Scientific Method

An historical and philosophical introduction

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Contents

1 Introduction

We have increasingly powerful reasons for acquiring some understanding of the natural sciences. Their influence on the technologies that shape our lives has already been immense, and undoubtedly will continue to grow. In peace and in war, in work and in leisure, in health and in sickness, in each of the different stages of life, we cannot escape that influence. This book is being written with the aid of an electronic computer of a type which, as little as twenty years ago, was unavailable and unimagined by most people. You could well be reading it in circumstances equally unanticipated. On the surface at least, the most prominent differences between our lives and those of earlier generations are differences which have come about as a result of discoveries, investigations, explorations and inventions in the natural sciences. If we compare our modes of transport or communication with those available to previous generations, or compare our education with theirs, we cannot help but be struck with the consequences, for good or for ill, of scientific knowledge. On the credit side, that knowledge, but not that knowledge alone, has resulted in such benefits as the elimination of drudgery and repetitive work for some people, the eradication and control of some life-threatening diseases, and increases in crop productivity. For the sake of these and other benefits we have welcomed science. But we also fear science because, on the debit side, scientific knowledge, though not scientific knowledge alone, is responsible for such harms as the damage suffered by our environment, and has led to questionable experimental practices which need the control of so-called 'ethics committees'. Without the scientist's knowledge of theories, of laws, of techniques and, in general, of what is possible and what is not, the circumstances in which we live our lives would undoubtedly be different.

At a deeper level, too, we feel the effects of the growth of natural science and its technological consequences. Many people feel uneasy about, and some are alienated by, the impersonality of science, and even more so by what they perceive as its inhumanity; the future societies imagined by science-fiction writers tend to be uncomfortably alien rather than reassuringly familiar. This sense of science being something apart from us becomes more apparent if we take a longer view and compare the role that the science of Shakespeare's England played in understanding the place and significance of people in the world with its present role. Four hundred years ago the natural sciences and the picture they presented were inconsequential. They engaged the attention of otherwise idle gentlemen of means and leisure, but there was no sense that they were initiating enquiries which could or would have significant practical effect on people's lives. Even Francis Bacon, who was more sensitive than most to the practical consequences of scientific knowledge, would be astonished today to find that Her Majesty's Government is spending ever larger sums of money on laboratories to create what he called 'experimental histories'. Until the beginning of the twentieth century, scientific knowledge was limited in its scope and therefore limited in its practical significance. Industrialisation had affected some parts of western Europe, but it was the management of that process rather than the technology and science it used which raised moral and social issues. The discoveries of scientists were, it is true, interesting and sometimes entertaining, but for the most part neither they nor their effects presented a challenge which was more than intellectual. Now, though, at the end of that century, scientists know, or we think they know, so much about the natural world, including the species of animals we call human beings, that nothing important can be missing from the scientific picture of the world they present. The picture presented today by the scientific enterprise is so large and so comprehensive that we sometimes wonder whether there is room for anything else. Accordingly we raise questions about the role of creativity, sensitivity, feeling and reflection. We wonder, therefore, how and why the arts and music, literature, religion and philosophy can have a place in our lives. Sometimes these questions take a striking form in the work of artists, musicians, and writers themselves. The growth of science and technology makes many of us feel uneasy, not just because of its effect on the details of our lives but on account of its implications for the way we think about ourselves, our responsibilities and our place as individuals in an impersonal world where everything is weighed, counted, measured and, we think, understood. Our image of the scientist as philosopher, in some sense of that elastic term, is being replaced by the image of the scientist as accountant. Both images have the capacity to disturb us.

The tension which underlies this uneasiness arises from the need we all feel to control and predict our environment. We want to improve the circumstances in which we live our lives; we want to secure our future. To achieve these ends we must understand our environment; we must be able to explain why our circumstances are as they are. The kind of explanation which has proved most helpful is that provided by science, particularly natural science. There is, then, a close connection between our desire to enhance and make secure the circumstances in which we live, and the quest for the knowledge which will enable us to explain our world scientifically. Yet we are social beings; we live with other people who form an essential part of our environment. In so far as we think of them as having an inner life which is as important to them as ours is to us, we do not seek to control and predict their behaviour. Often what we look for in others is autonomy rather than conformity, spontaneity rather than predictability. We wish to understand the people who share our environment, but a scientific explanation will, we feel, miss what is essential. No doubt much of what matters to us is illuminated by natural science. After all, natural science has been a subject of interest for many people for many hundreds of years. It is not an insignificant object of knowledge. But still there is much more which is not, and cannot be, illuminated in an appropriate way by natural science or by any other kind of science. The projects which have tried to enlighten us–religion, philosophy, art, music, literature–have engaged our attention for thousands of years. They contribute something other than knowledge which is nevertheless important to us. They have served needs which are not met by science, despite its pervasiveness and power.

We should not, though, think that science is some all-devouring monster growing by force of its own inner logic. We can control and shape the way in which scientific knowledge changes; we do so control and shape it, though we are seldom aware of what we are doing. We, or more usually politicians, bureaucrats, financiers and industrialists, influence the development of the natural sciences and thus that of the technologies with which they are coupled. There is, in short, influence the other way. To be sure, the personal circumstances of a scientist and the interests she or he is able to develop have an effect upon what the scientist achieves, but more importantly there are powerful social, economic and political circumstances which will shape that effect as well. If we know more about some aspects of our natural environment than we do about others, that is not so much because some things are easier to investigate than others, though that is no doubt true, but rather because some sorts of enquiries are deemed more important by those who pay for science.

What we know is, in this sense, influenced by and perhaps determined by socio-economic forces. There are a great number of things which could become the object of scientific knowledge; a proportion of them have become the object of scientific knowledge, and socio-economic considerations have affected the selection of that proportion. Scientific knowledge is, in this sense, 'socially contructed'. But what we know is what is true and, on the face of it, this is a matter of how the facts are, rather than of what social forces are operating.

Scientific knowledge is, in this sense, not constructed, either socially or in any other way; what we or others might want to know or need to know in order to further a political agenda, or promote a social policy, or secure a financial advantage, is beside the point, for knowledge can only be of what is true. Similarly, when I buy a lottery ticket and choose a number, my choice is determined by many factors, some perhaps social, but the number chosen is not, or at least not for that reason, a social construct. What I choose, in the sense that it is my choice, is indeed 'constructed' by me rather than given to me. But what I choose, in the sense that it is a number, is given rather than 'constructed'.

There are, no doubt, complex issues about how in practice socioeconomic forces work, and we need to assess them carefully. But we also need to exercise care in describing their effects. Our interest in these forces and our conviction that they play a crucial role can easily blind us to dimensions of the scientific enterprise that have been important in characterising it. Until relatively recently, social factors played a negligible role in that characterisation, and there is perhaps a temptation to suppose that, in order to give them proper prominence, we must urge the neglect of all other factors. We suppose, or argue, that natural science is entirely a creature of our own making and the responsibility for its effects on our lives and our thinking lies with the vested interests of multi-national corporations, neo-colonial powers and the so-called free market; science neither has, nor has need of, a 'logic'. We infer that the supposed intellectual content of natural science is spurious and is no more than a means of deceiving people into maintaining and enhancing the current role of science in a society which respects those intellectual values science is supposed to represent. But there is little merit in this supposition, or in what we infer from it. Science does sometimes succeed in stating the truth, or at least a good approximation to it, and that truth is independent of the wishes of anybody, however powerful. The facts that keep aircraft aloft are, fortunately for passengers, not socially determined but ascertained on the basis of evidence by logical reasoning. The truth that cigarette smoking increases susceptibility to lung cancer is, unfortunately for smokers, not socially determined but established on the basis of evidence by logical reasoning. We could, no doubt, use our reasoning powers to establish other conclusions about other matters, such as the prevalence of schizophrenia in Eskimos, or the facts about the forces driving weather systems in the North Atlantic. If we have not done so, then the responsibility may well lie with social, economic and political considerations. But even if the selection of a subject matter for scientific enquiry is determined by powers, sinister or benign, outside of science and its methods, the nature of the logical reasoning we use in that enquiry has a part to play in understanding science.

So, although the understanding of natural science that we need does depend upon the powerful political, economic and social forces which drive the activities producing it, we should not glibly set aside the methods enabling it to have a fact-stating capacity. In this respect, as in some others, the work of a scientist is analogous to that of a detective. A detective attempting to solve a crime is no doubt subject to many kinds of forces and influences, if only because he or she will be part of an organisation which acts upon, and reacts to, the society in which it exists. But when the detective successfully solves a crime, in the sense that he or she correctly identifies the person or persons responsible, an important part of the explanation for the success will be the extent to which the reasoning used to justify the identification is persuasive. If it were just a matter of identifying a person or persons then, no doubt, we could provide an adequate explanation for the choice made by reference to forces and influences of various kinds. However, correct identification would elude such an explanation. Similarly in the case of scientists. The sociologist or anthropologist can study the work of a scientist in the same kind of way that they study other human activity, such as playing football or participating in a religious ceremony. Some of that work will have a successful outcome in the sense that it will result in new knowledge; some will not. This difference will not matter to the sociologist or anthropologist. Their explanations will not discriminate between successful and unsuccessful science. If we wish to explain the success of the work of scientists we will have to refer to the methods they use; we will have to refer to the reasoning they use to justify their new knowledge.

So we need to know something about scientific method in order to understand the production of new knowledge. But it is also important when we turn to the distribution of that new knowledge. For it is a further important feature of the late twentieth century that an everincreasing amount of information, including scientific information, is available to people. Information technology gives us access to much of what we might want to know and to even more of what we do not want to know. Even if we do not have a professional involvement with science we should, as responsible and effective citizens, have some knowledge of some science. But in order to transform scientific information into knowledge we have to understand and assess it, and this in turn requires a grasp of the kind of reasoning relevant in science. Often this knowledge is of practical value: knowledge of current research findings on causes of heart disease might have a practical effect on a person's diet and life style; knowledge of trials undertaken by a pharmaceutical company might have a practical effect on investment decisions; knowledge of successful biochemical techniques might affect a person's career development, etc. So understanding scientific reasoning–the methods of science–is not only an interesting and challenging task in itself; by undertaking it we will be better equipped to make the practical evaluative judgements that are required of us.

There is, then, a reason for paying some attention to scientific method. It is of course connected with the fact that many people have confidence in claims described as scientific. This is not to say that people think scientists are always right, even about scientific matters; they know perfectly well that scientists have made mistakes and that they will continue to make mistakes. Rather, it is to say that people think scientific claims are reliable. It is to say that, when faced with a choice between the predictions of an astrologer and an astronomer, many of us would place greater trust in the latter, even though we might think the former more interesting, more challenging, and indeed more relevant to our lives. It is to say that many of us find the geologist's account of the origin of the Earth more credible than the creationist's alternative account, even though we might recognise and respect the mythical, imaginative and emotional power of the latter. What, though, is the basis for this confidence? Setting aside the question whether we can justify confidence in scientific knowledge, we can take that confidence as a psychological fact and ask for its explanation. No doubt the authority of science is part of the answer: we trust scientific claims simply because they are scientific and therefore authoritative. But this is hardly helpful, for it simply invites a question about the basis of the supposed authority of science. A better answer to questions about the reliability of scientific claims, and hence their authority, directs our attention to the way in which they are established. Claims which count as scientific and therefore as reliable do so because they are established by means of a scientific method.

Associated with these ideas we sometimes find the view that the natural sciences are characterised not so much by their subject matter as by their methods. Thus we can, it is said, define physics as 'all that could be profitably studied by using a certain method'. This method of physics, we are told, was invented by Galileo and is experimental; an adequate understanding of physics depends on a grasp of this experimental Galilean method. That is why, in introducing novices to this science, and indeed to any science, it is thought important to refer to method. But there is a further implication that we should note. If scientists do need to know something about method in order to understand science, then it is also true that 'a scientific method cannot adequately be discussed if it is divided from the science to which it applies' (Toraldo di Francia 1981:6–7).

So the study of scientific method should shed some light on the character customarily attributed, rightly or wrongly, to beliefs expressed in scientific statements. Individually considered, scientific beliefs are of course very various. Some are general in their scope, others are specific and particular; some are well established, others are more tentative;

some are fundamental and central, others are less important. But despite these differences they have something in common which explains why we count them as scientific, namely the method by which they are established. The evidence which makes physicists believe Newton's law of gravity is quite different from the evidence which makes chemists believe that acids are electron acceptors, and yet in both cases the same kind of method is used to justify the beliefs on the basis of the relevant evidence. This means that, despite their difference, the beliefs share an important characteristic, and by describing them as scientific beliefs we show that we recognise that characteristic. To study this characteristic, and thereby to study what makes beliefs scientific, we have to turn our attention to the method used to justify them.

In the light of this background it is perhaps surprising that scientific method is currently neglected by philosophers of science. Many think the neglect justified. The interesting philosophical issues raised by examining scientific reasoning have been explored sufficiently thoroughly, and in any case a naturalistic approach to a subject which involves social co-operation rather than solitary ratiocination, calling on the insights of social scientists, is more appropriate. Part of the reason for resisting this facile response is that, until comparatively recently, scientific method occupied an important place in the study of both logic and the philosophy of science. For besides the connections often, and naturally, made between scientific method and science itself, there is a complex history which deserves our attention and interest. For a long time, analysing the logic of scientific reasoning has seemed important not only to those concerned with the scope of logic but also to those concerned with the scope of science. If we are to take seriously those who would have us believe that science has no logic, but is steered by rhetoric and ideology, we need to take this history into account. It is possible that all those analysing the logic of science were suffering from an illusion created by the psychological, social and ideological circumstances in which they found themselves. It is possible that there is no logic of science even though there is a long, sustained and coherent history of people who thought that there is such a logic. But possibilities are not always reasonable; we can and should ignore them unless they are shown to be reasonable. Moreover, there is a kind of arrogance involved in the claim that we are able, at last, to correct the mistake they made. Presumably we are as much influenced by our circumstances as they were by theirs; we are as liable as they to suffer from illusions. Of course, plausible tales can be told of the effects people's lives have on what they think. But this will not show that the possibilities envisaged are reasonable. For we can accept these tales without having to set aside other reasons why people think as they do, and some of them might be reasons of logic. To grasp the history of thinking about scientific method, and thereby illuminate current thinking, we cripple ourselves if we set aside the idea that the existence of real, objective, logical issues about the nature of scientific reasoning could have been an important reason why so much attention has been paid to scientific method.

Given its complexity, it is inevitable that the history traced in the chapters of this book is selective. My aim has not been comprehensiveness, even if that were possible. Rather, I have tried to tell a story which is coherent in the sense that certain ideas grew out of preceding suggestions and formed the basis for succeeding developments. Especial emphasis is placed on the role of ideas about probability in scientific method. One reason for this is that a great deal of interesting work has been done recently in recovering those ideas and relating them to the intellectual aspects of the historical contexts in which they emerged. A second reason is that current discussions of scientific method, even if they do not endorse the view that it is fundamentally probabilistic, cannot afford to ignore that view. This does, indeed, involve a certain distortion of history because, in describing and evaluating the contributions made by participants in that history, implicit use is made of information not available to them, namely, information about the fate of what they accomplished. Some distortion is, though, unavoidable if history is to connect with the present and help us understand it. One particular kind of distortion is readily apparent. It results from the omission of certain important thinkers and their ideas from this history. Thus, Aristotle's views about method are ignored, despite the influence that they exerted prior to the scientific revolution in the seventeenth century. In many respects we cannot properly appreciate the achievements of that century without information about those views, and about the very many ways in which they were received and modified. Nevertheless, the link with thinking about probability, which began to take shape in the seventeenth century, has meant that this history begins with Galileo and Bacon. There are other omissions. Descartes is a more important figure in the history of scientific method than is here acknowledged. In the nineteenth century there are several whose contributions are neglected, including Auguste Comte, Stanley Jevons, George Boole, John Dewey and Leslie Ellis. As for the twentieth century, the list of those neglected is embarrassingly long. To itemise it would be to invite further criticism and embarrassment. It does, though, include some important contributors that are unjustly ignored by others, such as W.E.Johnson.

In the past, both those we describe as philosophers and those we describe as scientists found it natural and important to reflect on the methods used in science. From the seventeenth century through to the nineteenth century, scientific beliefs were supplanting religious convictions, and it seemed important to clarify and explain the nature of the authority which enabled them to do that. Moreover, given that people were questioning, implicitly or explicitly, the authority of religious claims, it was entirely appropriate that they should question the authority of scientific claims. Galileo challenged the authority of the Church with respect to matters which, in his view, lay outside its scope. But he himself was attempting to draw conclusions about the real world, particularly about the structure of the celestial world, from evidence concerning the apparent world, the only world legitimately available to scientific investigation. Accordingly, he himself was challenged for laying claim to knowledge of matters outside the scope of his investigations. Similarly, Darwin challenged the power of the established English Church and claimed that with the aid of his theory of evolution, he could explain all those facts about the appearance, structure and behaviour of living creatures which had previously been thought to point to the existence of a designing creator. But critics of Darwin were quick to point out that if the only reason for believing the theory of evolution is its ability to account for such facts, then there can be no objection to other theories, including the theory postulating a designing creator, capable of accounting for the same facts. There is, as it were, more than one 'likely story' we can tell about the reason for the facts we observe; if Darwin's 'story' is to prevail over others, we need a good argument as to why it should.

Today, the motives for examining methods in science are different. We undertake such an examination, not because we seek to challenge the legitimacy of science, but because we wish to explain that legitimacy and, perhaps, to show that it is well grounded. For the most part, contemporary scientists do not find it either necessary or appropriate to turn to scientific method in defending the conclusions they state in their research reports. Reviewers and readers of such reports will take for granted what authors could have said about their method, and rightly so. There are, to be sure, occasions when these assumptions are mistaken, for scientists, like the rest of us, are sometimes economical with the truth. Reasons are found for setting aside data as 'untypical'. Sometimes the reasons are bad and, as a result, fraudulent claims are made. We may be led to believe that all relevant experimental evidence has been taken into account in reaching a certain conclusion whereas, in fact, evidence incompatible with the conclusion has been discarded for reasons which scientists may not have stated and may not be good. But those who deceive know that they are deceiving. They will know, that is, what conclusion they can legitimately draw, even if they fail to draw it.

In general, though, the point of paying attention to method is not to prevent or discourage fraud, or dishonesty, or carelessness. It is, rather, to enable us to see how the various sources of scientific knowledge contribute to the elaborate structures expressing that knowledge, and how we make judgements about the credibility of scientific conclusions.

This appears to be the reason why scientists often find it appropriate to explain what they mean by scientific method in the textbooks they write for students. They will there emphasise, usually in an introductory chapter, the importance of observation, of data, and of evidence in general, in determining what conclusions scientists should reach. It is, they say, an essential characteristic of scientific conclusions that evidence should be relevant to their truth or falsity. Many make it clear that the most satisfactory form of evidence is experimental evidence, and that therefore scientific methods are essentially experimental. The evidence yielded by experiment is important both because scientists can reproduce it and because they can control it in such a way that it will yield answers to specific questions. Thus, the authors of a recent chemistry textbook write that, in the laboratory, 'nature is observed under controlled conditions so that the results of experiments are reproducible' (Brady, Humiston and Heikkinen 1982:2). A similar view is expressed in a current physics textbook: 'In order to fulfil its objective, physics, as well as all natural sciences, both pure and applied, depends on…experimentation [which] consists in the observation of a phenomenon under prearranged and carefully controlled conditions' (Alonso and Finn 1992:4). In experiments, scientists interrogate rather than passively observe nature and are sometimes able to make their interrogations highly specific. Outside the laboratory they can do little more than observe and record the passing show of physical, chemical or biological phenomena, but inside the laboratory they can manipulate and control phenomena in order to answer the question: what would happen if…? The analogies between scientific evidence and legal evidence, between the interrogation of nature in the laboratory and the interrogation of witnesses in courts of law, have not been lost on scientists. The writer of a biology textbook writes: 'As with legal evidence, scientific evidence can be strong and convincing, or merely suggestive, or poor' (Weisz 1961:8). In the law, we seek to link conjectures about guilt or innocence with facts by interrogating witnesses and experts; in science, we seek to link our hypotheses and theories about nature with facts by interrogating nature in experiments.

Perhaps because it is expressed in such a general manner, the image of experiment which emerges from a reading of these accounts of how science is done is both misleadingly impoverished and disappointingly dull. The value and interest of a science depends, it would seem, entirely on its subject matter, on what is established rather than how it is established. And yet it takes little knowledge of a science or its history to realise that, often, experimentation is a source of evidence only because experimenters have exercised their manipulative skills, their imaginative powers and their patient persistence. Jakob Berzelius set new standards of experimental accuracy with his meticulous studies of the atomic weights of elements. Galileo, with an ingenious thought experiment, demonstrated brilliantly that objects falling freely from rest must travel the same distance in the same time. Michael Faraday established the identity of the different forms of electricity by means of a painstaking series of experimental investigations. But experimental know-how, ingenuity and determination are not inexhaustible, and consequently experiments are rarely decisive. Laboratory interrogations yield results which are often ambiguous and sometimes opaque. Part of the craft knowledge of a skilled experimentalist is to persuade us to adopt a particular interpretation of the evidence, to resolve the ambiguity in a particular way, to see despite the opacity. But when we come to form our own judgements about the conclusions that experiments are said to support, we try to resolve the uncertainties of the evidence before us and we are able to do so because, in part at least, we have balanced probabilities. The craft of the experimenter faces the critical judgement of those he or she wishes to persuade. Critical judgement will not always deliver the right answer but, by analysing the way it works, we will enrich our understanding of science as intellectually challenging.

So, although we construct and justify scientific knowledge on the basis of experimental evidence, the way that we do this is much more interesting, and much more problematic, than science textbooks suggest. The suggestion of these textbooks that to adopt a scientific method is to adopt a simple routine fails to do justice to the sophisticated skills which scientists use when they experiment and when they reason from evidence. Since the seventeenth century, philosophers have been trying to characterise the nature of these skills and, in the case of reasoning, they have made good progress. Less, though, has been achieved in understanding experimental skill. There are reasons–or excuses–for this. First, not all natural science lends itself to experiment. Astronomy throughout most of its history has been a conspicuous example, but in the earth sciences, too, fieldwork observation is sometimes more significant than laboratory experiment. Second, and more important, experimental expertise is closely tied to the subject matter of science. There are some general features which a good experiment in spectroscopy has in common with a good experiment in bacteriology, but the most interesting and the most important features are different. But third, the sheer variety of experiments defies any attempt to generalise. The language of experiment, or 'experimental discourse', is no longer confined to what we would ordinarily think of as science. We describe certain novels, plays, musical compositions and visual art as experiments; we refer to some political, social and economic programmes as experiments; and our everyday activities such as cooking or driving a car are sometimes described as experimental. What we mean when we use such descriptions is that what is described is in some sense out of the ordinary; it is unnatural. We know, or think we know, how a musical composition should sound, and when we hear music which fails to fit our expectations we describe it as experimental. We know, or think we know, what makes a political programme acceptable and practical, and when a programme is proposed which we think unacceptable or impractical we label it experimental. We know, or think we know, how to bake a cottage pie and we experiment when we depart, more or less radically, from the standard recipe. Such usages are not metaphorical; they signify, rather, that the scope of 'experimental discourse' is large. There is a complex network of similarities which bind these usages together, just as there is in usages of the term 'game'; but there is no single characteristic which they share and which could therefore serve as a definition of 'experiment'. It is not surprising, therefore, that the 'nature' of experimental investigation has proved elusive. Francis Bacon provided a sustained study of experiment in his *Novum Organum* but, until recently, very little had been added to it, perhaps because of the prevalent but unjust view that 'at least among philosophers of science Baconian method is now only taken seriously by the most provincial and illiterate' (Lakatos 1974:259). Studies by historians and sociologists of science are now beginning to enrich our appreciation of the complexities and subtleties of 'experimental discourse'. Whether these studies will enable us to create an enlightening philosophy of experiment remains to be seen.

Textbook writers also give some attention to the way in which experimental evidence is used to justify conclusions about the scope of a novel technique, the worth of a tentative hypothesis, the reliability of a natural law, and the value of a scientific theory. We can find more sustained accounts of this reasoning in textbooks of logic, and in the work of philosophers of science who have offered analyses of the logical aspects of scientific methods. In the nineteenth century, certainly, studies of logic which aimed at comprehensiveness would have included as a matter of course some discussion of methods of scientific reasoning. And in the twentieth century a number of well known texts which are designed to introduce students to logic contain chapters on scientific method. A typical account tells us that, as a plausible basis of observations of some natural phenomena, scientists formulate a hypothesis. They will then use this hypothesis to predict what will happen in new, untried, circumstances. If possible these untried circumstances will be created in, and controlled by, an experiment. Prediction and experimental result can then be compared. If they agree then the hypothesis is confirmed, or substantiated; if they disagree the hypothesis must be modified or discarded. Continued confirmation of the hypothesis by experimental evidence will increase the scientists' confidence in its truth, and they may begin to think of it as a reliable law. This is no more than a roughly sketched outline of what is called 'the method of hypotheses' or ‘the hypothetico-deductive method’; a detailed analysis is neither intended nor given. It is sufficient, nevertheless, to indicate where the crucial logical steps are taken.

Scientists, the textbooks say, have to derive testable predictions from their hypotheses. How, in practice, is this done? Lavoisier hypothesised that, when mercury is slowly burnt in a sample of air to form what we now call mercuric oxide, it appears to absorb part of that air. If he is right then, when we heat the mercuric oxide further so as to recover the mercury, we should find that the air released in the process is equal in quantity to the air absorbed. This prediction does follow from the hypothesis, but not from the hypothesis alone, for we need to make important if natural assumptions about what is happening in the experiment. As Lavoisier said, the slow burning of the mercury took place over a number of days and the reduction in the quantity of air remaining may have been due not to absorption of part of the air by the mercury but due to some overlooked fault in the experimental apparatus. In deriving the prediction to test, therefore, Lavoisier depended on the practical reliability of his equipment. The common and essential practice of testing and calibrating experimental equipment shows that scientists are fully aware of how important its reliability is in reaching secure conclusions.

Suppose, though, that an experimental test of the derived prediction is successful; the test and the prediction match. Lavoisier’s evidence about the quantity of air released from heated mercuric oxide, for example, matched the prediction he derived from his oxygen hypothesis. In general, any coincidence between test result and prediction is not perfect, and it certainly was not perfect in this example. Tests are only more or less successful and we cannot define the borderline between a successful and an unsuccessful test, despite the weight we wish to place on the difference. Evidence from tests can be clear and decisive, leaving no doubt whether a prediction is true or false. But more usually it is difficult to obtain, ambiguous and indecisive. Often it has to stand alongside other evidence with which it may appear to conflict, and if we are unable to resolve the conflict our confidence in the evidence may be compromised. We cannot, therefore, assume that the scientist’s judgement of a match between evidence and prediction is unproblematic; it will be made in the light of experience and will be fallible.

To these practical problems about the correct interpretation of experimental evidence we must add the logical difficulties we encounter when we try to draw a conclusion from a successful test of a hypothesis. The fact that we describe the test as successful suggests that our conclusion about the truth of the hypothesis should be positive. A successful test should increase our confidence in the truth of the hypothesis. But can it? Admittedly, this hypothesis enables us to derive a successful prediction, but there are other hypotheses which we could have used to derive the same successful prediction. Not all these hypotheses can be true, for there will be incompatibilities between them. Why, though, should we conclude that one rather than another is true, or that one rather than another is more likely to be true? If the only ground for concluding that a particular hypothesis is true is its ability to lead to one or more successful predictions, then the fact that there are other hypotheses which also lead to those same successful predictions seems to show that the conclusion is arbitrarily drawn. We might just as well have selected the hypothesis by lot, or randomly. Lavoisier's oxygen hypothesis did indeed lead to a successful prediction in the case of mercuric oxide, but we can make the contrary hypothesis that mercuric oxide absorbs a material substance–phlogiston–rather than releases oxygen, and it will lead us to the same successful prediction if we couple it with suitable other hypotheses. We have, so far, no reason for preferring either hypothesis over the other. In practice, of course, we normally have no difficulty in choosing one hypothesis rather than others as the one supported by the successful predictions to which they all lead. The difficulty arises when we try to explain the choice and to identify its rational basis.

Often, the reasoning which leads from a successful prediction to the conclusion that the hypothesis which generated it is likely to be true, or is more likely to be true than it was prior to the prediction, is described as 'inductive' reasoning. The 'method of hypotheses', that is to say, is an inductive method. If, by induction, we mean reasoning which leads from true premisses to a more or less probable conclusion, or probable reasoning, then this description is correct. Such reasoning is not truth-preserving, for it does not guarantee a true conclusion when true premisses are provided. But it is ampliative, for the conclusions it yields make claims which go beyond what is claimed in the premisses. We are risking a false conclusion when we reason inductively, but if the conclusion is true then it can be of considerable value to us and we may think that the risk is worth taking. Much will depend upon our being able to claim that the premisses of the inductive argument make the conclusion probably true to a high degree. But it is just this claim that is challenged. There are other conclusions which we can draw from the same premisses; why should we think any one more probable than any other? The method of hypotheses allows us to 'justify' any number of distinct and incompatible hypotheses, in the sense that we can derive these hypotheses using this method. Nothing, so far, entitles us to favour one over any other. So inductive, or probable, reasoning seems impotent. If the conclusion yielded by an inductive argument with given true premisses is probable, it is no more probable than any number of other conclusions that we can draw from those same premisses, so we have no reason for accepting it in preference to any of those other conclusions.

One way of responding to this difficulty is to eschew the method of hypothesis altogether. It is unreliable and cannot legitimately justify the adoption of any scientific belief. Should we justify any belief by using this method, it will fail to count as scientific. This response is found in many philosophers and scientists since the seventeenth century. Galileo and Newton both recognised that the weakness of the method meant that no proposition defended by it could have a claim on our allegiance. If we consider hypotheses as propositions justified by this method then, according to Galileo and Newton, hypotheses have no proper place in science. Resistance to, or at least suspicion of, the method has persisted. In the twentieth century, we find philosophers of science such as Reichenbach and Popper developing accounts of scientific method which avoid dependence on the method of hypotheses. Scientists, too, have sometimes tended to doubt or ignore the supposed implications of a successful prediction for the truth of a hypothesis, though they have accepted the implications of an unsuccessful prediction for the falsity of a hypothesis. Thus, the physicist Richard Feynman said that, in science, we compute consequences of a proposed new law and compare the result with experiment 'to see if it works'. 'If,' he continued, 'it disagrees with experiment it is wrong. In that simple statement is the key to science' (Feynman 1965:156). Significantly, he did not say what follows if computation and experiment agree.

A second way of responding has been to turn to ideas about probability. Henri Poincaré declared, of the mathematical calculus of probability, that without it 'science would be impossible' (Poincaré, 1952:186). And Bertrand Russell was equally forthright: 'we cannot understand scientific method,' he said, 'without a previous investigation of the different kinds of probability' (Russell 1948:354). The claim is that, prior to our having any information about the success or otherwise of a prediction implied by a hypothesis, we can make a judgement about how probable the hypothesis is. But we will make different judgements about different hypotheses, even though they are all capable of implying the prediction. The effect of this is that, after the prediction is known to be successful, we will be able to identify one hypothesis as the most probable. We will be able to assign a new probability to the hypothesis selected in the light of the successful prediction. Further successful predictions will further enhance this probability. There are, though, difficulties with this approach, for if these probabilities are going to justify our belief that a hypothesis is true we must show that they have a rational basis. We must show, that is, that our judgements of 'prior' probabilities are rational judgements. Philosophers and probabilists, such as Leibniz, the Bernoullis, Bayes and Laplace, suggested that our reason could provide a sufficient ground for the rationality of our judgements; that prior probabilities are a priori probabilities. But during the course of the nineteenth century their project began to founder as it became clear that reason does not always speak with a single consistent voice when consulted about probabilities. In the twentieth century, philosophers of science have sought to meet this challenge by developing a so-called subjective or personalist Bayesianism. This identifies constraints on prior probabilities which, though much weaker than those which had been proposed, are claimed sufficient to deliver rational judgements. But, despite the efforts that have been made over a long period of time to analyse probable reasoning, there are some who doubt that those efforts will help us to understand scientific method: 'probability', according to one influential contemporary philosopher of science, 'is a distinctly minor note in the history of scientific argument' (Glymour 1980:66).

A third response is to urge that we recognise the role played by the historical and social context within which any scientist must work and make decisions. We cannot understand any important part of what a scientist does by appealing to logic or to scientific method. The weakness of the method of hypothesis simply shows that scientists can decide nothing of significance by reference to it. This is not to say that scientists are illogical or unmethodical; it is to say that logic and method are totally inadequate when what we seek is an understanding of the construction of scientific knowledge. We are persuaded to accept scientific claims in the same way that we are persuaded to accept non-scientific claims, namely by means of interests and rhetoric rather than reason or experiment. In science, as in politics, religion, philosophy, etc., our beliefs are consequences of complex historical, psychological and social processes and interactions. The power of these processes and interactions makes the attempt to identify a method for science misleading and unnecessary.

In the chapters which follow, these issues will recur in one form or another. The logical character of scientific method has tended to dominate the thinking of both scientists and philosophers, and the issues that have arisen are prominent in most chapters. Isaac Newton laid great stress on the need for rules of reasoning in science, and was very clear that some kinds of arguments used to justify conclusions are superior to others. His eighteenth-century successors turned to rapidly developing ideas about chance and probability in their exploration of the limits of legitimate reasoning. Nineteenth-century debates about scientific method grew out of this exploration and gave a sharper edge to the issues. Disagreements, with practical implications, emerged; the reasoning used to justify a theory was a legitimate target for those defending alternative theories. In the twentieth century, logical techniques have helped to illuminate and clarify some important questions, though they have also led to idealisations which sometimes seem to have only a remote connection with real scientific reasoning. Artificial, idealised languages may be conducive to clarity and precision, but arguments in science are expressed in natural languages which, though they may be supplemented with carefully defined scientific terminology, invariably contain obscurities and imprecisions. Many of the questions addressed by philosophers of science during the last fifty years have acquired a life of their own, in the sense that they have seemed worth solving irrespective of their relevance to real scientific enquiry. There is, that is to say, a philosopher's agenda which has to some extent developed independently of the practical questions which were its original basis.

The experimental character of scientific method has, until recently, received less attention. It has been enough to say that scientists simply assemble data relating to phenomena yielded by experiments, and then use those data to test theories and hypotheses. Thus Galileo used data from experiments with inclined planes to support his hypothesis that the speed reached by a falling object is proportional to the square of the time taken to fall from rest; Thomas Young, in his attempts to support a wave theory of light, used data derived from experiments in which an interference pattern was observed as a result of monochromatic light being passed through two narrow but close-together slits in a screen; Millikan used data from a series of experiments in order to show that each electron carries a discrete electrical charge. These examples, all from physics, are matched by similar examples from other natural sciences. In the eighteenth century, Stephen Hales devised experiments with results showing that the sap does not circulate in plants as blood does in animals but rather ebbs and flows in a regular daily manner; and towards the end of the nineteenth century, Louis Pasteur used data from his experiments on what we now call vaccines to test his ideas about how they work in protecting people from the effects of viruses (see Harré 1983:52–8, 96–104).

Such cursory accounts are, however, insufficient, and we will have an opportunity to see why in the chapters about Galileo and Bacon. They knew well that experimental discourse has a grammar which requires a more sensitive appreciation of the skills and abilities which experimentalists bring to bear in their laboratories. Their contributions were, though, very different. Galileo is often hailed as the first experimental physicist, and there is much in his published and unpublished writings which justifies that description. It was his consummate ingenuity in calling on evidence from experiments, sometimes quite simple experiments, which impressed his friends and followers, and which irritated his foes. Bacon, by contrast, had little or no skill in devising and using apparatus and measuring devices; yet he thought harder than most about why experiments are important. In them, he showed, we can bring together the practical arts and the theoretical sciences, and thereby promote the 'advancement of learning'.

During the last few years there has been a revival of interest in the philosophy of experiment. As a result of this interest, philosophers of science have begun to pay more attention to the vocabulary of 'data', 'results', 'observations', 'phenomena', etc. that we use to speak about experiments, and to distinctions previously overlooked, such as that between data and phenomena. They also take more account of the practical difficulties in obtaining 'good' data from experiments, and of how experimenters deal with data they judge not to be 'good'. Experiments are not simple events with clear beginnings and ends; they are human interventions in a world of numerous conflicting influences and forces, and have their origins in earlier related investigations and their termination in later explorations. It is not surprising that the data produced in laboratories are sometimes unreliable, often contradictory, and always ambiguous. Experimental enquiries do have a life of their own, independent of any theories or hypotheses to which they may be relevant. We will consider the outcome of this recent revival in the final chapter.

What, though, of the project of elucidating the methods used by scientists when they justify, to themselves or to others, the claims they make about how our world works? Has it been a misguided failure? Is the search for a characteristic logic of science doomed to be fruitless and unnecessary? Some would claim that, whatever else may be true about science, and however limited our success may so far have been, there must be criteria which scientists can use in order to judge whether the conclusions they reach are likely to represent the facts correctly. Otherwise there would be no better reason to believe the conclusions of scientists than the conclusions of pseudo-scientists, quacks and confidence tricksters. To identify these criteria is to identify the methods and logic of science; it is not a trivial task even though it is a matter of making explicit what is taken for granted. We may still be debating important details about how the methods and logic of science work. We may even still be debating some issues of principle. But this does not in itself show that the project is misconceived; it may instead be a project where both principles and details are immensely difficult to make explicit. After all, for most of recorded history people have reasoned in ways that were always regarded as acceptable even though no-one knew how to make explicit the rules which made the reasoning acceptable.

Others, though, will say that a project intended to identify scientific methods would reveal the presuppositions or prejudices of scientists only at particular times and in particular places. To dignify these prejudices with labels like 'logic of science' and 'scientific method' is to attempt to give them political weight by investing them with a spurious authority and permanence (see Fuller 1987:150). Science is a social activity, and the means by which it is pursued are a matter for negotiation between scientists, and between them and those other members of their society who have some control over what they do. Accordingly, the methods of science are grounded in the needs and interests of the particular and different societies within which scientists work; they are not, as the philosophers would have us believe, grounded in universal requirements of rationality. It is, in short, not universal reason but the conventions agreed in particular societies which determine the legitimacy of the reasoning used by scientists in that society.

This issue, too, is relatively recent, though there are intimations of it in Poincaré. In the final chapter I will mount a defence of the project intended to identify and elucidate a logic for natural science. Such a project, I claim, meets a need which has, at least, a psychological basis. As earlier chapters will show, scientific method has played an important part in both logic and in the philosophy of science. Of late it has been neglected. Many have thought that, if there is anything interesting to be said about it, let psychologists or sociologists say it, for they have the appropriate concepts and they will know how to collect and analyse the relevant evidence. There are, no doubt, psychological and sociological aspects of scientific method which lend themselves to such scrutiny. Certainly it has historical aspects which deserve the attention of historians. But I do not doubt either that there are questions about scientific method which are philosophical questions. Such reasons as have been given for dismissing those questions as inadmissible are less than compelling. The contribution philosophers can make to our understanding of the natural sciences is, perhaps, modest. It is, nevertheless, legitimate, both as a contribution to that task and as an application of our ability to reflect upon, to question and to evaluate a characteristically human enterprise.

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