'm not going to get into the details of that debate here. Instead, all I'm going to do is to try to illuminate certain aspects of scientific practice in the hope that this may lead to a better appreciation of how science does, actually, work. And if anyone reading this finds it useful in helping to think through the issues involved in determining how science should work, then that's all to the good.

CHAPTER 2

DISCOVERY

When people think of scientists, they usually think of a man (typically) in a white coat; and when they think of what scientists do, they generally think of them making some great discovery, something for which they might be awarded the Nobel Prize. Discovery – of some fact, of some explanation of a phenomenon, of, again typically, some theory or hypothesis – is seen as lying at the heart of scientific practice. So, the fundamental question we will try to answer in this chapter is, how are scientific theories, hypotheses, models, etc. *discovered*? Let's begin with a very common and well-known answer.

COMMON VIEW: THE EUREKA MOMENT

In cartoons, creativity is often signified by a light-bulb going on over the head of the hero. It is supposed to represent the flash of inspiration. Scientific discoveries are likewise typically characterised as occurring suddenly in a dramatic creative leap of imagination, a flash of insight or a kind of 'aha!' experience. The classic example is that of Archimedes, the great Greek scientist of the third century BC, who, famously, was asked by the King of Syracuse to determine if a wreath he'd been given as a present was real gold or, somehow, fake. (The King wished to consecrate the wreath to the gods and of course it wouldn't do if it were anything other than pure gold. And because it was to be consecrated, it couldn't be opened up or analysed.) The wreath seemed to weigh the same as one made of solid gold, but that, of course, wasn't enough. Archimedes is supposed to have been visiting the public baths when he noticed that as he relaxed into the bath, the water overflowed and the deeper he sank, the more water flowed out. He realised that the water displaced could be used to measure the volume of the object immersed, and if the wreath were pure, that volume would be equal to that of an equal weight of pure gold; if not, if it were adulterated with an equal weight of, say, silver or lead, which has a different density from gold, then the volume would be different. At that point, Archimedes is reputed to have leapt from the bath and run naked through the streets, shouting 'Eureka!', or, 'I've found it!' (As it turned out, the volume was greater than the same weight of pure gold and the King realised he had been cheated.)

This might seem an old, outdated story. But here's Professor Lesley Rogers, a world famous neuro-biologist:

> A visitor to my lab, doing some labelling of neural pathways with these tracer dyes, happened to think, 'Well, let's give it a go.' And when we saw it, that was a Eureka moment. Yet it was chance – he happened to come, he was looking at something entirely different, I offered him the place in the lab, we then decided to just give it a go, and it turned out.[8]

Another notable example is that of Kary Mullis, who won the Nobel Prize in 1993 for his discovery of the 'polymerase chain reaction' (PCR). This is a technique that allows you to identify a strand of DNA that you might be interested in and make vast numbers of copies of it, comparatively easily (and by vast, I mean vast – from one molecule, the PCR can make 100 billion copies in a few hours). It is this which lies behind genetic 'fingerprinting', made famous through the TV series CSI for example, and it has become a standard technique in molecular biology, leading to a huge number of other applications and research results. Here is Mullis' own recollection of the discovery, made, he claims, as he drove up through the hills of northern California, with the smell of buckeye blossom in the air and a new idea in his mind:

> My little silver Honda's front tires pulled us through the mountains. My hands felt the road and the turns. My mind drifted back into the lab. DNA chains coiled and floated. Lurid blue and pink images of electric molecules injected themselves somewhere between the mountain road and my eyes.
>
> I see the lights on the trees, but most of me is watching something else unfolding. I'm engaging in my favourite pastime.