# Chapter 6: The Privileged Status of ‘Science’

> Plato is dear to me, but dearer still is truth.
> — Aristotle[1]

> If we see knowing not as having an essence, to be described by scientists or philosophers, but rather as a right, by current standards, to believe, then we are well on the way to seeing conversation as the ultimate context within which knowledge is to be understood.
> — Michael Oakeshott[2]

## Introduction: Science and Rationality

In the previous chapter, I critiqued the Enlightenment and the more extreme claims made for human reason in that tradition. In particular, I rejected the proposition that it was possible for human beings to possess certain objective knowledge. This chapter explores the implications of those insights, looking in particular at the status of those activities going together under the rubric of ‘science’ and of the knowledge they produce. The chapter is not intended to decry the enormous achievements of scientists in the past several centuries in throwing light on the natural world and the contribution that those achievements has made to our standard of living. Clearly, the institutionalised search for new scientific knowledge is a very important part of contemporary civilisation. What is intended in this chapter is a critique of the story told about the nature of that search in the past century and a half.

Rorty reminds us that in our culture the ideas of ‘science’, ‘rationality’, ‘objectivity’ and ‘truth’ are bound up with each other, where ‘truth’ is conceived of as correspondence with reality.[3] It has been usual to claim that ‘science’ is the very paradigm of rationality. The meaning of this claim is, however, uncertain as it is now quite clear that there is no logic of science as such—no certain single mechanical rubric for choosing and evaluating scientific hypotheses. Indeed, American philosopher of science Harold Kincaid tells us that the attempts to identify the defining features of science have a long and disappointing history.[4] The claim is a throw-back to the discredited positivism discussed in the previous chapter and to the hypothetical-deductive view of science associated with it. Such claims are part of the rhetoric surrounding the Enlightenment’s search for absolute knowledge—a knowledge that enjoys a privileged status over commonsense perceptions and understandings.[5] The critique outlined in Chapter 5, however, undermines the epistemological claims on which Western science has been based since the Enlightenment. Furthermore, as physical chemist and philosopher of science Michael Polanyi tells us, the rules of rational inquiry can be of little practical importance to the scientist: '[D]iscovery, far from representing a definite mental operation, is an extremely delicate and personal art which can be but little assisted by any formulated precepts.'[6]

In the same spirit, philosopher of science Ernest Nagel (1901–85) described science as an institutionalised art of inquiry and, as we will see shortly, that is a far better description.[7] It should also be clear from Chapter 5 that the particular Enlightenment view of rationality—which sees science as the paradigm—misrepresents the nature of human intelligence itself. Therefore, British philosopher of social sciences Peter Winch (1926–97) writes:

> Now it is of course true that the role played by such [scientific] work in the culture of [W]estern industrialised societies is an enormously important one and…[it had] a very far-reaching influence on what we are and what we are not prepared to call instances of 'rational thought'. But it was an essential part of my argument…to urge that our own conception of what it is to be rational is certainly not exhausted by the practices of science.[8]

Rorty challenges this identification of 'rational' with a special method; rather, he suggests it names a moral virtue: the virtue of being reasonable, encompassing tolerance, respect for the opinions of others, a willingness to listen, reliance on persuasion rather than force and eschewing dogmatism, defensiveness and righteous indignation.

Furthermore, it might be more appropriate to consider scientific investigations as being a response to our limited cognitive abilities—an attempt to create closed systems of belief to enable us to get by in the world—rather than an expression of a God-like capacity for generating understanding through 'rationality'. As social psychologist Paul Secord tells us, such closed systems rarely occur in the world, and only then in the laboratory.[9]

The above claim also assumes that Newtonian physics is the exemplar of a single archetypal scientific method whose laws are valid universally. Not only has this admiration for Newtonian physics faded, physicists are speculating that the so-called fundamental natural laws of physics are not immutable and transcendent but could be no more than local by-laws—valid only in our particular patch of the cosmos.[10] Davies reminded us only recently that conventional physics had no idea of what the external source of these laws might be.[11] Some theorists are speculating that they emerged as part of the evolution of the universe itself and our observation of it.

The assumption that there was a single scientific method was reflected in the work of Comte, who asserted that there was a hierarchy of knowledge in which 'science' was the pinnacle. Consequently, Comte argued that even sociology could be a positive science modelled after physics[12] —an ambition that sociology has long since abandoned, but one to which economics clings. Implicit in this belief is the proposition that the generalisations in physics are somehow more basic than those of the other sciences and certainly more basic than in the social disciplines, and that somehow everything can be reduced ultimately to physical generalisations. Reductionism in the spirit of Greek atomism lies at the heart of this assertion. This reductionism, this reification, this scientism, is, however, inconsistent with the wide range of real scientific practices and theories that are not reducible to physics. This inconsistency suggests that changes in belief and terminology are required. What is more, American philosopher Norman Swartz—drawing on Wittgenstein's model of family resemblances in which there is no core property shared by all members of a family—tells us that it is exceedingly difficult to tell precisely what a scientific law is. Like all concepts, there is no single defining set of necessary and sufficient conditions for a statement being a scientific law. While we would all accept that there is considerable order in the natural and social worlds, the generalisations we use to describe that order are social artefacts that are not literally true—being at best approximations, idealised reconstructions or instrumental tools going beyond the evidence available to us.[13]

Similarly, from the Enlightenment, we have inherited a cultural image of the scientist as a hero overcoming ignorance and bringing reality under control. The effect is to privilege particular types of inquiry, particular social practices and their associated stories over other forms of inquiry. It is not so much that one should necessarily object to the use of a general term such as 'science' to encompass the wide range of systematic inquiries carried out into the character of the physical and social worlds; rather, it is that 'science' now carries too many misleading entailments, implying a privilege and a unity of method that cannot be sustained.

Blaug reports that in the mid-nineteenth century, the usual story told about scientific investigations was that they started with the free and unprejudiced observation of facts. Such investigations were then supposed to progress by inductive inference to the formulation of universal laws and theories about those facts. The induced laws and theories were then to be checked by comparing their empirical consequences with all the observed 'facts'—including those with which they began.[14] In this context, scientific progress was seen as a linear process with the inclusion of more and more kinds of phenomena under laws of greater and greater generality—a reflection of the Enlightenment's faith in reductionism and in progress.

This image of science reflects the ideas of Bacon—one of the fathers of empiricism referred to in Chapter 2. This story has, however, been discredited. All perception and language is theory impregnated. Only those sensory impressions that are significant from some particular perspective become 'perceptions'.[15] Similarly, the positivist claim that only the statements that are verifiable by observation are meaningful is contradicted by positivists' own claims and by the large numbers of scientific theories that are based on far more than direct conclusions from sensory data—on ideas that are not observable directly.[16] Since Hume pointed to the inability of induction to establish logical certainty, the idea that induction formed the basis of a rational scientific method has been problematic.[17] Importantly, it is now said that these images of science give a distorted picture of the way in which scientific investigations have been conducted. In particular, the belief that it is possible to verify scientific theories has had to be discarded because rival theories can always be developed to fit the data in any particular case and there is no formal method that allows us to choose between such competing theories.[18] It is also clear that the picture of science as a cumulative linear process cannot be sustained. As positivist Donald Fiske (1917–2003) and cultural psychologist Richard Shweder tell us, '[T]he criteria for progress in the sciences are task-specific, diverse, ambiguous, and shifting. No criterion has served as a general standard or a universal ideal.'[19] Furthermore, not all science is concerned with a search for general laws. Indeed, political scientist Phillip Converse argues that each science has its own texture; while educational psychologist Lee Cronbach (1916–2001) claims that the social disciplines are progressive because they possess an ever-richer repertoire of questions—not because they have ever more refined answers about fixed questions.[20] Nor does the above image take adequate account of the institutionalisation of scientific investigations in the modern world.

So let us be quite clear: this story, this legitimising mythology—the legacy of the Enlightenment—has been discredited. A fundamental change in our understanding of scientific investigations has resulted from the work of recent philosophers of science[21] —an understanding that is not positivist and that makes far humbler claims. In particular, as we have already seen, the foundationalist claim that philosophy can describe on a priori grounds the standards for scientific knowledge has been discredited.[22] As a result, the late Australian philosopher of qualitative research, Michael Crotty, advises us to hold all our understandings of the natural and social worlds lightly, tentatively and far less dogmatically—'seeing them as historically and culturally affected interpretations rather than eternal truths'.[23]

It is now clear that scientific inquiry cannot provide us with the certain knowledge sought by the Enlightenment. That has proven to be a utopian dream. There is no certain truth to be found through method or technique. All knowledge is tentative and subject to revision. At best, all we can have is 'justified' belief, wherein the criteria for justification are themselves contestable. It is also agreed generally that all scientific knowledge is constructed socially—the work of an interpretive community. Furthermore, Descartes' method of radical scepticism has very little attraction for real scientists. The majority of scientific knowledge—including knowledge of the appropriate methods—is accepted on authority, as an act of trust in a particular scientific tradition with its corpus of knowledge, norms, ideals, heroes and heroic stories, as passed on by teachers and colleagues either through direct instruction or by example. Any individual scientist does not build her or his field anew, but lays down new deposits on the theoretical sediments already in place.[24] Importantly, critical theory warns us that these inherited constructed meanings can serve particular hegemonic interests and power structures in a world in which there are strong disparities in the distribution of power.[25]

According to Kincaid, there is a common thread to nearly all contemporary philosophy of science: it is not positivist.[26] Also, many of the differences between the above theorists are matters of emphasis.[27] It is questionable whether a strong differentiation is possible or desirable. Bernstein sums up this new perspective in the following terms:

> Awareness has been growing that attempts to state what are or ought to be the criteria for evaluating and validating scientific hypotheses and theories that are abstracted from existing social practice are threatened with a false rigidity or pious vacuity and that existing criteria are always open to conflicting interpretations and applications and can be weighed in different ways. The effective standards and norms that are operative in scientific inquiry are subject to change and modification in the course of scientific inquiry. We are now aware that it is not only important to understand the role of tradition in science as mediated through research programs or research traditions but that we must understand how such traditions arise, develop, and become progressive and fertile, as well as the ways in which they can degenerate.[28]

## The Contemporary Philosophy of Science

Let us look at these issues in a little more detail. Physicist and philosopher of science Pierre Duhem (1861–1916) taught us that there were no critical experiments in physics that could establish that a hypothesis was true. Theories face experimental refutation collectively. An experiment that refutes a hypothesis refutes a network of interconnected ideas—rather than a single idea—pointing to a problem within that network rather than pinpointing the problem. This is because predictions that are tested are deduced from theoretical hypotheses, auxiliary hypotheses and other knowledge. Consequently, it is always possible for the scientist to save a hypothesis by adjusting the auxiliary hypotheses. Duhem pointed out further that the choice of hypotheses to test is governed by considerations of order, symmetry and elegance rather than by their ability to describe the world accurately. Quine extended Duhem's idea to take in the whole of science, suggesting that it was an entire web of belief—even a world-view—that faced refutation as a whole. He also points out that it is possible to save any belief if we are prepared to make the necessary adjustments elsewhere. This is a phenomenon that is widespread and has been well documented by anthropologists in the case of beliefs in such things as magic. Norwood Hanson (1924–67) taught us that what we took to be facts depended on our conceptual system.[29]

Consistent with the above body of criticism, Popper rejected the nineteenth-century attempt to prescribe a method of discovery or of verification. He tells us:

> Science is not a system of certain, or well-established, statements; nor is it a system which steadily advances towards a state of finality...The old scientific idea of *epistēmē*—of absolute certain, demonstrable knowledge—has proved to be an idol...It may indeed be corroborated, but every corroboration is relative to other statements which, again, are tentative. Only in our subjective experience of conviction, in our subjective faith, can we be 'absolutely certain'.[30]

Popper does not provide us with a logic of science, nor does he believe that such logic is possible. He also rejected the positivist attempts to distinguish the meaningful from the meaningless along the lines proposed by the positivists. Instead, he sought to divide all human knowledge into two categories: science and non-science. In his view, science is distinguished from non-science by its method of formulating and testing propositions, not by its subject matter and not by a claim to certainty of knowledge. Nevertheless, Popper draws no absolute line between science and non-science, as falsifiability and testability are matters of degrees. All 'true' theories are merely provisionally true—having so far defied falsification. Because no individual scientific hypothesis was ever falsified conclusively, Popper suggested certain normative limits on the methods that could be used to safeguard theories against falsification based on what he believed to be sound practice. Let me emphasise the point: there exists no formal method to rule out *ad hoc* assumptions to save a hypothesis, and Popper has to employ normative rules to save his conjecture–falsification approach from such tinkering.

> The empirical basis of objective science has thus nothing 'absolute' about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or 'given' base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop, when we are satisfied that the piles are firm enough to carry the structure, at least for the time being.[31]

This does not mean, however, that falsification thereby ceases to be a valuable practical scientific tool—though there will be occasions when a hypothesis fails a test because of the inadequacy of auxiliary assumptions. Importantly, for Popper, a theory is scientific only if it gives rise to a known set of conditions that are testable and which will falsify that theory if they do not occur. This was the basis of his critique of Marxism: that it could not be subjected to empirical test and therefore was not scientific. A similar criticism is often made about the core of the neoclassical economic program. Blaug complains—with considerable justification—that mainstream economics preaches falsification but does not practise it. He sees this as a problem all through the social disciplines and even in the natural sciences.[32]

Hungarian Imr Lakatos (1922–74) followed in Popper's footsteps but talked about progressive and degenerating research programs, suggesting that it was a research program as a whole as it developed over time that should be the focus of attention, rather than its state at a particular point in time. He sees research programs as comprising a hard core, which is essentially untestable, and auxiliary hypotheses, which are testable. He suggests that a research program is theoretically progressive if it predicts some novel, hitherto unexpected fact, and empirically progressive if each new theory leads to the discovery of some new fact. He also cautions about being too hasty in assessing a program, while acknowledging that it is possible to persist with a degenerating research program for too long. This approach, however, while apparently reflecting real practice, weakens the normative significance of Popper's message. It also gives little practical guidance to a researcher or observer evaluating such a program at any particular point in time.

Importantly, Thomas Kuhn (1922–96), the most influential modern philosopher of science, argues that the appeal to falsification is misleading, because in practice scientists seem to be trying to verify rather than to falsify theories, and because theories that are falsified by particular experiments are rarely abandoned. His seminal work, *The Structure of Scientific Revolutions*, looked at the history of scientific practice and concluded that all science was based on an agreed framework of unprovable assumptions about the nature of the universe, rather than simply on empirical facts. These assumptions—a paradigm—comprise a constellation of beliefs, values and techniques that are shared by a given scientific community, which legitimise their practices and set the boundaries of their research.[33] Importantly, this view undermines directly the claimed objectivity and value-free neutrality of scientific investigations.[34] Kuhn argues that what he calls 'normal science' 'aims to elucidate the scientific tradition in which [the scientist] was raised rather than to change it'.[35] It uses the same methods that the rest of us use in everyday life. He suggests, therefore, that examples are checked against criteria, data are fudged to avoid the need for new models and guesses—formulated within the current jargon—are tried out in the search for something that covers the cases that cannot be fudged.[36] He goes on to argue that radically new theories arise not as a result of falsification but by the replacement of a hitherto explanatory model—or paradigm—with a new one. Such revolutionary science—the overthrow of a paradigm as a result of repeated refutations and anomalies—is the exception in the history of science. Implicit in this view is the idea that science does not advance in a steady, linear process.

Normal science is a thoroughly social process in which the problems to be examined and the general form the solutions should take are the result of agreement among a scientific community. It is a self-sustaining, cumulative process of problem solving within the context of a common analytical framework. The breakdown of normal science is marked by a proliferation of theories and by methodological controversy. In this climate, a new framework can appear offering a decisive solution to hitherto neglected problems. Conversion to the new approach takes on the nature of an identity crisis or a religious experience. Importantly, Kuhn tells us that there is no neutral algorithm or systematic decision procedure that will determine choice between competing paradigms. He claims that new paradigms are not only incompatible with their predecessors, they are incommensurable. This is because there is no third, neutral language within which rival paradigms can be expressed in full.[37]

Importantly, Bernstein likens this decision process to Aristotle's practical reasoning—the type of reasoning in which there is a mediation between general principles and a concrete situation that requires wit, imagination, interpretation and the judicious weighing of alternatives—reasoning that is shaped by the social practices of the relevant community. Resolution does not take place by an appeal to the canon of deductive logic or by any straightforward appeal to observation, verification or falsification. Rather, 'the cumulative weight of the complex arguments advanced in favour of a given paradigm theory, together with its successes, persuade the community of scientists'.[38]

Kuhn subsequently listed five criteria for choice—accuracy, consistency, scope, simplicity and fullness—stressing that these criteria, which functioned as values, were imprecise and were frequently in conflict. He explains that this does not involve a total abandonment of rationality in science, but rather a shift to a more realistic understanding—to a different model of rationality. Indeed, this shift from a model of rationality that searches for determinate rules to one that emphasises the role of exemplars and judgemental interpretation is a theme that pervades all of Kuhn's thinking. It is a view that picks up on Michael Polanyi's strong emphasis on the tacit knowledge of the scientist—knowledge acquired in the practice of science, which cannot be formulated explicitly in propositions and rules.[39]

Importantly, for Bernstein, the real character of rationality in the sciences in general—but especially in theory choice—is closer to the tradition of practical reason than to the image of *epistēmē* .[40] MacIntyre puts it this way:

> Objective rationality is therefore to be found not in rule-following, but in rule-transcending, in knowing how and when to put rules and principles to work and when not to. Consider how practical reasoning of this kind is taught, whether it is the practical reasoning of generals, of judges in a common law tradition, or surgeons or of natural scientists. Because there is no set of rules specifying necessary and sufficient conditions for large areas of such practices, the skills of practical reasoning are communicated only partly by precepts but much more by case-histories and precedents. Moreover the precepts cannot be understood except in terms of their application in the case histories; and the development of the precepts cannot be understood in terms of the history of both precepts and case histories.[41]

As the new framework achieves dominance, it becomes the normal science of the next generation.

Kuhn subsequently acknowledged that his earlier description of scientific revolutions involved some rhetorical exaggeration. Paradigmatic changes during scientific revolutions do not imply total discontinuities in scientific debate. In this later account, scientific development is characterised by overlapping and interpenetrating paradigms, some of which can be incommensurable. Paradigms do not replace each other suddenly; rather they achieve dominance in a long process of intellectual competition. Nevertheless, Kuhn's stress on the role of normative judgements in scientific controversies—and sociological factors such as authority, hierarchy and reference groups—remains intact, along with a mistrust of the role of cognitive factors as determinants of scientific behaviour.[42] Because it is a social process, scientific research is heavily dependent on the norms and ideals underpinning society in general and the norms embedded in inter-subjective communication in particular.

Reflecting on the above literature, Kincaid has suggested that good science requires at least the following evidential virtues—though he acknowledges that they are abstract and simplistic and admit multiple interpretations:

- falsifiability as the first line of empirical adequacy
- empirical adequacy—the more predictive success the better
- wide scope—predicting a wide variety of different kinds of phenomena
- coherence with the best information from other sciences
- fruitfulness in terms of a past track record and a future promise
- objectivity.

Kincaid goes on to stress the importance of fair tests, independent tests and cross tests. Given what has already been said, we might baulk at the possibility of achieving objectivity, but at a practical level this seems a useful suggestion—particularly in respect of normal science—as long as we do not get carried away with its status as 'the method' to the exclusion of other lists or with the knowledge status of the results.

Austrian-born Paul Feyerabend (1924–94) goes further than Kuhn and Kincaid. He points out that the physical sciences—the usual exemplars of scientific practice—have not advanced in a manner consistent with the canon of the strict methodologists, including those of Popper. Rather, he believes that progress has depended on a willingness to breach those canons. In part, this is because he sees normal science as a process of indoctrination;[43] and, because science cannot be grounded philosophically in any convincing way, he warns us expressly that scientific findings are no more than beliefs that should not be privileged over other beliefs. Indeed, there is a substantial anthropological literature arguing that the religious stories and beliefs of other cultures are no less rational than our own scientific beliefs—making good sense of experience to the members of those cultures. It is simply that the frames of reference, the paradigms and the tools available differ significantly. It is the height of ethnocentric intellectual arrogance to suggest otherwise. In this regard, Shweder tells us:

> A remarkable feature of the entities of religious thought is that they are thought to be external, objective, and real. But it seems to me, it is precisely that feature that marks a point of strong resemblance with scientific concepts, for one of the features of scientific thinking is that 'representations' of reality are typically treated as though they were real, and unseen ideas and constructs are not only used to help interpret what is seen but are presumed to exist externally, behind or within that small piece of reality that can be seen.[44]

Nevertheless, Feyerabend believes that scientists should test their perceptions—seeing this willingness as the difference between science and non-science, though these beliefs are no less culturally, socio-politically and historically conditioned.[45] He also draws our attention to the ways in which scientific communities can become closed, rigid and intolerant of new ideas, even though science is often seen as the very model of openness. It is an important part of the argument that will be advanced in Chapter 8 that the community of neoclassical economists has become such a closed group.

The fact that the creation of scientific knowledge is a social process has an important corollary. There are power relationships within any scientific community, as within any other community. Those power relationships, associated with such things as prestigious professorships, the editorship of journals, the referring of papers, participation in funding and appointment committees and positions within the broader community, can have a big influence on the acceptance or rejection of particular theories. And, of course, scientists are no more virtuous than the rest of us. American social scientist and methodologist Donald Campbell (1916–96) described this social construction of knowledge as a quasi-conspiratorial social negotiation involving ambiguity, equivocality and discretionary judgement.[46] This group identification can suppress intra-group disagreement, while exacerbating disagreements between groups and restricting the flow of information and people between them.[47]

Let me reiterate that there is significant agreement on some essential points among these critics of the nineteenth-century image of science. They are all anti-positivist. Strict justification cannot be achieved. In particular, we cannot stand outside our current language and structure of thought. Ultimate justification is not achievable; neither is inquiry free of presuppositions. Consequently, the belief that scientific knowledge is an accurate representation of reality has had to be abandoned. As Rorty put it: 'We understand knowledge when we understand the social justification of belief, and thus have no need to view it as accuracy of representation.'[48]

It is also clear that any explanation is an explanation from a particular partial point of view—an attempt to reduce the unfamiliar to the more familiar—so that there can be multiple, even inconsistent explanations. All such explanations are tentative.[49] All theories involve abstraction from the complexity of the world and any particular theory highlights particular attributes only. It is an extraordinary leap of faith to believe that any particular point of view can capture successfully the essence of any phenomenon.

In particular, Rorty tells us that the attempt to isolate science from non-science through the use of words such as 'objectivity', 'rigour' and 'method' assumes that scientific success can be explained in terms of discovering the language of nature. Galileo—in claiming that the book of nature was written in the language of mathematics—meant that mathematics worked because that was the way things really were.[50] For Rorty, this was simply a bad metaphor; rather, Galileo's reductionist mathematical vocabulary just happened to work—something that lacked a metaphysical, epistemological or transcendental explanation.[51] Consequently, for Rorty, the moral that seventeenth-century philosophers should have drawn from Galileo's success was that scientific breakthroughs were not so much a matter of deciding which of various alternative hypotheses were true but of finding the right jargon in which to frame hypotheses in the first place.[52] What is clear is that the extent to which our mathematical vocabulary matches that of nature—whether nature can reasonably be described as having a vocabulary—will always remain problematic.

It follows that empirical sciences cannot claim an essential grasp of reality and, as a result, a privileged status in the human conversation. It also follows that economics, sociology, political science or even philosophy cannot claim to be objective and rational in a way that moral philosophy, aesthetics and poetry are not.[53] In all cases, justification is a search for persuasive arguments—a fully social phenomenon—not a transaction between the inquirer and reality. In this connection, Peirce[54] referred to the 'indefinite Community of Investigators', while Mead[55] spoke of the 'Community of Universal Discourse'. As we will see in Chapter 7, these concepts have much in common with Habermas's ideal speech conditions. When it comes to matters of the basic structures of society and major issues of public policy, this community is to be found in the ordinary citizens of the society—not in some intellectual elite who would be the equivalent of Plato's guardians.

The real problem lies with us and the excessive faith we want to place in scientific knowledge—including knowledge about economics—and the faith we want to place in its practitioners, and in their capacity to free us from anxiety. As Gadamer tells us, '[T]he problem of our society is that the longing of the citizenry for orientation and normative patterns invests the expert with an exaggerated authority. Modern society expects him to provide a substitute for past moral and political orientations.'[56]

Further, he says that philosophical hermeneutics 'corrects the peculiar falsehood of modern consciousness: the idolatry of scientific method and of the anonymous authority of the sciences and it vindicates again the noblest task of the citizen—decision-making according to one's own responsibility—instead of conceding that task to the expert'.[57]

The above difficulties in grounding rationality and science undermine any sharp distinction between science, philosophy and any other critical inquiry—undermining the special status that we have hitherto attached to science. They also point to our inability to insulate scientific inquiry from the need for practical reason, for judgement and even wisdom. As German critical rationalist philosopher Hans Albert tells us, '[T]he problem of adequate criteria is a very general problem. It is to be found in every field of social activity—in every kind of problem-solving activity; in law, morals, politics, literature, the arts, etc—and not merely in the enterprise of acquiring knowledge in science.'[58]

## The Particular Difficulties of the Social 'Sciences'

Theorists have often sought to differentiate the social disciplines from the natural sciences on the grounds that the latter are more objective. Indeed, an invidious comparison is often made between the social disciplines and the natural sciences. This follows from a tendency to idealise the natural sciences and to see Newtonian physics as the exemplar of scientific practice. It is then assumed that the production of universal laws characterises the natural sciences in general—but this is far from being true.[59] Such a sweeping generalisation does not do justice to the diversity of scientific practice in the natural sciences or to the variety of criteria of success negotiated within those diverse fields.[60] Simple law-like behaviour and predictability are elusive in the natural sciences also—though in the natural sciences it is possible more often to get away with simple idealisations, to isolate a system and to treat its properties as context independent.[61] In any event—as the above account makes clear—the broad claims of the natural sciences to objectivity—in the sense advanced by the Enlightenment tradition—cannot be sustained.

Nevertheless, there are particular difficulties with the social disciplines, which add to the above problems, and which are reflected in unease about their status. The result has been the development of a separate theoretical discussion of the philosophy of the social sciences, which can be quite esoteric. This discussion is at pains to distinguish itself from the positivism criticised earlier—though there are still unreformed positivists in economics. William Outhwaite categorises this discourse into three schools—involving realist, hermeneutic and pragmatic perspectives—though there appears to be significant overlap between them.[62] Nevertheless, the main issue separating these perspectives is the extent to which any social discipline can describe a social reality independent of the observer and her or his description of it. This discourse overlaps with that in the natural sciences described earlier. Critical realism—following Roy Bhaskar—is possibly the current dominant school. It agrees that a distinction is to be made between the natural and the social sciences, that the latter do not operate in the same way as the former and cannot be studied with the same methods, and that social life is constructed continually through practice.[63] Nevertheless, in neglecting the limitations of language, they attempt 'to privilege a concept of the real that can be definitely discovered, described and activated under definable conditions'.[64] In this, they appear to be too optimistic. As educationalist and methodologist John Schostak explains, symbolic representation—including through language—can never be the full measure of the 'real'.[65] There is something missing of the 'real' in any representation that we cannot recover, however much we try to tame it. Schostak suggests that for critical realism to be useful, it has to deal successfully with representation in all its possible articulations, and with the emergence of understanding as acts of creative imagination shared through discourse. This is why I lean towards the pragmatic and hermeneutical schools.

None of the above positions suggest that we should not try to understand the social world. The disagreement is about the extent to which we are likely to succeed and the confidence with which we are prepared to apply the resulting insights. No one is claiming that in any particular investigation there is a single, ultimately true theory that is accessible to us. Nor can we ever fully escape the language with which we describe the social system. In short, the 'TRUTH' about society is not available to us.

For example, Kincaid, who describes himself as a realist—believing the idea that things exist and act independently of our descriptions—claims that because there is no simple logic of science we cannot evaluate social science by looking at simple formal traits. At the same time, he believes that good social science cannot be ruled out on a priori conceptual grounds. Rather, he claims we have to look in detail at the methods used and the kinds of evidence adduced. Importantly, he concludes that large parts of the social disciplines have failed to produce such good science.[66] He goes on to claim that the philosophy of science can contribute to the study of society only if it eschews a priori armchair theorising in favour of a philosophy tied intimately to the real practice of social science research. In respect of that social science practice, Fiske and Shweder tell us:

> It is obvious that social science is not a single integrated discipline; rather it is a collectivity of endeavours sometimes working cooperatively, sometimes borrowing from each other, and only occasionally collaborating in joint enterprises. It is a range of disciplines and methodologies, above and beyond the somewhat anachronistic categories in university catalogs.[67]

Kincaid agrees that the social disciplines employ methods that are not found anywhere in the natural sciences.[68] Nevertheless, he claims that the social disciplines can be good science by the standards of scientific adequacy of the natural sciences—describing basic patterns found in nature—but only by meeting those standards. This is because he believes that human beings are part of the natural order and are amenable to scientific understanding. This, he declares, is simply an extension of an Enlightenment tenet. Given our critique of the Enlightenment, this is hardly a persuasive argument. Furthermore, he believes that behind the diverse methods of the natural sciences there is a common core of 'scientific rationality', which the social disciplines sometimes share.[69] Importantly, he believes that social science is distinct from psychology—with its own domain of inquiry largely to do with understanding large-scale social structures—and in the process rejects the methodological individualism of much of the social disciplines. Interestingly, Kincaid goes on to define those scientific standards in terms of 'scientific virtues'—virtues promoting confirmation and those promoting explanation—standards that deny that scientific justification can be reduced to a certain method. It should already be clear that Kincaid agrees with Rorty and that methods do vary across the sciences and do not provide a foolproof, mechanical basis for choosing theories. Nor does Kincaid believe all is well with social research. Nevertheless and confusingly, Kincaid appears to believe that there is something special about science, that, in effect, it possesses a privileged form of justification—a belief I have already discounted.

Kincaid's belief that human beings are part of the natural order goes to the heart of the problem of social inquiry. This is a belief that we must reject as being much too strong. In effect, Kincaid seeks to defeat the dichotomy made in our vocabulary between the natural and the social—a vocabulary he uses while denying its import. While the phenomena studied in the natural sciences could have an existence independent of the concepts used to describe them, this might not be true very often, if at all, of the social disciplines.[70] Rather, the social disciplines are concerned with human beings who—as we saw in Chapter 2—construct their social reality, defining themselves in symbolic forms with shared understandings of the world, which they use to structure their actions.[71] Consequently, it is not the way the world is, but the way we conceptualise it, that influences our actions.[72]

This is the reason why leading Canadian political philosopher Charles Taylor makes a distinction between human 'behaviour' and human 'action', in which the former is caused by forces over which the individual has no control—analogous to the forces of nature—and the latter results from that person's intentions. He then points out that the language describing human conduct is mainly an intentional one and it is about human action rather than human behaviour. It is the language of reasons and not of causes. This is important because—as we have seen already—the interpretation of any phenomenon depends on the language available to us, bringing with it particular theoretical entailments. The meaning of everyday behaviour and even the very fabric of society are woven into our ordinary vocabulary.[73] It is also clear that the meaning we attach to human actions depends on the particular circumstances with which we are dealing. Importantly, social structures and institutions play a large role in determining our actions. Secord seeks to clarify the situation, telling us that while social structures have real effects, they are different from natural structures in that they do not exist independently of our conceptions; nevertheless, they precede the individual. Such 'structures preceded the entrance of individuals into society, and individuals act within them as a medium'.[74] This is a view I endorsed in Chapter 2.

Additionally—as has been pointed out already—language, including the language used in the social disciplines, is inherently metaphorical. Similarly, the interpretation of any text and of any situation is dependent largely on historically situated conventions. Gergen draws our attention to the way in which the particular literary figures used dominate the process of interpretation.[75] He reminds us that, once a particular metaphor is selected, it restrains what else can be said. The root metaphors differ across the social disciplines, providing different perspectives—ideologies even—which are difficult to reconcile.[76] These stories—these definitions of ourselves—reflect to some extent the stories that social researchers tell. Our stories, therefore—our language games—cannot be objective or normatively neutral, as we will see in greater detail in the next chapter. While this is also true of the natural sciences, there is almost a qualitative difference in the extent to which these respective disciplines can aspire to objectivity. One further consequence is that generalisations in the social disciplines are generally narrow in scope.[77] Nagel suggests:

> [The] conclusions reached by controlled study of sample data drawn from one society are not likely to be valid for a sample obtained from another society. Unlike the laws of physics and chemistry, generalisations in the social sciences therefore have at best only a severely restricted scope, limited to social phenomena occurring during a relatively brief historical epoch within special institutional settings.[78]

Similarly, Gergen tells us that there are few patterns of human action that are not subject to significant alteration, while cultural anthropologist Roy D'Andrade records that the different fields of science have different canons of generalisation.[79] While researchers aspire to tell integrative stories, it could be simply inappropriate for social researchers to seek to emulate the natural sciences in an attempt to derive 'fundamental general laws' describing human conduct. Cronbach argues that this particular idealisation of scientific research—the development of general lasting laws on the model of parts of physics[80] —is not achievable in the social disciplines.[81] It might also not be achievable in much of the natural sciences. Nor is there any good reason to expect a unity of method across the social disciplines. On the contrary, Fiske tells us that such knowledge is fragmented, composed of multiple discrete parcels—a consequence of the different objects of inquiry and different methods of knowing. As a result, these bodies of knowledge are likely to always remain separate.[82] In particular, generalisations and theories in the social disciplines are rarely abandoned because most conceptual statements in those disciplines are formulated in such a way that they cannot be falsified. Fiske suggests that, in part, some of these difficulties arise because of too high a level of aspiration on the part of the social researcher.

All of this suggests that a strong onus lies with the theorist intent on developing systems of interrelated generalisations in a particular area of human activity to demonstrate that such generalisations do exist and then to delineate their scope. Consequently, the question arises as to whether neoclassical economics has discharged that obligation. I think not. As Ormerod tells us, the idea that people respond to economic incentives could be a universal generalisation, but the strength of any response to any particular set of incentives is emphatically not universal; it depends on the social, institutional and historical context. Human beings are not compelled to act by social 'forces' in the same deterministic way that natural phenomena respond to natural forces. Weber suggested therefore that the natural sciences were concerned with *erklären* or explaining focused on causality, while the social disciplines were concerned with *verstehen* or understanding. Such things as meaning, intention, ideas, values and emotions were, according to Descartes, non-things and were beyond the reach of the mechanical sciences.[83]

One approach used in an attempt to get around this problem is to consider reasons as causes. While Weber agreed that there was a logical distinction between natural and social reality, he did not believe that these differences required different scientific methods. He believed that uniqueness and historicity were features of natural as well as social phenomena. In any event, with his positivist, rationalist bent, Weber sought a rigorous method that would enable claims made about the social world to be subjected to empirical validation. While Weber accepted that no conceptual system could do full justice to the complexity of particular social phenomena, the tool he adopted for this purpose was the concept of an 'ideal type'—an idea used also by Mill and his contemporaries. This idea—a reflection of the perfectionism and transcendentalism embedded in Western thought and in particular the positivism popular at the time—is the conceptual source of the idealisation of the market in economics. An ideal type is an analytical construct, a rationalised reconstruction, a stereotype, a fiction even, deliberately exaggerating what are thought to be typical actions to produce a coherent whole in an attempt to get to the essence of a social reality—assuming in the process that there is such an essence to be got at. As such, it looks suspiciously like an attempt to revive Plato's forms in the context of the social disciplines. The ideal type was to be derived inductively from historical reality, though it would never correspond with reality. Importantly, Weber thought this tool could be applied only to social behaviour that was rational and goal oriented, which he believed was increasingly dominating Western society. In this regard, it is important to remember that Weber conceived of four different orientations towards social action—instrumentally rational, value rational, affective and traditional—though these categories were not intended to provide an overall classification. As we will see in the next chapter, the rational, instrumental nature of much economic activity is open to devastating criticism. In these circumstances—on the basis of Weber's own qualification—it can hardly be assumed to apply to economics.

Furthermore, the technique is open to misinterpretation resulting from the common metaphysical assumption that 'scientific laws' are authoritative—that is, that they determine the way the world is (that scientific generalisations, 'laws', are causal agents) rather than being simply descriptions of the way the world is. In the absence of a god—conceived of as a lawmaker, dictating the laws of nature and of human conduct in the way that the Enlightenment and Smith had assumed—it is hard to imagine where any authoritative force could come from. No one these days, however, thinks that the invisible hand of the market is the hand of God. Perhaps, given Weber's restriction of this method to the analysis of rational social action, it is rationality that is to provide this authoritative force. If so, it will just not do. To claim that the world is inherently rational or even mathematical, as the Pythagoreans thought, is only to postpone the question momentarily, as well as to overlook the problematic nature of those concepts. What gives rationality or mathematics an authoritative force? In any event, the critique of rationalism and mathematics in Chapter 5 undermines all such pretensions. Additionally, the work of the Nobel Prize-winning economists Simon, Daniel Kahneman and Vernon Smith on our cognitive limitations has undermined it at the empirical level. The fact that scientific laws do not have authoritative force has another important implication: the natural and social worlds are not, in principle, ultimately explicable.

If, on the other hand, one assumes that social laws are simply attempts at describing the way the social world is, the meaning to be ascribed to any such 'laws' based on unrealistic idealisations is problematic. While it might be interesting to some people to speculate about how people might behave if they were entirely economic beings, the value of such speculation and their 'tendency laws' to policy decision is far from certain when we all know that the assumption is false. Such idealisation is a highly reductionist strategy, with its origins in ancient Greek atomism, which attempts to reduce physical reality to fundamental and identical particles. One can complain justly that economic systems cannot be dissected in this fashion. Weber was not aware of the difficulties later theorists found in modelling interdependent complex systems. They are not open to this reductive strategy. Importantly, the idea that the factors left out can be added back in to form a more complete description—for example, the idea that economic analysis deals with 'tendency laws'—assumes that such entities are separable in the first place and are independent. They might not be if, for example, we are dealing with non-linear dynamic systems.[84] If they are not independent and it is improbable that they would be, such influences cannot simply be added together. Complex or non-linear dynamics could produce multiple possible solutions, while even very small changes in initial conditions could produce drastic changes in outcomes.

What this means is that human behaviour is not describable by simple deterministic models. Such reductionism has a systematic bias in that it ignores or over-simplifies the importance of the context of the system being studied.[85] This simplification of the context 'also often legislates higher-level systems out of existence or leaves no way of describing inter-systemic phenomena appropriately'.[86] Indeed, 'assumptions that appear benign at such an individual level may be dangerous over-simplifications when viewed from a higher level'.[87] This is, of course, what we find with Margaret Thatcher's claim that there is no such thing as society and with the methodological individualism practised by economics. It is this reductionism and methodological individualism that leads directly to the modelling of society as if it is based on self-serving individuals.

I have already drawn attention to the fact that this modelling is not a neutral strategy, and have expressed concern about the potential impact of such modelling on society itself. This concern leads me to question whether methodological individualism is a legitimate, albeit potentially dangerous, analytical strategy or simply a cloak disguising the ideological prejudices embedded in neoclassical economic analysis. If our study is intended to influence our policy decisions—and if there are reasons to believe that there are higher-level social structures that impact on our social problems—we are honour bound to study them. Additionally, the conclusions drawn from such simplifying assumptions could simply be the artefacts of those assumptions with little or no connection with the phenomena that we are supposed to be studying.

A further problem surrounds how to choose the ideal type—what is to sit within the system to be examined and what sits outside as the 'context'. This is hardly a normatively neutral exercise and it is a problem for which no persuasive answer has been given. A further and fundamental question surrounds whether such ideal types lead to generalisations that are, in fact, empirically falsifiable. Given what was said above, they certainly cannot be verified. We will return to this topic in Chapter 8 when we discuss the content of economics more directly. It is important in the interim to remember that while Weber was a positivist, he never intended these ideal types to be used as normative ideals. For Weber, any understanding of causation in the social disciplines is a result of 'an interpretative understanding of social action and involves an explanation of relevant antecedent phenomena as meaning-complexes'.[88] This seems a far cry from the deterministic, mechanical modelling of neoclassical economics.

In 1953, American economist Milton Friedman (1912–2006) offered a radical and highly influential new defence of economic idealisation in *The Methodology of Positive Economic*s. Friedman accepted that experience reflected the complex influences of numerous causes and therefore could support numerous interpretations. This is consistent with the position taken here. Friedman believed that it was possible to subject our policy beliefs to empirical test and that the role of economic theory was to provide a system of generalisations able to generate predictions that could be checked against experience. Because no decisive disproof was possible of any hypothesis, however, we should have confidence only in those hypotheses that survived many tests and performed consistently better than the alternatives. Friedman was interested only in empirically meaningful and testable hypotheses as an engine of analysis for the problem at hand, rather than as a description of reality. For Friedman, the reality of a hypothesis was simply irrelevant. He claims that 'the more significant the theory, the more unrealistic the assumptions…To be important a hypothesis must be descriptively false in its assumptions…the relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic", for they never are.'[89]

To most of us this seems transparent nonsense—a further *ad hoc* rationalisation to defend the indefensible. Perhaps, however, he was just confused, as claimed by leading institutional economist Geoffrey Hodgson[90] and applied economist Daniel Bromley.[91] Hodgson points out that this position is not only theoretically incoherent; it has not been adopted in practice.[92] Hodgson draws a distinction between different kinds of assumptions: negligibility, domain and heuristic. Negligibility is where some factor will have a negligible impact on the result; domain assumptions specify the domain in which the theory is applicable; and heuristic assumptions are simplifying assumptions made in the early stages of a theory to allow successive approximations. Hodgson argues that Friedman is talking about negligibility assumptions and that it is not true that such assumptions are descriptively false, only that they have a negligible influence on the phenomenon being explained and consequently can safely be disregarded. Clearly, the core assumptions of neoclassical economics are not ones leaving out factors that have a negligible influence, but are truly descriptively false. This does matter.

Of course, Friedman's claims could be defended on the instrumental ground that the truth of a theory is irrelevant and that all that matters is the accuracy of the resulting prediction. Surely, however, the objective of such studies is not simply to make predictions—desirable though that might be—but rather to provide credible explanations? This is the generally accepted position in the philosophy of science. This instrumental approach would eliminate explanation and falsification from science and that should be the end of the matter. Friedman does not apply this criterion consistently in his own work. Rather, he uses Popper's falsification criterion in the case of the maximisation hypothesis. He neglects, however, to provide any relevant evidence for his claims and asserts that a failure of critics to develop any coherent, self-consistent alternative provides evidence of the worth of the maximisation idea. In any event, it is extremely doubtful that this approach has, in fact, led to successful prediction. Indeed, given the complex nature of economic systems and the sensitivity of non-linear models to initial conditions, the very possibility of making reliable predictions is being undermined.

German philosopher Wilhelm Dilthey (1833–1911) goes further than Weber, contrasting *verstehen* with *erklären* and suggesting that natural and social reality are different kinds of reality requiring different investigative methods—a position more in line with the position taken here.[93] British sociologist Anthony Giddens describes this aspect of the social disciplines as a 'double hermeneutic':

> The theory-laden character of observation-statements in natural sciences entails that the meaning of scientific concepts is tied to the meaning of other terms in a theoretical network; moving between theories or paradigms involves hermeneutic tasks. The social sciences, however,

> imply not only this single level of hermeneutic problems, involved in the theoretical meta-language, but also a 'double hermeneutic', because social-scientific theories concern a 'pre-interpreted' world of lay meanings. There is a two-way connection between the language of social science, and ordinary language. The former cannot ignore the categories used by laymen in the practical organization of social life; but on the other hand, the concepts of social science may be taken over and applied by laymen as elements of their conduct. Rather than treating the latter as something to be avoided or minimised as far as possible, as inimical to the interests of 'prediction', we should understand it as integral to the subject–subject relation involved the social sciences.[94]

There has been a broad recognition that a proper understanding of the social disciplines requires an appreciation of the hermeneutical dimension of them. In fact, there is a convergence between the insights of the hermeneutical tradition and the insights derived from the pragmatism that is influencing the philosophy of science outlined above. As Gadamer tells us:

> When Aristotle, in the sixth book of the *Nichomachean Ethics*, distinguishes the manner of practical knowledge…from theoretical and technical knowledge, he expresses, in my opinion, one of the greatest truths by which the Greeks throw light on the 'scientific' mystification of modern society of specialisation. In addition, the scientific character of practical philosophy is, as far as I can see, the only methodological model for self-understanding of the human sciences if they are to be liberated from the spurious narrowing imposed by the model of the natural sciences.[95]

For Aristotle, there were three intellectual virtues: *epistēmē*, *phronēsis* and *techne*. As we have already seen, *epistēmē* is the kind of certain geometric knowledge to which the natural sciences aspire. *Phronēsis* is the kind of practical wisdom we all use in the expert social practice and moral judgements we make in day-to-day life; this was, for Aristotle, the most important of the intellectual virtues.[96] *Techne* is technical knowledge or technology. For Aristotle—as for Toulmin, Gadamer, Mary Hesse and Bent Flyvbjerg—it is *phronēsis* that provides the appropriate methodological model for the social sciences. From this perspective, the natural and social sciences are simply different intellectual ventures. In short, not only have the social sciences—including economics—not achieved practical success in providing certain predictive epistemic theory, they cannot in principle aspire to that certain geometric knowledge of *epistēmē*. Importantly, it is a view that denies that knowledge of human activity can ever be universal and context-independent in the same way as knowledge in the natural sciences. As Flyvbjerg—following Dreyfus—argues, 'a theory which makes possible explanation and prediction, requires that the concrete context of everyday human activity be excluded, but this very exclusion of context makes explanation and prediction impossible'.[97]

The actors in a concrete situation will not necessarily conceive of any action in the same way that any attempt at a context-free definition of a social action based on abstract rules or laws might do. Importantly, context-dependence does not imply a more complex form of determinism but an open, contingent relationship between context, action and interpretation. Consequently, it is not meaningful to speak of theory in the natural science sense in the social disciplines. We will return to the ontological consequences of context and openness for neoclassical economics in Chapter 8. This limitation of the disciplines is no real failing, as *epistēmē* in turn cannot provide the reflective analysis of values that is at the heart of political, economic and cultural life. In this spirit and consistent with Toulmin, Flyvbjerg calls for the social sciences to be restored to their classical position as practical intellectual activities, clarifying the problems, risks and possibilities involved in social and political praxis.

Bernstein and prominent American economist Deirdre McCloskey tell us that part of the problem arises from the English word 'science' and the distinctions we English speakers make between the natural sciences, the social sciences and the humanities. In contrast, in German, a distinction is made between the natural sciences and the moral sciences only. The consequence has been that English speakers tend to think of the social sciences as natural sciences concerned with individuals in their social relations, on the assumption that the social sciences differ in degree but not in kind from the natural sciences. In contrast, German speakers have a much greater tendency to think of the social disciplines as moral sciences, sharing essential characteristics with the humanities. McCloskey goes on to advocate the adoption of the word 'discipline' to describe these social investigations.

From this perspective, the sciences should be seen as a confederation of enterprises, with methods and patterns of explanation to meet their own distinct problems—not the varied parts of a single, comprehensive, 'unified science'.[98] The Platonic image of a single, formal type of knowledge is replaced by a picture of enterprises that are always in flux and whose methods of inquiry are adapted to the nature of the case. Importantly, the belief that we can start again by cutting ourselves off from inherited ideas is as illusory as is the hope for a comprehensive system of theories. The hope for certainty and clarity in theory has to be balanced with the impossibility of avoiding uncertainty and ambiguity in practice. We need to reappropriate the reasonable, tolerant, but neglected legacy of humanism more than we need to preserve the systematic, perfectionist legacy of the exact sciences. In particular, formal calculative rationality can no longer be the only measure of intellectual adequacy; one must also evaluate all practical matters by their human 'reasonableness'. Consequently, for Toulmin:

> [T]he charms of logical rigour must now be unlearned. The task is not to build new, more comprehensive systems of theory with universal and timeless relevance, but to limit the scope of even the best-framed theories, and fight the intellectual reductionism that became entrenched during the ascendancy of rationalism. It calls for more subdisciplinary, transdisciplinary, and multidisciplinary reasoning.[99]

In particular, Toulmin believes that biology provides less constricting analogies for thinking about social relations than does physics. In the organic world, diversity and differentiation are the rule and not the exception. The universality of physical theories is rare. In this spirit, Wimsatt recommends that, in studying human behaviour, we should use a variety of models and approaches in the hope that we can thereby detect and correct for biases, special assumptions and the artefacts of any one approach.[100] This perspective sees science as being like other human investigations employing a variety of heuristics that, while not guaranteeing success, is the best we can do. A reductionist, mathematical deductive heuristic is only one possible approach to such modelling. While I am not rejecting the mechanistic modelling of neoclassical economics in its entirety, I am cautioning that it provides only one limited perspective, which could well be mistaken, and there might be more fruitful metaphors. Importantly, it is up to the advocates of reductionism and mathematical deduction to demonstrate its usefulness—particularly as a policy tool—to a justifiably sceptical audience. Furthermore, rather than clinging stubbornly to physics envy and the illusion of certainty provided by what appears to be a degenerating research strategy, economists should learn to embrace pluralism for the richness of the insights it can provide. In particular, economists should open themselves more fully to the possibility of explanation at various levels of organisational complexity throughout the economic system and not stick stubbornly to a reductionist story.

It is within such a framework that American anthropologist Barbara Frankel—drawing on Bateson and Mead—suggests that it might not be forces and objects that are central to human action but rather the information and messages that define the social context and order behaviour within those contexts.[101] She argues, in particular, that there is a danger of confusing biological individuals with social persons—leading to an inability to deal conceptually with contexts and meanings—as opposed to objects and forces. She suggests, therefore, that it might be more appropriate to consider the selves studied by the social disciplines as the sum of an individual's achieved and ascribed social roles, as nodes in a network of communications, avoiding the distraction of biological boundaries. Consequently, she suggests that we need to take seriously 'the notion of social persons created by and existing only within systems of interaction, and as bounded, not by the skins of biological individuals, but by contextual boundaries that may be…of indefinite extent'.[102]

One consequence of the positivist approach to the social sciences and the associated attempt to appropriate the prestige associated with the natural sciences has been to suppress political and moral discourse, to confer a privileged position on the *status quo* and on the professional expert with a capacity for judgement based on the unsustainable claim to technical expertise, neutrality and impartiality. All of this should lead us to be wary—as leading American legal theorist Grant Gilmore (1910–82) advises—of abstract and impersonal values, of universal solutions and of logical imperatives[103] within economics, the law and social life more generally. We should also be wary of grand theory, sacred rules and mystical absolutes that have little connection to reality—especially since we have been taught to be wary of such claims in our spiritual life. As legal historian Morton Horwitz confirms, the belief in the explanatory possibility of general laws capable of making predictive statements in the social sciences has plummeted: 'The result has been a dramatic turn towards highly specific "thick description" in which narrative and stories purport to substitute for traditional general theories…a complex, multi-factored interdependent world has lost confidence in single-factor "chains of causation" that were embedded in most nineteenth-century explanatory theories.'[104]

In this spirit, English economist Edward Fullbrook recently argued for pluralism among the knowledge narratives with which we organised and interpreted experience, each of which would offer a different view of the object of inquiry.[105] This is because all representations—even the most sophisticated and comprehensive of scientific narratives—involve a radical, stylised and somewhat arbitrary simplification of reality, a choice among an infinite number of possible perspectives or conceptual frameworks. Such a choice rests ultimately on the explanatory usefulness of the narrative and the entities it connects. Fullbrook cites American-born quantum physicist David Bohm in support:

> What is called for is not an *integration* of thought, or a kind of imposed unity, for any such imposed view would itself be merely another fragment. Rather, all our different ways of thinking are to be considered as different ways of looking at the one reality, each with some domain in which it is clear and adequate. One may indeed compare a theory to a particular view of some object. Each view gives an appearance of the object in some aspect. The whole object is not perceived in any one view but, rather, it is grasped only *implicitly* as that single reality which is shown in all these views.[106]

Any such perspective brings with it a system for classifying the empirical domain, which in turn limits possible descriptions, possible facts, possible questions and possible stories and thus uniquely circumscribes our possible understanding of reality. In particular, the meaning of any concept depends on the framework within which it appears. Viewing a domain from a new perspective brings with it the possibility of new dimensions of understanding. Fullbrook goes on to distinguish between closed narratives such as those of Newtonian mechanics and neoclassical economics and open narratives such as those of evolution, which admit indeterminacy arising from chance, contingency, choice, uncertainty, randomness and spontaneity. In the process, he challenges the hegemony of such closed narratives and the hostility they exhibit to 'alien' and open narratives. Rather, he argues that a plurality of narratives enriches our understanding and is essential to the advancement of knowledge.

It might be thought that a rejection of the search for general criteria for judging theories poses problems when it comes to judging economic theories.[107] The undermining of the pretensions of science should not, however, detract us from the task of reasonable judgement in research. It seems that we just have to learn to live with the understanding that all knowledge is a social and linguistic construct, and that this applies with particular force to the social disciplines. Recognition of these difficulties does not justify the proposition that empirical tests are unnecessary. The fact that we are unable to guarantee the truth of a proposition—fulfilling utopian demands of the rationalists—does not absolve us from attempting to develop the best methods we can, even in the absence of an absolute criteria for 'best'. In particular, it provides no excuse for a failure to take falsification seriously or to subject our theoretical speculative narratives to serious examination. On the contrary, it should provide a good reason to take these tasks and narrative pluralism much more seriously. The awareness of the limitations of one's tools and how best to use them does not provide an excuse for using them badly—or not using them at all—but rather points to the need to develop the ability to employ them skilfully and honestly. Nor does it license a sloppy use of statistical inference within economic research—a practice that McCloskey has documented.

Of course, it is a standard critique of neoclassical economics that it has abandoned realistic assumptions and has insulated its core beliefs from empirical testing, and does not meet the canons of any reasonable methodology. We will discuss these implications for economics in greater detail in Chapter 8.

## Summary

Summing up, it can be concluded that the claim that science has a privileged epistemological status in virtue of its empirical basis cannot be sustained. Rather, scientific and other inquiry uses much the same approach—a conclusion that applies in particular to normative reasoning. Against this background, the next chapter will turn to the examination of two interrelated issues—the distinction that has been made between positive and normative theorising, and the status of economics—before going on to consider the suggestion that economists should study moral philosophy.

## ENDNOTES

[1] Medieval saying attributed to Aristotle, apparently derived from a passage in the *Nicomachean Ethics*.
[2] Oakeshott 1962.
[3] Rorty 1991.
[4] Kincaid 1996.
[5] Levine 1986.
[6] Polanyi 1946, p. 34.
[7] Nagel 1961.
[8] Winch 1970.
[9] Secord 1986.
[10] Davies 2004b.
[11] Davies 2007.
[12] Turner 1987.
[13] Swartz 1995.
[14] Blaug 1980. This story draws heavily on Blaug's account, supplemented by the other references cited.
[15] Webb 1995.
[16] Crotty 1998.
[17] Webb 1995.
[18] Kincaid 1996.
[19] Fiske 1986, p. 2.
[20] Fiske and Shweder 1986.
[21] Blaug 1980.
[22] Kincaid 1996.
[23] Crotty 1998, p. 64.
[24] Ibid.
[25] Ibid.
[26] Kincaid 1996.
[27] Bernstein 1983.
[28] Bernstein 1983, pp. 24–5.
[29] Hanson 1958.
[30] Popper 1959, pp. 278–81.
[31] Ibid., p. 111.
[32] Blaug 1998.
[33] Kuhn 1970.
[34] Crotty 1998.
[35] Kuhn 1977, p. 234.
[36] Rorty 1980.
[37] Bernstein 1983.
[38] Kuhn 1970.
[39] Bernstein 1983.
[40] Ibid.
[41] Cited in Bernstein 1983, p. 57.
[42] Blaug's account emphasises the extent to which Kuhn backed away from his earlier position, but this is not a shared view.
[43] Feyerabend 1993.

[44] Shweder 1986, p. 172.
[45] Crotty 1998.
[46] Campbell 1986.
[47] Wimsatt 1986.
[48] Rorty 1980, p. 170.
[49] Webb 1995.
[50] Rorty 1980.
[51] Ibid.
[52] Ibid.
[53] Phillips 1986.
[54] Peirce 1934.
[55] Mead 1934.
[56] Gadamer 1975a, p. 312.
[57] Ibid., p. 316.
[58] Albert 1987, p. 79.
[59] Secord 1986.
[60] Richter 1986.
[61] Wimsatt 1986.
[62] Outhwaite 1987.
[63] Schostak 2002.
[64] Ibid., pp. 2–3.
[65] Ibid.
[66] Kincaid 1996.
[67] Fiske and Shweder 1986, pp. 369–70.
[68] Kincaid 1996.
[69] Ibid., p. xv.
[70] Winch 1958.
[71] Bernstein 1983.
[72] Wimsatt 1986.
[73] Secord 1986.
[74] Ibid., p. 215.
[75] Gergen 1986.
[76] Shweder 1986.
[77] Fiske and Shweder 1986.
[78] Nagel 1961, p. 459.
[79] D'Andrade 1986.
[80] Converse 1986.
[81] Fiske and Shweder 1986.
[82] Fiske 1986.
[83] Fiske and Shweder 1986.
[84] Richter 1986.
[85] Wimsatt 1986.
[86] Ibid., pp. 301–2.
[87] Ibid., p. 305.
[88] Crotty 1998, p. 69.
[89] Friedman 1966, p. 14.
[90] Hodgson 1988.
[91] Bromley 1990.
[92] Hodgson 1988.
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