The Killing of History

How a Discipline is Being Murdered
By Literary Critics and
Social Theorists

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Chapter 7

History as a Social Science

RELATIVISM, HERMENEUTICS AND INDUCTION

HISTORY is a discipline that straddles both the humanities and the social sciences. History's credentials as a science derive from three of its objectives: first, it aims to record the truth about what happened in the past; second, it aims to build a body of knowledge about the past; third, it aims to study the past through a disciplined methodology, using techniques and sources that are accessible to others in the field. The claim that history is a science is a highly contested issue, which calls for justification rather than mere assertion, and so later sections of this chapter discuss the scientific status of the methodologies employed by historians. To start, however, let us focus on the issues of truth and knowledge. The study of history is essentially a search for the truth. Without a claim to be pursuing truth, writing history would be indistinguishable in principle from writing a novel about the past. A work that does not aim at truth may be many things but not a work of history. Historical knowledge can either be discovered, by finding evidence that provides new revelations, or can be synthesised, by ordering what is already known in a way that provides a new perspective on events of the past. Either way, historians long believed they were engaged in an enterprise that had some claim to be adding to the knowledge produced by others, by making new discoveries, and by seeing things from different angles, even in the act of criticising and overturning other claims. No historian ever started on a topic completely from scratch. Until recently, all acknowledged they relied to some extent on those who had gone before them and all assumed they were, in [begin page 186] turn, contributing to an accumulating body of knowledge that would be drawn upon by others.

In the academic environment of today, however, the pursuit of truth and the accumulation of knowledge have become highly questionable endeavours. One of the reasons that the nihilism of French radical theory has been able to gain such a grip on the study of human affairs is because there is now widespread scepticism about the concepts of truth and knowledge. Many academics believe that neither the social sciences nor even the natural sciences can provide us with any kind of certitude. The fashionable and some say the now-dominant view, is that knowledge can never be absolute and there can be no universal truths. Let me quote a random, but representative, selection of recent statements from academics in both the humanities and the social sciences about the concepts of science, knowledge and truth. Anthony Giddens, Professor of Sociology at the University of Cambridge, and one of Britain's most influential social theorists has written:

> In science, *nothing* is certain, and nothing can be proved, even if scientific endeavour provides us with the most dependable information about the world to which we can aspire. In the heart of the world of hard science, modernity floats free.[1]

The feminist historian Professor Ann Curthoys, of the University of Technology, Sydney, has claimed:

> Most academics in the humanities and social sciences, and as far as I know in the physical and natural sciences as well, now reject positivist concepts of knowledge, the notion that one can objectively know the facts. The processes of knowing, and the production of an object that is known, are seen as intertwined. Many take this even further, and argue that knowledge is entirely an effect of power, that we can no longer have any concept of truth at all.[2]

The literary critic Dr Harry Oldmeadow, of La Trobe University, Victoria, while making a trenchant criticism of postmodernist theory's rejection of traditional values, nonetheless accepts its critique of truth.

> The epistemological objections to the liberal ideal of a disinterested pursuit of truth are more difficult to counter. The positivist rubric of 'objectivity' is now quite rightly in tatters: Kuhn, Rorty and others have shown how the apparently objective basis of the scientific disciplines themselves is illusory (never mind the more absurd pretensions of a positivist sociology or a behaviourist psychology.)[3]

As the last two quotations underline, the most pejorative insult to hurl in today's academic climate is the label 'positivist'. This term refers to a movement in philosophy that began in the nineteenth century and that,

under the name adopted by its pre-war Viennese adherents, 'logical positivism', reached its greatest influence in the English-speaking world in the 1950s and early [begin page 187] 1960s. During the Vietnam War, positivism became identified with the political Right because some of the leading positivists at the time were outspoken supporters of American intervention (even though others, such as Bertrand Russell, were equally well-known opponents of both the war and the nuclear arms race). Positivism is but one of a number of philosophical analyses that have supported and justified the scientific method that rose to prominence in Europe in the seventeenth and eighteenth centuries in the period now known as the Enlightenment. This scientific method, based on drawing conclusions from empirical observation and experiment, provided the apparatus for all our subsequent knowledge of the physical and biological worlds and has been the engine of the industrial and technological societies that have emerged in the train of this knowledge. When postmodernists and their fellow travellers write dismissively of 'positivist illusions' their real target is the claim of the empirical methods of science to provide the path to knowledge. Scientific method, in their view, has no universal validity; it is just another transitory product of an era that is now rapidly fading.

Those readers who have not followed the debates inside academia over recent years about the status of knowledge and science may find all this odd, to say the least. After all, most of the educated population today attribute the enormous explosion of knowledge of the last three hundred years to the methods of empirical science. It has freed our culture from the shackles of superstition, mysticism and quackery, and it appears, indeed, still to be taken for granted by most intelligent people in the world at large. Unfortunately, within many schools of humanities and social sciences today such views are few and far between. As a result, this chapter needs to make a longish diversion from the book's principal focus to examine the current status of scientific knowledge. For if the fashionable view is correct, and truth and knowledge are really beyond our reach, then we might as well give history away altogether. The debate on this issue has taken place in no less than three separate forums: the sociology of science, the philosophy of scientific method and the field of hermeneutics.

THE SOCIOLOGY OF SCIENCE

One of the major figures responsible for the current levels of doubt about scientific knowledge is the American author Thomas Kuhn, whose very influential book, *The Structure of Scientific Revolutions*,[4] has been in print continuously since it was first published in 1962. Kuhn is responsible for introducing the concept of the scientific 'paradigm' to provide a sociological explanation of how changes in scientific opinion and methods come about. Kuhn uses this term in his account of how a widely accepted scientific [begin page 188] framework is overthrown and replaced by another. He distinguishes three phases in the life of any body of science. The first is the pre-science phase in which a range of unstructured and uncoordinated activities take place. If these activities are taken up and organised by what he calls a 'scientific community', that community adheres to a 'paradigm'. A paradigm is made up of the range of techniques, assumptions and theories that the members of the community work with in pursuit of their science. While they are working within the framework of the paradigm, they practise what Kuhn calls normal science, which is the second of the phases he identifies. Kuhn says that normal science is characterised by periods of calm and steady development dominated by one accepted set of concepts. The third phase is composed of a crisis within the science, which produces a period of radical change when the ruling paradigm is overthrown by another. This constitutes a paradigm shift or a scientific revolution. The most well-known examples are the overthrow of Ptolemaic astronomy by that of Copernicus, or the replacement of Newton's mechanics by Einstein's theory of relativity. Crises recur because any existing paradigm is almost always subject to anomalies, that is, observations that are difficult to explain or reconcile with the central doctrine. At first these might appear marginal, but they gradually accumulate to the point where the scientific community eventually loses faith in the existing paradigm. The door is then opened for a scientific revolution to occur which will establish a new paradigm that explains both the former body of data and the inconsistencies that the old paradigm could not handle. A new period of normal science then continues until it, again, is subject to its own crisis and revolution.

The Kuhn thesis is a radical challenge to familiar notions of science, especially the idea that our knowledge of nature has gradually built up over time. Kuhn replaces the picture of a cumulative and progressive model of growth with a discontinuous and revolutionary process of change. For

example, Einstein's theory of relativity did not add a new increment of knowledge to the secure truth of Newton's theory of gravitation but overthrew it completely. However, because Einstein's theories are themselves destined ultimately for the same fate, as would be the paradigm of any successor of Einstein, the Kuhn thesis is committed to denying the possibility of any scientific knowledge at all, in the normal sense of 'knowledge' implying truth and certitude. Kuhn eschews talk of 'truth' and 'falsity' in science and insists that the beliefs of scientists are all 'paradigm-relative', that is, they make sense within their own intellectual environment but not in others.

Kuhn also argues for what he calls the *incommensurability* of scientific theories. New paradigms may borrow some of the vocabulary and apparatus of the old, but they seldom use these borrowed elements in the same way. Different paradigms operate with different concepts, sometimes changing the meaning [begin page 189] of old terms, and they have different standards of acceptable evidence, as well as different means of theorising about their subject matter.

> Consider … the men who called Copernicus mad because he proclaimed that the earth moved. They were not either just wrong or quite wrong. Part of what they meant by 'earth' was fixed position. Their earth, at least, could not be moved.[5]

Overall, Kuhn's thesis on incommensurability argues, there is no common measure for the merits of competing theories, nor any common agreement about what constitutes either a scientific problem or a satisfactory scientific explanation. Hence, there is no way of ranking scientific theories and thus there are no grounds for arguing that science is progressive. Einstein is not superior to Newton, just different.

Kuhn insists that, although a paradigm has to be supported by compelling evidence and arguments in its favour, it is never accepted for purely objective reasons; rather, it gains acceptance because a consensus of opinion within a scientific community agrees to use it. He says the issue is not decided by purely logical argument but is more like sudden conversion or a 'gestalt switch'.[6] The factors that lead scientists to change their allegiance to paradigms, he argues, need to be explained in terms of the scientists' values and the personal relations within a scientific community. 'Paradigm choice can never be unequivocally settled by logic and experiment alone.'[7] Following Kuhn, a bevy of sociologists have entered the field to take up what they see as one of the most enticing consequences of his position: the idea that what is believed in science is determined by the customs and power relations prevailing within a particular scientific community.

One of these sociologists, David Bloor, has gone so far as to suggest that the content and nature of science can be explained through the methods of the sociology of knowledge, and that scientists accept scientific laws primarily for reasons of justification, legitimation and control.[8] Another, H. M. Collins, has made a radical critique of the concept of scientific experiment. All experiments, Collins claims, are subject to 'experimenter's regress'. This argument contends that experiments cannot perform the function that scientists claim for them, that is, to independently assess the success or otherwise of competing scientific theories. This is because the theories themselves determine what counts as an effective experiment. Hence, there are no objective criteria that can be derived from experiments to separate the outcome of the experiment from the theory it has been designed to test. After interviewing scientists about the reasons why they accept or reject experimental results published by other scientists, Collins concluded that they were strongly influenced by such things as the size and prestige of the university where the experiment was done; the personality, nationality and reputation of the scientist; whether the experiment was performed within a university or within industry; and the [begin page 190] way the results were presented. 'It is not the regularity of the world that imposes itself on our senses', Collins writes, 'but the regularity of our institutionalised belief that imposes itself on the world'.[9]

Many of the conclusions of Kuhn and his followers have parallels in French radical theory. In particular, Michel Foucault's version of the history of ideas follows the concepts of Kuhn very closely. Kuhn's notion of the 'paradigm' is the model for Foucault's more encompassing but still very similar 'episteme'. Kuhn's argument that consensual custom and power determine what is accepted as scientific truth is almost identical to Foucault's claim that truth is established by intellectual power groups. In fact, it is highly likely that, just as Foucault constructed his thesis on institutions by borrowing without acknowledgement the ideas of the American sociologist Erving Goffman (as shown in Chapter Five), Foucault did much the same with Kuhn's book to produce his history of ideas. Whatever is the case, though, it is clear that the work of Kuhn, and its subsequent popularity among sociologists and their students, helped pave the way for the acceptance of French radicalism and for the prevailing derision about the claims of historians or anyone else to be pursuing the truth and producing knowledge.

THE PHILOSOPHY OF SCIENTIFIC METHOD

At the same time as Kuhn's essentially sociological analysis was made, the philosophy of scientific method had arrived, by another route, at a similar set of conclusions. By the early 1960s, the Viennese-born, English-based philosopher Karl Popper was widely regarded as having solved one of the most vexatious problems in philosophy: the justification of empirical scientific method. Popper devised, and from the 1930s to the 1980s was the leading advocate of, the principle of 'falsifiability'. He argued against the view accepted by most scientists that evidence was used to verify scientific theories. The traditional scientific method of induction has held, since the writings of Francis Bacon in the early seventeenth century, that we gain scientific knowledge by generalising from our observations. Popper claimed, however, that the proper role of evidence is to falsify scientific conjectures. Thus, instead of the traditional view that a scientific theory is *verifiable* by observation, Popper contended that a scientific theory is one that is *falsifiable*. Theories, Popper said, are not the kind of things that can be established as being conclusively true in light of observation or experiment. Rather, theories are speculations, guesses or conjectures about some aspect of the world or the cosmos. The role of observation and experiment is to rigorously test these theoretical conjectures and to eliminate those that fail to stand up to the tests that are applied. Science advances by trial and error, with observation and experiment progressively [begin page 191] eliminating unsound theories so that only the fittest survive. As the title of one of Popper's best-known books describes it, scientific method is a problem of 'conjectures and refutations'[10] in which we learn not by our experience but by our mistakes.

Falsifiability appeared to solve a number of the philosophical problems surrounding the scientific method of induction in which evidence comes first and theory later. Following the critique made by the eighteenth century Scottish philosopher David Hume, it appeared that the method of induction is fatally flawed, Inductive arguements, Hume argued, are logically invalid. Against traditional, inductive scientific method, which held that, after repeated observations of A being B, and no observations of the opposite, one is justified in claiming that all As are B, Hume argued that such inductive arguments are invalid because it is always open to the possibility that the next A we find will not be a B. Given the remarkable success that science and technology enjoyed in the eighteenth and nineteenth centuries, Hume's argument was quietly ignored by most philosophers. In the twentieth century, however, when some of the old certainties, especially those of Newton's physics, were overthrown, a new generation headed by Popper argued that Hume had been right all along. Popper accepted that universal arguments such as all As are B are *un*provable. However, he added that they remain in principle *dis*provable. If we find just one A that is not a B then we can be certain that the theory that all As are B is false. The very mark of a scientific theory is that it can be disproved by experience, he said, and the more disprovable it is compared with its rivals, then the better than them it is.

Falsifiability also appeared to avoid another critique that has been made of induction: the theory-dependence of observation. Scientific induction assumes that the observatins of the world that go towards constructing a scientific theory are themselves objective and theoretically neutral. However, critics have countered that theory of some kind must precede all observation statements. One of Popper's former students the Sydney philosospher, Alan Chalmers, has claimed: 'Observation statements must be made in the language of some theory, however vague.' As well as some scientific examples, Chalmers gives the commonsense instance: 'Look out, the wind is blowing the baby's pram over the cliff edge!' and says that much low-level theory is presupposed even here. 'It is implied that there is such a thing as wind, which had the property of being able to cause the motion of objects such as prams which stand in its path.'[11] Falsifiability avoids such a critique because it freely admits that observation is guided by and presupposes theory. The aim of falsification is to start not with observations but with theories themselves.

Popper's approach, then, appeared to have much to recommend it. He abandoned the logically problematic claim that evidence counted *towards* the [begin page 192] acceptance of a theory and, instead, argued that evidence only counted against a theory. For example, the argument 'A black swan was observed in Australia; therefore, not all swans are white' is a logically valid deduction. The falsity of a universal statement can be demonstrated by an appropriate singular statement. Moreover, the falsifiability criterion had the added advantage of readily identifying certain kinds of

statements as non-scientific. For example, a logically necessary statement such as 'A father is a male parent' is not a scientific statement because it is true by virtue of the meaning of the term used and is thus unfalsifiable. Similarly, many statements deriving from religion, mysticism or metaphysics could be labelled unscientific because there is no evidence that could be brought to bear to falsify them. Hence, Popper had an argument that seemed to have three advantages: it solved the problem of induction, it was an empirical not a metaphysical approach and it defended science. For these reasons, falsificationism won widespread support in the 1960s from both philosophers and scientists themselves.

Falsificationism gained this acceptance despite the fact that it introduced a large element of uncertainty into the notion of science. It said that a scientific theory holds until it is disproved and that science advances by a process of elimination. However, Popper agreed that no matter how many searches fail to find a negative instance to falsify a theory, this can never provide grounds for thinking the theory has been conclusively established. Hence, on the falsifiability principle, no scientific theory can ever be conclusive. It remains forever a conjecture or a hypothesis. So we can never have sufficient grounds for gaining from science anything as concrete as 'knowledge' in the usual sense of that word. We get good theories (those that are falsifiable but not yet falsified) and bad theories (those that have been falsified or that are unfalsifiable) but nothing more definite than this. In the 1990s, social scientists such as Anthony Giddens still cite Popper's early work as the basis for their belief that nothing can be proved and that nothing is certain.[12]

Despite its wide acceptance, Popper's theory was subject to some searching criticisms from the outset. Thomas Kuhn argued that Popper's approach was little different from the verification theory that it was designed to replace. All scientific theories are accompanied by anomalies that they find difficult to explain, Kuhn pointed out. These are rarely regarded as falsifications, but, rather, are seen as 'the incompleteness and imperfection of the existing data-theory fit that, at any time, define many of the puzzles that characterise normal science'. If these anomalous observations are regarded as so powerful as to overturn an existing theory, they act as something that 'might equally well be called verification' for the newly emerging paradigm in the field. Kuhn said he doubted if outright falsifications ever existed.[13] Other critics pointed out that Popper had not removed the major difficulties inherent in the observa- [begin page 193] tion process. Observations that refute theories had no higher a level of reliability than observations that confirmed them. Scientists, moreover, will often reject an apparently falsifying observation in order to retain a theory. For example, supporters of Copernicus's theory that the planets orbited the Sun found it difficult to account for naked-eye observations that the apparent size of Venus did not change throughout the year, as the theory said it should, yet they stuck to the theory anyway.[14] If they had followed Popper, they would have been forced to agree that this observation amounted to a falsification of the theory that the planets orbit the Sun. It took another seventy years before better observation technology was developed that showed that the apparent size of Venus did change and that Copernicus was right.

Popper's most influential supporter was the Hungarian-born philosopher Imre Lakatos, who succeeded him as Professor of Logic and Scientific Method at the University of London. Although a critic of a number of aspects of the way in which Popper had formulated his falsification thesis, Lakatos tried to improve upon, and overcome objections to, the doctrine. Lakatos was also a critic of Kuhn, but nonetheless adopted some elements of the sociological approach into his work. Lakatos argued that any description of science cannot be confined to statements of laws or singular observations. He added that simple falsifications are rarely fatal to a scientific theory. Scientific endeavour has to be regarded as a 'research program'. This is a structure or framework that provides guidance for future research in a way similar to Kuhn's 'paradigm'. A research program has a hard core of general theoretical hypotheses from which the future research of the program can develop. For example, the hard core hypotheses of Copernican astronomy are that the Sun is the centre of the solar system, that the Sun remains stationary while Earth and the other planets orbit it, and that Earth spins on its axis once every day. In the early stages of a research program, Lakatos said, there might be many observations that appear to falsify its core, but the program should not be rejected because of these alone. It needs time both to develop its potential and to see if it can answer or overcome what initially look like major stumbling blocks. In the case of the Copernican research program, it had to await technological developments — such as the invention of the telescope — and theoretical developments in related fields — such as Newton's theories of gravitation and motion — before it could be properly assessed. A good research program, according to Lakatos, is one that has a high degree of coherence, that has the potential to inspire a great deal of future research,

and that makes novel predictions that are eventually confirmed. Instead of falsification by observation, Lakatos substituted the contrast between a research program that is progressing and one that is degenerating. A degenerating (rather than falsified) research program is one which no longer makes novel predictions compared to a more [begin page 194] progressive rival. This is why the old Ptolemaic astronomy, which held that the Earth was stationary at the centre of the universe while the stars and planets circled it, was displaced by Copernican theory.[15]

Although it played down the concept of falsification and gave more weight to confirmation, the Lakatos reformulation left science with the same degree of epistemological uncertainty as Popper's account. How can one tell when a research program has deteriorated enough for its hard core assumptions to actually be disproved? How can one tell that a research program has really run out of novel predictions? How can one be sure that a rival new research program has long-term potential and is not just a flash in the pan? Lakatos had to concede that these were questions that can only be answered in hindsight and that, meanwhile, a high degree of uncertainty prevails. It always remains possible that a waning research program can be revived, as had happened many times in science when the assumptions of old and unfashionable theories were suddenly found to hold the answers to new questions. If this is true, no research program can ever be said, in principle, to be dead and buried and no rival can ever truly claim the field. Following Lakatos, science still stood on shifting sands.

FROM RELATIVISM TO ABSURDITY

According to Thomas Kuhn, the criteria used to assess whether a scientific theory is superior to its rivals are those upon which scientists themselves place most value: how it fits the facts better; how it makes the better predictions; how it has the ability to solve more problems; as well as its aesthetic appeal, that is, its simplicity and neatness. Kuhn argues that 'the importance of aesthetic considerations can often be decisive'.

> Though they often attract only a few scientists to a new theory, it is upon those few that its ultimate triumph may depend. If they had not quickly taken it up for highly individual reasons, the new candidate for paradigm might never have been sufficiently developed to attract the allegiance of the scientific community as a whole.[16]

In other words, the value system and tastes of the scientific community are decisive factors. Kuhn is quite specific about this. 'As in political revolutions, so in paradigm choice — there is no standard higher than the assent of the relevant community.' Individual scientists embrace a new paradigm for more than one reason, and usually for several at once. 'Even the nationality and prior reputation of the innovator and his teachers can sometimes play a significant role.'[17]

Since Kuhn acknowledges that the values and standards that prevail within a scientific community vary considerably, depending on the cultural and his- [begin page 195] torical background of the time, this means that in Kuhn's account there can be no universal standard by which to assess a scientific theory. In other words, Kuhn's position is a relativist one — a successful scientific theory is one that is judged so by its peers, relative to their own values, culture and taste. This is a point that Lakatos used to make a trenchant critique of Kuhn. Lakatos said that the relativist position, in which there is no standard higher than that of the relevant community, leaves no room for any way of judging that standard. If what counts is the number, faith and persuasive energy of its supporters, then 'truth lies in power', acceptance of scientific change is no better than 'mob psychology' and scientific progress is merely a 'bandwagon effect'.[18] Without an independent or rational guide to assessing theories, the acceptance of new theories was no better than religious conversion.

Though Kuhn himself has attempted to deny the charge of relativism, there is little doubt in the minds of later commentators on his thesis not only that the charge is correct but that Lakatos himself, through the sociological elements used in his thesis on research programs, is in the same position. One of those who makes this point is Paul Feyerabend, a Viennese-born former student of Popper and Lakatos who spent most of his academic career as a professor of philosophy at the University of California at Berkeley. Feyerabend has pushed the argument from the sociology of science to its furthest conclusion. He argues that the history of scientific research is in itself testimony against the universal validity of any rules to judge the correctness of a scientific theory. Since there are not, nor ever have been, any such universal rules, Feyerabend says Kuhn's theses about paradigms and Lakatos's theses about research programs both share the same, relativist status.[19]

Feyerabend has taken Kuhn's notion of the incommensurability of scientific theories and used it to argue some extraordinary conclusions. Rival scientific theories can be so different from one another, Feyerabend contends, that the basic concepts of one cannot be expressed in terms of the other and that what counts as an observation in one does not count as an observation in the other. He gives the example of classical mechanics and relativity theory. In classical mechanics, physical objects have shape, mass and volume. In relativity, properties of shape, mass and volume no longer exist. This means, Feyerabend says, that any observation about physical objects within classical mechanics has a meaning different from an observation within relativity theory. 'The new conceptual system that arises (within relativity theory) does not just *deny* the existence of classical states of affairs, it does not even permit us to formulate *statements* expressing such states of affairs. It does not and cannot share a single statement with its predecessor… '[20] Given the degree of incommensurability that Feyerabend sees between these two theories and a number of others he compares, he concludes that that there are no 'rational' or 'objec- [begin page 196] tive' grounds on which to choose between rival theories. Theory choice is essentially subjective. It is strongly influenced by propaganda and is made on the basis of 'aesthetic judgements, judgements of taste, metaphysical prejudices, religious desires … *our subjective wishes*'. Hence, he asserts, a fairer way to decide the merits of scientific theories would be to put them to the vote.[21]

Feyerabend applies the incommensurability principle not only to rival scientific theories but to the whole of science itself as compared with other fields that claim to understand the world. Because they, too, are incommensurable, he asserts there can be no argument in favour of science over other forms of understanding. He compares science with astrology and voodoo and claims that there is no general criterion that gives scientific knowledge priority over the latter. Hence, he argues, it is wrong to teach science to school children as if it had a monopoly on wisdom. Non-scientific ways of viewing the world deserve the same kind of attention. The grip that the ideology of science has on government policy deserves to be broken, he says, in the same way that secular educationalists last century broke the nexus between Church and state. This would clear the way for other approaches, such as magic, to be taught instead of science. 'Thus, while an American can now choose the religion he likes, he is still not permitted to demand that his children learn magic rather than science at school. There is a separation between state and Church, there is no separation between state and science.'[22] In Feyerabend's view, science should be studied not as some holy writ but as an historical phenomenon 'together with other fairy tales such as the myths of "primitive" societies'.[23] Consistent with this line, Feyerabend has defended Christian fundamentalists who want to have the biblical version of creation taught in American schools alongside Darwin's theory of evolution.[24]

Feyerabend's deliberately outrageous and attention-seeking epigrams might seem to many readers to put him in a different category from the other three authors discussed here. Certainly, in terms of reputation, his openly irrationalist and 'anarchist' position is seen by most scientists and philosophers as markedly different from that of Kuhn, Popper and Lakatos. For the past thirty years, the latter trio have been widely regarded as having provided the most plausible account of scientific activity available. Yet it should be apparent from the account given above that Feyerabend's views start from the central points made by Kuhn's thesis. Feyerabend himself argues persuasively that the philosophy of science of Lakatos is different from his own in words only, and not in substance.[25] Kuhn has done a similar job to show the great affinity between his own ideas and those of Popper.[26] Later writers about this debate, such as the Sydney philosopher Alan Chalmers, whose book *What Is This Thing Called Science?* has become a best-selling commentary, agree that [begin page 197] Feyerabend's views are a logical conclusion of the premises established by the other three. Let us consider some of the implications of this.

What we are looking at is a school of thought — a paradigm, if you like, in its own right — that contains a number of implicit conclusions that are absurd and that no rational person should accept. Since it believes that knowledge is not accumulative, this school is committed to denying that there has been a growth in knowledge since the sixteenth century. The idea that the Earth is flat and that the stars and planets circle the skies above it should not be regarded as wrong or false but, rather, as a set of statements from an older paradigm which is, incommensurable with the later one established by Copernicus, Kepler and Galileo. Moreover, these three scientists did not actually prove that the Earth and the other planets orbit the Sun, since nothing can ever be proved conclusively. They simply persuaded many people for the next three hundred years, for largely aesthetic and subjective reasons, that their own paradigm was worth accepting. And, of course, Einstein's theory of relativity was no advance on their earlier position either, it was just different. In the same vein, the

story that the world was made by God in seven days, that it is only about 4500 years old, and that the fossils of long extinct sea creatures found embedded in mountain peaks are nothing more than remnants of the great biblical forty-day flood, is not mistaken but is simply a set of statements that is incommensurable with later paradigms. Some of us might believe the earth is billions rather than thousands of years old but our grounds for this belief are not superior to those of religious fundamentalists. Similarly, astrologists, fortune tellers and faith healers are not misguided or dishonest, only different. Indeed, Paul Feyerabend will defend the rights of those who want to teach these beliefs to your children at school.

But surely, one might object, the theories of Karl Popper have been so widely accepted by such an eminent group of scientists — Popper records in his autobiography that he met both Albert Einstein and Nils Bohr at Princeton in the 1950s and, he says, they generally agreed with his views[27] — that he could not possibly be in the same camp as that anarchist Feyerabend? Surely Popper, who has repeatedly denied any connection with subjectivism and relativism, is not committed to a form of scepticism so deep that it cannot provide us with rational grounds for elevating science above magic and voodoo? To respond to these objections, let me bring the material in this chapter full circle with a discussion of Popper's views on the status of observation and knowledge in the field of history.

In *Conjectures and Refutations,* just after he has made a critique of Francis Bacon's methodology of induction, Popper turns to the issue of how we learn about what happened in society. He claims that if we try to establish the veracity of an observation of an event in society we are forced to ask questions [begin page 198] that themselves inevitably lead on to others, in a sequence that can never end. Popper calls this sequence an 'infinite regress'. He gives the example of an apparently innocuous statement by a newspaper: 'The Prime Minister has decided to return to London several days ahead of schedule.' How do you know that the statement is true? he asks. You answer that you read it in *The Times*. Popper says two questions follow from this. First, how can you be sure it was *The Times* and not something disguised as *The Times*? Second, how can you be sure that the newspaper got the information right? If you ignore the first bit of scepticism and follow the second trail, you might approach the editor of *The Times*. He would confirm that the paper had a telephone call from the Prime Minister's office. You can speak to the reporter who took the call, and you can ask him how he was sure that the call was genuine and that the voice really came from the Prime Minister's office. Popper claims that for every answer you get you can always ask another question.

> There is a simple reason why this tedious sequence of questions never comes to a satisfactory conclusion. It is this. Every witness must always make ample use, in his report, of his knowledge of persons, places, things, linguistic usages, social conventions, and so on. He cannot rely merely upon his eyes or ears, especially if his report is to be of use in justifying any assertion worth justifying. But this fact must of course always raise new questions as to the sources of those elements of his knowledge which are not immediately observational. This is why the program of tracing back all knowledge to its ultimate source in observation is logically impossible to carry through: it leads to an infinite regress.[28]

In the study of the past, this whole process is even more difficult, Popper continues, because we usually lack any eyewitnesses to the events we think occurred. Historians rely on documents and have learned that they can never accept them uncritically. 'There are problems of genuineness, there are problems of bias, and there are also problems such as the reconstruction of earlier sources.' Even those documents that purport to be the reports of eyewitnesses themselves nonetheless always provide grounds for doubt. As most lawyers know, he says, eyewitnesses often err. Even those most anxious to be accurate are prone to 'scores of mistakes', especially if the witness was excited at the time or became influenced just after the event by some 'tempting interpretation'. Even in the case of what most people would regard as an extremely familiar historical event — he uses the example of the assassination of Julius Caesar in the Roman Senate — the available observations such as the statements of eyewitnesses and spectators of the event, and the unanimous testimony of earlier historians are insufficient to avoid the 'infinite regress'. Hence, Popper concludes, those who believe that historical sources can be used to provide knowledge are entirely mistaken. What he calls 'the empiricist's ques- [begin page 199] tions', for example, 'How do you know? What is the source of your assertion?' are '*entirely misconceived*'.[29]

Now, this is an argument that is just as bizarre as anything to come out of Feyerabend. It not only rejects, in principle, the idea that historians can ever produce knowledge, but it is committed to being profoundly sceptical about our ability to know anything about what happens in society at any time. Let us take, for instance, Popper's case about the murder of Julius

Caesar and update it from the Ides of March 44 BC to 22 November 1963 and apply his very same logic to the assassination of John F. Kennedy. According to Popper, any eyewitnesses to that shooting must be regarded as unreliable because, as lawyers know, witnesses sometimes get things wrong. All the film footage of the event must be just as subject to the 'infinite regress' as his example of *The Times'* report about the Prime Minister's return to London. We could never ultimately establish that any of the film was authentic. Nor could we trust any of the journalists who wrote that they themselves saw Kennedy killed. They might have been overexcited at the time or tempted into rash interpretations by later events, such as the subsequent assumption of office by Lyndon Johnson or the emotionally charged funeral of the allegedly dead President. This doubt must extend even to the testimonies of Mrs Jackie Kennedy who cradled her dying husband in her arms, of Governor John Connally who was shot at the same time, and of the doctors who examined the President's body and pronounced him dead. According to Popper's theory, all these are mere 'observations' and, as such, are insufficient grounds for providing knowledge that the assassination took place. Let us be clear about this. Popper's position commits him to doubting not simply whether Kennedy was killed by Lee Harvey Oswald, or was shot by one gunman or several, but whether he was killed at all!

This is the same man, and the same book, that leading social scientists of the 1990s have relied upon to claim that nothing in our understanding of the social or natural worlds can be certain. Yet if his conclusion is as absurd as this, there are two questions that arise. First, since the argument has obviously gone wrong somewhere along the line, where has this actually occurred? Second, how could such nonsense have been taken seriously for so long, that is, what is there about it that has made it plausible? The next section examines these issues.

THE LOGIC OF SCIENTIFIC SCEPTICISM

The most incisive critic of the Popper—Kuhn—Lakatos—Feyerabend position is the Sydney philosopher David Stove. In his 1982 book, *Popper and After: Four Modern Irrationalists*, Stove provides a devastating analysis both of why their [begin page 200] case is wrong and why many people have found this difficult to recognise. As I noted above, the starting point for Popper, Lakatos and Feyerabend is the philosophy of science whereas for Kuhn it is the sociology or the history of science. As the field of the philosophy of science was traditionally conceived, it was concerned with the logical relations between scientific statements, that is, what could and what could not be legitimately transmitted from one statement to another. The sociology of science, on the other hand, is about scientific practice, what scientists do and what they think. The two fields are distinct areas of inquiry. The philosophy of science is *prescriptive* in that it aims to establish what relations must hold between statements. The sociology of science is *descriptive* in that it is simply an account of scientific activity, irrespective of the degree of logic that prevails within it.

David Stove has shown that one of the central problems in the recent debate is the conflation of the two fields. The philosophers Popper, Lakatos and Feyerabend all derive the evidence for their claims from the history of science. This would be acceptable if they used this evidence simply to provide examples of statements whose logic they were examining. Time and again, however, Stove shows them *appearing* to be making logical statements about the relations between scientific propositions, while *actually* making statements about what scientists believe or accept, that is, using the latter as if they were examples of the former. There is a constant, subtle elision from one kind of statement to the other. Their radical scepticism derives from their attempt to resolve questions of logical value by appealing to matters of historical fact.

One of the problems for Popper's philosophy of science that has long been recognised is the central issue on which it rests, the notion of falsification. It is often very difficult to tell if a theory has really been falsified by an observation. In the case of the proposition 'All swans are white', the observation of one black swan fairly readily counts as a falsification. But in the example of Copernicus's theory of planetary motion, the naked-eye observation that Venus appeared not to change size was, for seventy years, until the invention of the telescope, held to be a demonstration that the theory could not be true. The question of who is to judge whether a scientific theory has been falsified is one that, in practice, is naturally left to scientists. This is what Popper supports. In *The Logic of Scientific Discovery*, he writes that 'a physicist is usually quite well able to decide' when to consider a hypothesis as 'practically falsified'; and 'the physicist knows well enough when to regard a probability assumption as falsified'; and 'we shall no doubt abandon our estimate [of probability] in practice and regard it as falsified'.[30] Now, in the theory that all swans are white, the discovery of one non-white swan falsifies the theory, that is, refutes it, as a matter of logic. It does not matter how many or how few scientists

recognise this logic, the logical inconsistency remains. But in the [begin page 201] examples from Popper above, falsification is a matter of judgement by scientists, not logic. Physicists 'decide' when an assumption has been falsified; they 'regard' estimates as falsified. Stove uses these examples to argue that Popper is involved in a process of 'epistemic embedding', by which he means changing the logical status of a word or of statements by embedding them in a sociological context.

> They use a logical expression, one implying inconsistency, but they do not imply the inconsistency of any propositions at all. They are simply contingent truths about scientists. Yet at the same time there is a suggestion that not only is a logical statement, implying inconsistency, being made, but that one is being made with which no rational person would disagree.[31]

All of those who support this radical sceptical position, Stove argues, are guilty of using logical words but depriving them of their logical meaning by embedding them in epistemic context about scientists. Kuhn talks of arguments that appear to be decisive rather than being logically decisive: 'Ordinarily, it is only much later, after the new paradigm has been developed, accepted and exploited, that apparently decisive arguments [against the old paradigm] are developed.' And Lakatos often uses quotation marks to neutralise the logical force of a term: 'The anomalous behaviour of Mercury's perihelion was known for decades as one of the many as yet unsolved difficulties in Newton's program; but only the fact that Einstein's theory explained it better transformed a dull anomaly into a brilliant "refutation" of Newton's research program.'[32] Another tactic is the use of terms that confuse logical with causal relations. Lakatos, in particular, is prone to applying to scientific theories expressions such as 'is defeated', 'is eliminated', 'is removed' and 'is abandoned' as though these causal expressions were logical expressions like 'is falsified'. All he can really imply by these terms is that scientists have abandoned the theories concerned, not that they have been refuted in the logical sense of having been proven to be false.[33]

The conflation of statements from the philosophy and the sociology of science has been responsible for two of the great myths perpetrated by Popper and his followers: first, that all scientific findings of the past have been rendered irrelevant by the findings of later theories; and second, that scientific knowledge is never cumulative. By applying Stove's distinctions to the most common example used by the radical sceptics, we can put these myths in their place. Even though the Copernicus—Galileo—Kepler theory that the Earth and the planets orbit the Sun has now been replaced (a sociological concept) by far more sophisticated and adventurous Einsteinian theories of cosmology, the central findings of the seventeenth century thesis have not been refuted (a logical concept) by the newer theories. The planets still orbit the Sun, just as the scientists of the Renaissance discovered 350 years ago, and nothing can [begin page 202] alter the fundamentals of that piece of knowledge that those scientists discovered. Moreover, the history of science over the last four hundred years has been, overall, a story of the accumulation of knowledge. Even if we concede to Feyerabend that Einstein's theory does not share a single statement with its predecessor, this is not an argument against the accumulation of knowledge. Einstein, as a matter of historical fact, wrote his theory of relativity in response to Newton's mechanics. One might argue, indeed, that it would not have been possible for Einstein to have written his theory before Newton wrote his, nor, for that matter, before the development in the nineteenth century of new kinds of non-Euclidean geometry. But because we are arguing a sociological-cum-historical point, the case for accumulation does not require us to go even this far. It is simply enough to record that Einstein was working on similar subject matter to Newton, that he knew Newton's work in detail and that his own theory provided a better account of the subject matter. All of these sociological details are true and so we have a clear example of the accumulation of knowledge in the principal field where Kuhn and Feyerabend deny such a thing is possible.

What is also ironic is that, once their case is identified as being sociological, its logic becomes an interesting issue. Popper et al are arguing from a number of examples in science, almost all of which are confined to physics and chemistry, to the whole of science. Their logic is that some examples of science are of a certain kind (some A are B), therefore all of science is of the same kind (all A are B). But this, of course, is the logic of induction, the very thing that they are united in rejecting from the outset. None of them dissect the nature of other sciences, such as medical science or geological science (Feyerabend does not even give us a decent account of the principles of voodoo), so their notions of rival paradigms and deteriorating or progressive research programs can only be applied to all of science by drawing the kind of inductive conclusions that they deny have any validity.

Another furphy in this debate is the claim that all observation statements are already preladen with theory. Now, if all observations were laden with theory, we could ask of any observation which particular theory it is supposedly laden with. Once we do this, it becomes apparent that the claim cannot be sustained. Consider the case of Galileo's observation through a telescope of the planet Jupiter and its moons in 1609. At the time, Galileo was a convert to the theory of Copernicus that the planets orbited the Sun and that moons orbited planets. This might have influenced the fact that, when he saw Jupiter's moons for the first time, they appeared to him to be orbiting the planet. However, would anyone imagine that if a supporter of the old Ptolemaic theory of astronomy had looked through a telescope at the same time he would have seen anything different? Would we expect the Ptolemaic theorist to see the [begin page 203] moons not in orbit but roaming the skies *above* Jupiter as his own theory might have expected them to be doing? If you look at Jupiter today, no matter whether you accept the theories of Edwin Hubble or Athena Starwoman, you still see the moons orbiting the planet, exactly as Galileo saw them. What you observe through the telescope is independent of any theory. How you *theorise* about your observation is, naturally, highly influenced by your theories, but the observation itself is never tied to them in any necessary way. Nor are we engaging in theory when we give names to the things we see in space such as 'moon' or 'planet' or when we describe a visible process as an 'orbit'. All we are undertaking is the common procedure of applying names to observable objects. A theory is always committed in some way to a statement about the unobserved, and the mere act of naming what we observe is nothing more than what it is. The 'low level' example cited by Alan Chalmers above (the warning about the wind blowing the baby's pram over the cliff) does not show that every observation statement must be laden with some scientific theory. Nothing in the statement he cites, which could have been made by child, deserves the status of scientific theory. The example presupposes nothing more than the use of language and the attribution of meanings to words to describe common experiences.

All this is not to claim, it should be emphasised, that the observations of natural scientists or social scientists are never influenced by theories. Indeed, the opposite is often the case. People frequently go hunting for observations and evidence to prove a theory upon which they have already settled. However, to restate a previous point: this is a sociological fact not a point of logic. There is no theory *inherent* in every observation; observations are not, as a matter of necessity, dependent on any theory of any kind.

There is one more question of logic that should be discussed at this point. In making his case that we can never have any certainty about the death of Julius Caesar because observations by historical characters are always subject to an 'infinite regress' of questions, Popper raises another issue. Because historians are describing the past, they are dealing with a finite world, something that existed at one stage but is now completely behind us. Now, we cannot ask an 'infinite' number of questions about a finite world. We might be able to ask a great many questions, but an infinite number is a logical impossibility. When historians accept observations about the past as evidence that an event really happened, they are always reluctant to take one report as proof of this. They prefer the corroboration of observations from many observers. This is what they have with the death of Julius Caesar. Every report they have ever seen about the Roman Empire around 44 BC, no matter how close or how removed the source, corroborates the assassination, and not one has yet turned up to falsify or even raise doubts about whether the event occurred. We [begin page 204] could, if we chose, calculate the probability that this event, out of all the possible things that might have been observed about Caesar and those around him at the time, did occur. For every corroboration, the odds in favour of the hypothesis that he was killed grow geometrically. There comes a point with historical corroboration about such a well-recorded event where any other scenario besides the one we have accepted becomes impossible. Because we are dealing with a finite world — the planet Earth in 44 BC — we can rule out the prospect that somewhere within an infinite number of scenarios lies one in which Caesar was not killed. Logical possibilities based on infinity do not count in this or any other historical case. There is, in fact, so much corroboration about this particular assassination that it is literally impossible for there to be a non-assassination scenario that fits everything else we know about what was happening in Rome at the time. We know that Julius Caesar was killed in Rome in 44 BC just as surely as we know that John F. Kennedy was killed in Dallas in 1963.

As I noted earlier, if we seriously entertained Popper's notion about the impossibility of observations providing us with historical knowledge, we would also have to agree that we can never know anything about society at all, including the most familiar of the everyday events that all of us experience. How could anyone have ever entertained such a ridiculous notion? Before answering this, let me turn to another set of ideas that makes the same assertion but has a different origin.

THE DOUBLE HERMENEUTIC AND REFLEXIVITY

There has long been a distinction in the humanities and social sciences between studying the *actions* of human beings and the *meanings* of human conduct. There have been times when one side of this division has been favoured at the expense of the other; and at other times the balance has tipped the opposite way. In the period between the Second World War and the late 1960s, action-based perspectives were very much in vogue. This was the heyday of behaviourism in psychology and sociology. Behaviourists argued that the meanings that people gave to what they did could be vague, contradictory and often difficult to either interpret or articulate. They thought it impossible to build a rigorous social science on such soggy foundations. Human actions, however, could be counted, measured and tested with precision and so appeared to provide the primary data from which a proper science of society could emerge. These days, the behaviourists' strictures to throw out meaning, interpretation and understanding and to focus only on measurement and overt actions looks both mistaken and fruitless. It is now a commonplace that the meanings people bring to what they do cannot be eliminated from any account [begin page 205] of human activity. It is clearly impossible to portray the richness of society and the reality of life once meaning is set aside.

Until recently, most historians were happy to include both action and meaning perspectives in their works. Most assumed that they could study actions (about which they could produce knowledge) as well as meanings (where they were probably limited to producing interpretations). This meant that many historians accepted that history had a dual nature as, on the one hand, a social science and, on the other, a member of the humanities. In recent years, however, the balance has not only swung away from the side of action but has gone right over the edge in the opposite direction. For we now have cultural and literary theorists insisting that it is only meaning that matters. Just like the behaviourists of the 1950s and 1960s, they have produced an orthodoxy with its own badges of identity and in-crowd terminology. One of the banners under which they are marching is called hermeneutics.

Hermeneutics is the theory of interpretation. It began as the field of interpreting religious texts such as the Bible but was later extended to history and sociology. It holds that the proper way to study human affairs is not to examine the causes or to measure the incidence of behaviour but rather to interpret the meanings of social actions from the point of view of the agents performing them. Nineteenth century hermeneuticists who wrote history said their goal was to reproduce the mind, or the mental perspective, of the people who lived in the past. This objective became enshrined as one of the basic and enduring principles of historical practice, especially in some of the great nineteenth century studies of European culture, such as Jacob Burckhardt's *The Civilisation of the Renaissance in Italy*. Twentieth century hermeneuticists, however, have gone much further to claim that their approach is the only proper way to contemplate human affairs. The study of human conduct, they claim, is fundamentally different from the methods of the natural sciences because its aim is the 'understanding' of human meanings, not the gaining of objective information. Because it is based upon meanings, human activity can be understood 'from the inside', unlike the natural world to which we relate only as outsiders.[34] The leading contemporary exponent of hermeneutics, the German philosopher Hans-Georg Gadamer, a colleague and ally of Martin Heidegger, says the appropriate model in seeking to understand the meaning of human conduct is that of interpreting a text.[35] The field is strongly influenced by the ideas of Nietzsche, especially his assertion that 'there are no facts, only interpretations'. Hermeneuticists such as Gadamer insist that interpretation itself is never a simple exercise. This is because the interpreter always brings his own meanings, prejudices and preconceptions to the task. He is attempting to understand the meanings of others but can do this, not in any objective sense, but only through the web of his own meanings and culture. [begin page 206]

The British sociologist Anthony Giddens has argued that there is an additional dimension involved when social scientists study their world. Social science, he says, is not insulated from its subject matter in the way that natural science is. For example, no matter what evidence a physicist finds or what theory he supports, his published work does not have any affect on the laws of physics. However, Giddens argues, the publications of social scientists often have a considerable impact on what happens in human affairs. The social sciences operate within what he calls a 'double hermeneutic' involving two-way relations between actions and those who study them. 'Sociological observers depend upon lay concepts to generate accurate descriptions of social processes; and agents regularly appropriate theories and concepts of social science within their behaviour, thus potentially changing its character.'[36]

The clearest example of this is the study of economics, which describes the motivations and institutions of economic life in terms defined by its participants. In turn, the theory of economics and the inferences that can be drawn from it have a considerable effect on the economic process itself, influencing activities ranging from market-driven phenomena such as the price of shares on the stock market or the value of the dollar, to more deliberated activities such as the formulation of national economic policy. Even those sociological activities that appear to be 'objective', such as the compilation of statistics on the distribution of population, birth and death rates, marriage and the family, all 'regularly enter our lives and help redefine them', Giddens says. One of the clearest examples of this, he says, is the self-fulfilling prophecy that social and economic analyses regularly provide. 'Theorising in social science is not about an environment which is indifferent to it, but one whose character is open to change in respect of that theorising.'[37] Giddens has used the concept of the double hermeneutic to develop what he calls his 'theory of reflexivity'.

> The reflexivity of modem social life consists in the fact that social practices are constantly examined and reformed in the light of incoming information about those very practices, thus constitutively altering their character... In all cultures social practices are routinely altered in the light of ongoing discoveries which feed into them. But only in the era of modernity is the revision of convention radicalised to apply (in principle) to all aspects of human life, including technological intervention into the material world. It is often said that modernity is marked by an appetite for the new, but this is not perhaps completely accurate. What is characteristic of modernity is not an embracing of the new for its own sake, but the presumption of wholesale reflexivity — which of course includes reflection upon the nature of reflection itself.[38]

Giddens uses his explanation of this phenomenon to argue that not only is what passes for knowledge of the social world inherently uncertain but this knowledge itself contributes to the 'unstable or mutable character' of the [begin page 207] social world. The conclusion he draws from this is that we cannot have any knowledge about society, in what he calls 'the old sense' of knowledge meaning certainty. The 'circulating of knowledge in the double hermeneutic ... intrinsically alters the circumstances to which it originally referred'.[39] Hence:

> … the equation of knowledge with certitude has turned out to be misconceived. We are abroad in a world which is thoroughly constituted through reflexively applied knowledge, but where at the same time we can never be sure that any given element of that knowledge will not be revised.[41]

Knowledge, then, according to Giddens can no longer mean truth or certainty. When we use the term, he says, we should understand it as referring to nothing better than 'claims to knowledge'. Hence, 'action' perspectives that try to provide knowledge are misguided. Social science is essentially a hermeneutic exercise, which attempts to deal with a 'necessarily unstable' subject, the 'careering juggernaut' of the modern world.[41]

It should be pointed out that, within Giddens's work over the past decade, there has been a subtle and unacknowledged shift in his account of reflexivity and the double hermeneutic. In his early writings about this process from 1984 to 1986, reflexivity was something that 'could' happen, not something that 'must' happen. For instance: 'The "findings" of the social sciences *very often* enter constitutively into the world they describe.'[42] However, by the 1990s, Giddens was confident he had grasped one of the *inherent* features of contemporary society. He now writes that the modern world is 'thoroughly constituted' by 'wholesale reflexivity', and that 'knowledge reflexively applied to the conditions of system reproduction *intrinsically* alters the circumstances to which it originally referred'.[43] His later work thus argues, first, that reflexivity is a *necessary* component of contemporary society and, second, that it *must* change the world to which it refers.

In his writings in the 1990s, Giddens has so persuaded himself of the strength of his thesis that when he now discusses reflexivity he feels he can dispense with the need to justify it by referring to any evidence. However, if we go back and look at the early examples he provides himself, it is apparent that he is grossly overstating his case. For instance, it is likely that the publication of sociological statistics about divorce has some impact on the divorce rate. It is always easier for individuals to take difficult decisions if they know there are others doing the same. But this is not something that is inevitable or necessary. Divorce statistics might influence some individual decisions but they are just as likely to be irrelevant in many cases where the nature of the relationship between spouses is by far the overriding factor, no matter what the statistics might be. Similarly,

if economic analysis indicates that the stock market is due to fall, the price of shares may well decline, but not necessarily so. If other factors are present, such as portfolio managers with large funds to invest, this [begin page 208] economic analysis may not produce any self-fulfilling prophecy at all. In other words, material conditions can often render beliefs about society irrelevant. When beliefs about society really do alter the circumstances to which they refer, this is a contingent matter, not something necessary.

Giddens's argument, moreover, overlooks two things. First, most people carry out their lives completely oblivious to sociological statistics and economic analysis, especially the former. If the undergraduate students I have taught over the last twenty years are at all representative, most people who gain a higher education degree in the humanities, let alone the majority of the population, cannot read a statistical table properly nor draw any conclusions from a time series graph. Second, there is the issue of the poor quality and sullied reputation of most sociological analysis and economic prediction. One of the conditions of modernity that Giddens should have considered is that people today are so bombarded with contradictory opinions from academic 'experts' in the media that most take them all with a grain of salt. Most business people today know that all economic predictions are bound to be wrong in varying degrees. When television viewers these days see the typical sociologist on their screen, adopting a predictably provocative position on some controversial issue, their most common response is not belief but wonder at how someone with views so divergent from ordinary intelligence ever got a job at a university in the first place.

The 'double hermeneutic' thesis, then, commits the same fallacy as the theses of Popper and his fellow philosophers of science, that of shifting from a sociological statement to a logical statement. From the premise that there are some examples of reflexive understanding in society, Giddens slides into the claim that reflexivity is therefore a logically necessary component of modern society. From this he goes on to draw the same conclusion as the philosophers of science, that 'knowledge' cannot mean certainty. But since the argument is invalid, it provides no support whatsoever for this conclusion.

Even in those cases where we recognise reflexivity is at work (say, couples being more inclined to divorce in an era with a high divorce rate) they do not provide grounds for a total lack of certainty. Just because an aspect of society is constantly shifting ground does not mean you cannot have knowledge about it. You can have knowledge about its movement. You can construct a narrative of the pattern of its shifting ground. This, indeed, is the very point on which historians have insisted all along. There is no aspect of society that stands still long enough to be subject to a sociological analysis. The only accurate way to understand society is historically; that is, as a moving phenomenon, as something with a time dimension. There is nothing in Giddens's hermeneutics or theory of reflexivity that undermines history's claims to provide knowledge nor its status as the proper study of humankind. [begin page 209]

THE LANGUAGE OF RELATIVISM

The scepticism about knowledge considered in this chapter has become so well entrenched that it deserves some discussion, not only about its logical fallacies, but about why scepticism itself exists. One could answer this with a long diversion into the politics of post-1960s intellectual fashions among all the familiar radical groups. However, let us confine the discussion to science and the philosophy about it. David Stove attributes scepticism largely to the impact on intellectuals made by Einstein's revolution in physics. For the two hundred years prior to Einstein's revolution, scientists had believed that Newton's laws of mechanics and gravitation provided them with certainty. Einstein's demonstration that this was not so, came as a great shock and, in the subsequent process of disillusionment, the notion of certainty itself was one of the major victims. Many philosophers concluded that, since Newtonian physics were not certain, nothing was. The subsequent intellectual environment was dominated by an anxiety that the vainglory that had existed before the fall of Newtonian physics should never recur. 'To philosophers like Popper', Stove writes, 'the moral was obvious: such excessive confidence in scientific theory must never be allowed to build up again'. Since the most irrefutable of all such theories had been shown to be not irrefutable, Stove argues that the mood was ripe for a response that denounced irrefutability and substituted its opposite: total suspension of belief.[44]

Now, radical scepticism is nothing new in philosophy. The ancient Greek philosopher Pyrrho, had defended the notion and in the eighteenth century the Scottish philosopher David Hume had argued for a general scepticism about the unobserved. The problem for philosophers in the early twentieth century who wanted to assert such a position was that science still seemed to be remarkably successful. In the era of such extraordinary phenomena

as air transport, radio, and antibiotic medicines, the public at large ignored the implications of Einstein's revolution and continued to believe that scientific discoveries were not only certainties but that they were increasing at a geometric progression. To be taken seriously in such an environment, a philosophical sceptic could not express himself outright and deny that science made discoveries or assert that scientific knowledge was not growing. According to Stove, Popper solved this dilemma by continuing to use words such as 'discover' and 'knowledge' but changing their meaning.

Stove points out that words like 'discover' and 'knowledge' are success-words. He gives a number of examples and counter-examples such as 'prove', which is a success-word because you can only prove what is true, and 'believe', which is not a success-word because you can believe what is not true. Similarly, the verb 'refuted' is a success-word since it means 'proved the falsity of', but 'denied' is not since it only means 'asserted the falsity of '. 'Knowl- [begin page 210] edge' is a success-word because you can only know what is true, 'discovery' is a success-word because you can only discover what exists, 'explanation' is a success-word because you cannot explain anything except what is the case. So, when Popper the radical sceptic writes books with the titles The *Logic of Scientific Discovery* and *The Growth of Scientific Knowledge*, and when Lakatos entitles a collection of his essays *Proofs and Refutations,* they are engaging in what Stove describes as 'neutralising success-words'.[45]

When writing about the history and sociology of science, Stove observes, it is very difficult to avoid success-words. The most common tactic adopted by Lakatos when he finds himself in this position is to put quotation marks around words such as 'proof,' 'facts' and 'known' as in: 'One typical sign of the degeneration of a program which is not discussed in this paper is the proliferation of contradictory 'facts' ... His 1887 experiment 'showed' that there was no ether wind on the earth's surface.'[46] There are also examples of Kuhn writing that when one paradigm replaces another, 'new knowledge' replaces 'knowledge of another and incompatible sort'. Citing this, Stove points out that knowledge implies truth and truths cannot be incompatible with one another. He says that the worst example of the neutralisation of a success-word is a phrase that is central to Popper's whole account: 'conjectural knowledge'. To say that something is known implies that it is known to be true. To say that something is conjectural implies that it is not known to be true.[47] Hence, what these examples demonstrate is not simply the coining of neologisms nor the bending of the rules of language (in Lakatos's term 'language-breaking') but the direct contradiction of the accepted meaning of the terms used. To maintain its plausibility, radical scepticism in the philosophy of science has had to reverse the common meaning of its central terms.

Precisely the same thing is evident in the words of those social scientists who argue that today nothing is certain. When Anthony Giddens writes 'Let us first of all dismiss as unworthy of serious intellectual consideration the idea that no systematic knowledge of human action or trends of social development is possible', we need to read this in light of what he says he means by, 'knowledge'. In the same chapter he tells us that 'the equation of knowledge with certitude has turned out to be misconceived'.[48] Now, if we are certain of something it must be true and we must know it to be true. Yet the same is true of knowledge. So Giddens's claim that we can have knowledge yet not be certain of it is, like the claims of Popper and Co., the assertion of a self-contradiction.

For the sake of argument, let us try a rescue operation on Giddens's ideas and accept that, if knowledge can never be certain, whenever he uses the success-word 'knowledge' we should interpret him as meaning the non-success-word 'belief'. In this light, his first sentence quoted above could be re- [begin page 211] written as: 'Let us dismiss the idea that no systematic belief about human action or social development is possible.' Put this way, we have a statement with which almost everyone would agree. That, of course, is the trouble with it. For it becomes immediately clear that, since beliefs do not have to be true, one systematic belief is as good as any other. The systematic beliefs about society held by religious fundamentalists or astrologists (all of whom will insist they have very good reasons for their beliefs) have the same status as that of the systematic beliefs of a sociologist or an historian. We are left with a relativist theory where what is counted as 'true' (though never 'certain') is determined by personality, aesthetics, money or, more likely, what Lakatos himself denounced as mob rule.

While this has implications for Giddens's own reputation as a scholar, the more serious issue is the extent to which he and the others discussed in this chapter — all of them highly placed academics in influential positions — are prepared to abuse the language in the way they have done. By trying to eliminate the truth content of words such as 'know', 'fact', 'proof' and 'discover', they are all involved in an arrogant but tawdry attempt to change the meaning of the language for no better reason than to shore up their own misconceived and otherwise self-contradictory theories.

IN DEFENCE OF INDUCTION

In 1628 the English physician William Harvey published his findings about the circulation of blood and the function of the heart in animals. In doing so, he overthrew the prevailing theory of the ancient Greek physician Galen, who believed that the heart functioned primarily as a source of heat. Over the 350 years since Harvey's findings were published, the details of his discovery have been refined, but no one has seriously questioned its central claim. Today, there are more than four billion human beings on the planet and many times that number of other mammals. No one has ever found a human or any other kind of mammal whose blood did not circulate through the body (unless of course it was dead). Throughout the world, every day of the year, there are millions of medicines administered orally and intravenously, each of which counts as a little experiment that confirms that blood does indeed circulate. Yet despite these millions and billions of observations that confirm that Harvey was right, and despite the absence of even *one* counter-observation, Karl Popper and his followers maintain that this is still not enough evidence for us to be able to say that we can be certain that the blood circulates. Not only this, but they are committed to the position that this enormous accumulation of data does not allow us to be certain that even *one* of the vast number of mammals [begin page 212] that will be born at any time in the future will have a body through which the blood circulates.

The reason that they hold what is, when put like this, such an obviously ridiculous position is because all the observations and experiments described above amount to nothing more than an inductive argument. You make an *inductive* argument when, after a number of observations of a certain phenomenon occurring, there comes a point when you say you have good reasons for drawing a more general conclusion. The example used in philosophy textbooks is usually 'All the ravens observed so far are black. Therefore, all ravens, now and in the future, are black'. An inductive argument, then, is an argument from premises about what has been observed to a conclusion about what has not been, or in some cases could not be, observed. These days, however, anyone who advances a case based on induction, whatever the form it takes, runs the risk of being accused of engaging in what recent generations of humanities students have been taught is a hopelessly flawed exercise.

The reason inductive arguments are now held in low repute, and the reason why so many students of social theory and scientific method today would rather reject Harvey's theory as a piece of knowledge than accept the principle of induction, is that they have been taught by Popper that they should accept the views of the Scottish philosopher David Hume. In two of his major works, *A Treatise of Human Nature* (1739) and *An Enquiry Concerning Human Understanding* (1748), Hume argued, first, that the premise of an inductive argument was no reason to believe the conclusion and, second, that there was no reason whatsoever (neither from experience nor anything else) to believe any contingent proposition about the unobserved. Popper acknowledges Hume's argument as the basis of his own rejection of induction.

> I approached the problem of induction through Hume. Hume, I felt, was perfectly right in pointing out that induction cannot be logically justified. He held that there can be no valid logical arguments allowing us to establish 'that those instances, of which we have had no experience, resemble those, of which we have/ had experience.' Consequently 'even after the observation of the frequent or constant conjunction of objects, *we have no reason to draw any inference concerning any object beyond those of which we have had experience*' [Hume, Treatise, Book I, Part III, sections vi and xii] … As a result we can say that theories can never be inferred from observation statements, or rationally justified by them. I found Hume's refutation of inductive inference clear and concisive.[49]

David Stove has argued that Popper, writing a generation before Kuhn, Lakatos and Feyerabend, felt the need to justify his own philosophy by citing Hume's argument as one of its foundations. While the latter trio do not identify Hume in the same way, they are nonetheless just as committed because, [begin page 213] in this area, their own writings are extremely derivative of Popper. Hence, Stove concludes, Hume's argument about induction is the basis of the radical scepticism of all four authors.[50]

Hume's conclusion, cited in italics above, is that we have no reason to believe any contingent proposition about the unobserved. In a detailed analysis of all the premises and subarguments Hume needed to reach this conclusion, Stove shows the starting premise is the invalidity of inductive

arguments. This, Stove agrees, is indisputable. 'Some observed ravens are black, therefore all ravens are black' is an invalid argument. The 'fallibility of induction' premise is linked in Hume's argument with a general proposition about deductive arguments that Stove calls 'deductivism'. Deductivism holds that the only good arguments are deductive ones, that is, for P to be a reason to believe Q, the argument from P to Q must be valid. Together, the invalidity of inductive arguments and the premise of deductivism produce the subconclusion that Stove calls 'inductive scepticism'. This holds that no proposition about the observed is a reason to believe a contingent proposition about the unobserved. This subconclusion is itself then linked to the general proposition of empiricism which holds that any reason to believe a contingent proposition about the unobserved is a proposition about the observed. Together, inductive scepticism and empiricism produce Hume's conclusion. Stove's summary diagram of the overall argument is:

Fallibility of Induction + Deductivism--> Inductive Scepticism
+ Empiricism
--> Scepticism about
the unobserved

Stove argues that the key premise to the whole case is the assumption of deductivism. The fallibility of induction, on its own, does not produce inductive scepticism. For from the fact that inductive arguments are invalid it does not follow that something we observe gives us no reason to believe something we have not yet observed. For instance, if all our experience of flames is that they are hot and they burn, this does give us a reason for assuming that we will get burned if we put our hand into some as yet unobserved flame. This might not be a logically deducible reason, but it is still a good reason. But once the fallibility of induction is joined with the deductivist assumption that the only acceptable reasons are deductive ones, that is, those from logically valid arguments, then inductive scepticism does follow. (The general proposition about empiricism in the second stage of the argument needs to be joined with inductive scepticism to produce the final conclusion because some people believe that you can know the unobserved by non-empirical means, such as faith or revelation. As an empiricist, Hume, as does Popper, rules these means out as proper grounds for belief.) [begin page 214]

Stove argues that to embrace deductivism as the only criterion for accepting an empirical argument is not, as it might appear, to impose the highest standards possible on debate. It is, in fact, to accept a point that carries no weight at all in this kind of argument. To assert the deductivist position is to assert a necessary truth, that is, something that is true not because of any way the world is organised but because of nothing more than the meanings of the terms used in it. Necessary truths are void of empirical meaning. So when any sceptic claims that a flame found tomorrow *might* not be hot like those of the past, or that the next baby born *might* not have circulating blood, he has no genuine reason for this doubt, only an empty necessary truth. Stove comments:

> If I have, as Popper says I should not have, a positive degree of belief in some scientific theory, what can Popper urge against me? Why, nothing at all, in the end, except this: that despite all the actual or possible empirical evidence in its favour, the theory *might* be false. But this is nothing but a harmless necessary truth; and to take *it* as a reason for not believing scientific theories is simply a frivolous species of irrationality.[51]

Outside the world of philosophers and sociologists of science and their students, there are very few people who believe that deductivism is true. Most people accept that observations often provide perfectly good reasons for believing a conclusion even though that conclusion might not be entailed by, or deducible from, these observations. Similarly, they accept that some observations provide good reasons for disagreeing with a conclusion, even though we might not have a knock-down, deductive case. In these cases, the logical relations involved are less than absolute but nonetheless persuasive. The terms we use to describe these relations are similarly less than absolute. We say that the observation P confirms the conclusion Q (rather than proves it), or that L disconfirms M (rather than refutes it), or that A is inconsistent with B (rather than contradicts it). The study of these kinds of logical relations has been called, variously, confirmation theory, non-deductive logic or inductive logic. The most important body of scholarship to come out of the study of these relations is probability theory. The development of probability theory began in the seventeenth century but its supporters agree that its major landmarks, were made in the mid-twentieth century, especially in the writings of Rudolf Carnap and Carl Hempel.[52] These two were members of the pre-war Vienna Circle of logical positivists. Probability theory, in other words, is the product of those hopelessly unfashionable positivists who are so peremptorily dismissed today by social scientists and literary critics. Yet, in the study of human affairs, probability theory and its derivatives and affiliates, such as statistical method, provide a far more relevant logical foundation than the empty deductivism of radical scepticism. Non-deductive logic, for instance, allows [begin page 215] us to have good reasons to believe well-known facts such as the assassinations of Julius Caesar and John F. Kennedy, unlike the radical scepticism that commits us to permanent doubt on both scores. Most importantly for the debate covered in this chapter, it shows that there is a rational alternative to the foundations of belief that are urged upon us by the radical sceptics and so willingly accepted by hermeneuticists, postmodernists and their kind. Neither the natural sciences nor the social sciences are doomed by logic to profound and perpetual uncertainty.

One who would not disagree with this last statement is no less a figure than David Hume himself. Despite being the progenitor of the radical sceptics' position, Hume later dismissed the thesis as 'a juvenile work'. It first appeared in the *Treatise on Human Nature*, published in 1739 when he was 28 years old, and was repeated nine years later in his *An Enquiry Concerning Human Understanding*. However, forty years on, in the work of his maturity, *Dialogues Concerning Natural Religion* (1779), one of the earliest positions that Hume summarily rejected was the inductive scepticism of his youth.[53] For the next 150 years, a period of unprecedented growth in scientific and technological marvels, the thesis was largely ignored by scientists and philosophers alike. It was revived in the twentieth century not because of its persuasive power but for psychological and political reasons flowing, as we have seen, from Einstein's theoretical revolution as well as from the general sense of instability that prevailed in Europe after the First World War. Its attraction in the 1980s and 1990s, similarly, owes far more to politics and psychology than to anything more compelling. And in this, at least, Thomas Kuhn was right. People often accept a theory for reasons of custom, fashion and peer pressure. As a sociological statement this is no doubt correct, but as a guide to the true worth of a theory it carries no weight at all.

THE STATUS OF HISTORICAL EXPLANATIONS

In the 1940s and 1950s there was a wide-ranging debate among philosophers in America and Britain about the scientific status of history. Some, including the logical positivist Carl Hempel, argued that the same kind of general laws that applied in the natural sciences also applied in history. He reasoned that since everything that happens has a scientific explanation and since all scientific explanations presuppose general laws, so everything that happens, including historical events, can be subsumed under general laws. The overall aim of the case was to demonstrate what Hempel called 'the methodological unity of empirical science'. His argument attracted a variety of replies, which ranged from complete rejections of his concepts of 'laws' and 'explanations' to agreement that, while it might be possible *a priori* for historical explanations to [begin page 216] be subsumed under general laws, given the current state of play there was so little chance of this happening that the prospect remained 'purely visionary.[54] Without going into the finer details of this debate, one can nonetheless record that, since the 1950s, Hempel's opposition has by and large prevailed and majority opinion has been against the idea that history should be regarded as a science.

There are a number of very obvious differences between the way that most scientists study nature and the way that historians study human activity. On the one hand, a chief aim of natural science is to find generalisations or laws that are invariant in space and time. To pursue this, many adopt the method of experiment where the aim is to study their subject in a laboratory, isolated from all the variables that occur in the real world. Most scientists are dealing with phenomena that repeat themselves and their aim is to be able to generalise about these repetitions. On the other hand, the variance of time is one of the defining characteristics of the study of history. Historians deal in change over time of events that, by their nature, cannot be repeated. They study specific circumstances, not undifferentiated phenomena. They can never isolate their subject matter from outside variables; indeed, the variables of the real world are essential components of their explanations. Instead of finding general laws, historians aim to produce narratives of unique events.

While it is indisputable that these differences exist between history and many of the natural sciences, the same is not true of all the natural sciences. In recent years, the American evolutionary biologist Stephen Jay Gould has given us a powerful reminder of how closely his own field of study relates to the methods and assumptions of human history. In his reflections on the Cambrian fossils of the Burgess Shale of British Columbia, Gould argues that the study of many large domains of nature, including human society, evolutionary biology and geology, must be undertaken

with the tools of history. Moreover, if the theory of the big bang and the expanding universe is correct, cosmology, too, is an essentially historical study.[55] In each of these cases, the research data used in the field come from the traces of the past that can be found in the present. In each case, the ultimate method of exposition is narrative explanation. In an historical explanation in any of these sciences, Gould says, an event, E, is explained in terms of narrative. E occurred because D came before it, preceded by C, B and A. If any of these stages had not taken place , or had emerged in a different way, E would not exist. Thus event E is intelligible and can be explained rigorously as the outcome of A through D.[56]

Narrative should not be interpreted as being any more than it is. Even though analysing the causes of events is common historical practice, a narrative account is not necessarily a causal explanation. As one of an earlier generation of exponents of historical method, G. R. Elton, has insisted, to sup- [begin page 217] pose that causal relationships are the only content of history is an error. Cause and effect constitute only one of a number of possible relationships that historians handle in a narrative that moves from state *a* to state *b*. 'If *a* can be said to have caused *b* the relationship happens to be causal; but it is none the less properly historical if *a* and *b* are linked by coincidence, coexistence, or mere temporal sequence, all relations very often encountered in history, however less intellectually satisfying they may be.'[57]

One thing that narrative cannot do is engage in prediction. An historical explanation does not involve a direct deduction from any laws of nature or of human society that may then be projected into the future. An outcome in history is not even predictable from any general property of a larger system. For example, while the victory of the Northern states in the American Civil War may seem with hindsight to have been determined by their superiority in population and industry, we cannot speak of any predictability about the outcome. This is borne out by the experience of other wars, for example Vietnam in the 1960s, where a smaller population and industrially inferior economy defeated its much more powerful American opponent. In the latter case, the general property of America's larger population and industrial sector was insufficient to produce victory. Though historians may explain event E as the outcome of its antecedents, there is never anything necessary or law-given about this. Any variant on E that arose from a different combination of antecedents (say, a southern victory in the Civil War) would have been just as explicable, even though radically different.

The impossibility of prediction does not, however, rule out the possibility of comprehension. What happens in history is by no means random or chaotic. Any major change in history is dependent on, that is, contingent upon, everything that came before. Contingency, Gould contends, is the central principle of all historical explanations. The modern order of animal life, he says, was not guaranteed by underlying laws such as natural selection nor by any mechanical superiority in the anatomical design of those animal types that have survived the evolutionary process. Gould uses the evidence from the Burgess Shale to show that, over the last 570 million years, the number of different animal phyla (the fundamental divisions among animals based on anatomical design) has greatly reduced, not expanded as older theorists of evolution thought. Dramatic changes in climate and geography in the ensuing period have eliminated many more species than now exist. The rule that determined which would survive was not that of 'the fittest' in absolute terms but merely that of the species that happened to be better adapted to the quirks of local environmental change. Often, relatively insignificant creatures — such as mammals were sixty million years ago — withstood drastic changes that eliminated creatures such as dinosaurs — that had been supremely well-adapted [begin page 218] to the previous environment. The fact that one of the phyla that survived the Cambrian era, the chordates, should have eventually evolved into human beings was, Gould argues, an 'awesome improbability'. Although this outcome was rooted in contingency, the historical method of evolutionary biologists can explain it in terms that are just as intellectually respectable as those of more conventional science. 'Our own evolution is a joy and a wonder because such a curious chain of events would probably never happen again, but having occurred, makes eminent sense.'[58]

Contingency in history does not mean that explanations are confined to singular statements, with one small event following another without more general phenomena being discernible. Gould points out that life on Earth exhibits a pattern obedient to certain controls: the chemical composition of the planet, the physical principles of self-organising systems and the constraints of design of multicellular organisms as well as the exigencies of the prevailing environment. Similarly, human affairs often conform to processes within which we can discern broad forces to which all persons must bend their will. In human history, it is usually possible to distinguish

between wider process and individual agency. In any era, depending on the degree of focus that they choose, historians can describe either the more general social constraints and opportunities or individual actions and their motives. Many, of course, readjust their lens over the course of a work to take in both. The choice faced by historians has been nicely delineated by P. J. Cain and A. G. Hopkins in their recently published history of British imperialism. Explaining their decision to focus more on process than on agency, they write: 'Thus, we are concerned less with anatomising the biographical entrails of a Dilke or a Rhodes than with explaining why Dilke-like or Rhodes-like figures arose in the first place.'[59]

Like other scientific practitioners, historians study their subject by means of a disciplined methodology. This involves adopting practices and standards that are commonly recognised throughout the discipline, especially in their handling of the evidence that goes to make up their explanations. The deployment of evidence within history, however, is one area in which many of those who reject its scientific status believe they have a winning hand. Historical evidence takes the form of the documents that remain from the past, and there are two arguments frequently given about why this is always problematic. First, it is claimed the process is inherently selective. The documents that remain from the past are not a complete record. What has been preserved is often determined by what the historical actors themselves thought desirable to leave to posterity. The evidence available is therefore claimed to be always tainted by subjectivity. Second, it is argued that the process is basically interpretative. Analysing documents is nothing more than interpreting texts and [begin 219] the process of interpretation is, again, always subjective. Hence, on this account, historians are just as far removed from any claim to a scientific method as are literary critics.

Many of those who put one or both of these arguments appear to assume that the evidence upon which historians rely is composed of a fixed and given body of documents. This certainly seems to be behind many of the assumptions of the French author Paul Veyne, whose book *Writing History* mounts a sustained critique of the scientific status of history.[60] The same is true of Michel Foucault who, when interviewed about his history of medicine, *The Birth of the Clinic*, said he had prepared himself by reading all the documents on the subject for the period 1780-1820, by which he meant nothing more than the small number of published works written by contemporary health reformers and medical scientists.[61] While this may be acceptable in France, in most other countries historians operate on a different plane. They do not assume there is a given body of specially preserved documents with which they must work. As G. R. Elton has observed, arguments like those above show their authors are not well acquainted with the way historical evidence comes into existence since 'that which is deliberately preserved by observers is a drop in the bucket compared with what is left behind by action and without thought of selection for preservation purposes'.[62]

Rather than 'selecting' from a given body of texts, most historians go in search of evidence to be used to *construct* their own account of what happened. To this extent, those structuralists and poststructuralists who say that history is constructed are correct. However, the historian's construction is not something derived solely from the internal machinations of his or her language and text. Nor is it a mere 'interpretation' of the texts provided by the people of times past. An historical explanation is an inductive argument constructed out of evidence, which is quite a different thing. There is actually a dual process involved: first, determining what evidence exists to address a given issue; second, analysing that evidence, which means testing it for authenticity and then assessing its significance for the case at hand. Although historians construct their case, they do not construct the evidence for that case; rather, they discover it. Very few documents left from the past are compiled for the benefit of historians. Probably the biggest single category is made up of the working records that all human institutions — family, workplace, law court, government or military — use to manage their affairs. The archive records of these institutions provide far more historical evidence than the limited range of published essays, books and memoirs consulted by Foucault and Co. Archival research has to be both painstaking and imaginative — the past does not yield up its secrets willingly — and is never neatly packaged and [begin page 220] readily accessible in the way many literary critics and social theorists assume on the basis of their own circumscribed research practices.

It is important to emphasise that those who insist that all historic evidence is inherently subjective are wrong. Archive documents have a reality and objectivity of their own. The names, numbers and expressions on the pages do not change, no matter who is looking at them, and irrespective of the purposes, ideologies and interpretations that might be brought to bear upon them. Historians are not free to interpret evidence according to their

theories or prejudices. The evidence itself will restrict the purposes for which it can be used. This is true even of those documents for which all historians agree that varying interpretations are possible. In these cases, the range of possibilities is always finite and can be subject to debate. Ambiguity or lack of clarity do not justify a Derridean dissolution into nullity. Moreover, once it has been deployed, the documentary evidence is there, on the historic record, for anyone else to examine for themselves. Footnoted references and proper documentation are essential to the practise of the discipline. This means that the work of historians, like that of scientists, may be subject to both corroboration and testability by others in their field.

While it is true that historians often come to the task of writing history with the aim of pushing a certain kind of theory, of establishing a certain point, or of solving a certain problem, one of the most common experiences is that the evidence they find leads them to modify their original approach. When they go looking for evidence, they do not simply find the one thing they are looking for. Most will find many others that they had not anticipated. The result, more often than not, is that this unexpected evidence will suggest alternative arguments, interpretations and conclusions, and different problems to pursue. In other words, the evidence often makes historians change their minds, quite contrary to the claims of those who assert that the reverse is true. Although theories or values might inspire the origins of an historic project, in the end it is the evidence itself that determines what case it is possible to make.

Overall, then, historical explanations have a number of characteristics that deserve to be regarded as properly scientific. Although they are narratives of unique, unrepeatable events and are not involved in formulating general laws or making predictions, historical explanations share these characteristics with several other fields of study including evolutionary biology, geology and recent approaches to cosmology. Like these fields, the history of human affairs is defined by its study of the variance over time of its subject matter. Again, like them, its explanations are grounded in contingency. What happens in history is not random but is contingent upon everything that came before. Historical explanations may focus on either general or specific accounts of human affairs, but usually involve the interplay between the two. Historians [begin page 221] adhere to a disciplined methodology that involves the construction of explanations from evidence. The evidence they use is not given but is something they must, first, discover and, second, analyse for authenticity and significance for the explanation. Only a minority of the evidence used by historians is that which has been deliberately preserved for posterity. Their biggest single source of evidence comprises the working records of the institutions of the past, records that were created, not for the benefit of future historians, but for contemporary consumption and are thus not tainted by any prescient selectivity. Most of these documents retain an objectivity of their own. Although much historical research may be inspired and initiated by historians' values and theories, the kind of documentation and reference citation used within the discipline means that their explanations can be tested, corroborated or challenged by others. Hence the findings made by historical explanations are the product of a properly scientific methodology.

HISTORY AS A DISCIPLINE

The concept of an academic discipline is being assailed these days on a number of fronts. This is especially true in the humanities and social sciences where, as Chapter One recorded, new movements in literary criticism and social theory want either to override the previous boundaries between disciplines or else to subsume some of the older fields within new ones. One of the authors discussed in this chapter, Anthony Giddens, has argued that there is no discernible difference any more between history and sociology and so both should be taken over by a creature of his own invention called 'structuration theory'.[63] From a different perspective, the proponents of cultural studies, as we have seen, believe that they are the ones now best equipped to handle historical issues. What is perhaps of even greater concern is the fact that the major recent champions of traditional academic values and the greatest critics of the new theories have themselves not seen fit to couch their defence within a framework based on the value of academic disciplines. Both Alan Bloom in *The Closing of the American Mind* and Roger Kimball in *Tenured Radicals* have upheld the value of 'the canon' of Western learning; that is, the generally recognised body of 'Great Books' that have stood the test of time and that, until recently, were acknowledged as central to a complete education. But their concept of preserving this canon has not extended to the intellectual disciplines within which most of these books were written. This is not, presumably, because Bloom and Kimball are against this idea. Let me give some reasons why they should have taken their argument one step further.

Rather than the production of a corpus of outstanding works, the basis of Western learning has been the organisation of the pursuit of knowledge

into [begin page 222] a number of distinct fields called 'disciplines'. Without decrying the stature of the Great Books, it is nonetheless true that their achievements were made possible by the contribution and the example of all those who laboured in the same intellectually coherent field of study. As Edward Gibbon, Isaac Newton and others openly acknowledged, the major figures have always stood on the shoulders of their peers. The history of Western knowledge shows the decisive importance of the structuring of disciplines. This structuring allowed the West to benefit from two key innovations: the systematisation of research methods, which produced an accretion of consistent findings; and the organisation of effective teaching, which permitted a large and accumulating body of knowledge to be transmitted from one generation to the next.

Intellectual disciplines were founded in ancient Greece and gained a considerable impetus from the work of Aristotle who identified and organised a range of subjects into orderly bodies of learning. The next major stimulus to the formation of disciplines was the scientific revolution of the seventeenth century and the Enlightenment of the eighteenth century when new disciplines proliferated and several older fields were revived. However, there had been a long, intervening period, from the early to the late Middle Ages, when most disciplines were obliterated by medieval scholasticism which insisted it could explain everything. The literature of this theological movement is characterised by an absence of differentiation between subject matter and by its lack of criteria for what type of arguments or evidence may be counted as relevant in any explanation. Obscurantism flourished and questions about the nature of the world were settled by appeal to the Bible and other religious texts. Cryptic deliberation reached such a point of absurdity that the fate of the Byzantine empire of the Orthodox church hinged on its dispute with the Latin church over whether the Holy Ghost proceeds from the Father and the Son, as from one principle and one substance, or proceeds by the Son, being of the same nature and substance, or from the Father and the Son, by one spiration and production.[64] To those who broke away from all this in the fifteenth and sixteenth centuries and who, by reviving the intellectual traditions of Greece and Rome, created the Renaissance, 'scholasticism' was a term of mockery and derision. One of the most striking things about the output of late twentieth century literary and social theory is how closely it resembles — through its slavish devotion to seminal texts and its unrestrained flight across all subject matter — the scholasticism of the medieval clergy. Instead of the title that this movement has conferred on itself, 'the new humanities', a more fitting epithet would be 'the new scholasticism'.

A discipline has a common viewpoint on its subject matter plus a common method of study. Several disciplines can share the same subject matter: human society, for instance, is the subject of history, sociology, anthropology and [begin page 223] economics. In this case, the difference between the disciplines is determined by the viewpoint with which the subject is approached and by the methodology used: history has always differentiated itself by its focus on the dimension of time and by an empirical, document-based research process. Disciplines are not fixed or static; they evolve over time, sometimes pursuing the logic of their founding principles into areas not imagined by their initial practitioners. Until recently, history itself was still evolving, as witnessed by the burgeoning of social history and 'history from below' in the 1960s and 1970s which added a valuable new dimension and insight to the field. But disciplines can also arrive at a point of crisis and suffer an irreparable breakdown. One could make a good case, in fact, that this is now the situation facing both sociology and anthropology. They were both founded as time-free studies of society and, now that it has dawned on them that it is impossible to investigate their subjects in this way, the inhabitants of these fields are on a desperate hunt for alternative territory. Hence their interest in occupying the ground that was once the sole province of narrative history.

Overall, it is fair to conclude that, despite all the claims to the contrary, history still retains its credentials as a discipline that demonstrates both the underlying merit of the Western scientific tradition and the fact that this tradition can be properly applied to the study of human affairs. The real test of intellectual value, of course, can only be demonstrated by the output of a discipline. Although they are being assailed on all sides, there is still enough work produced by empirical historians to confirm the worth of what they are doing and to establish that the complete victory of their opponents would amount to a massive net loss for Western scholarship. One of the best expressions of this comes from the now out-of-print and out-of-fashion 1960s manual by G. R. Elton cited earlier. Elton is one of the few commentators to have defended the discipline as a discipline, that is, as a joint effort by its practitioners who, through a process of research, dispute, claim and counterclaim, have made genuine advances in humanity's understanding of itself.

Anyone doubting this might care to take any sizeable historical problem — the decline of the Roman Empire, or the rise of industrial England — and study its discussion in the serious literature of the last fifty years. He will encounter a great deal of disagreement, much proven error, and probably a fair amount of plain nonsense; but if he is at all alert he will be astonished by the way in which the body of agreed knowledge has augmented and by the manner in which variations of interpretation come to be first increased and then reduced by this advance. Historians are so fond of parading their disagreements — and the study does, indeed, progress as often as not by the reopening of seemingly settled questions — that the cumulative building up of assured knowledge of both fact and interpretation is easily overlooked. Yet it is indeed impressive, the product of systematic, controlled, imaginatively conducted research.[65]

1. Anthony Giddens, *The Consequences of Modernity*, Polity Press, Cambridge, 1990, p 39

2 Ann Curthoys, 'Unlocking the Academies: Responses and Strategies', *Meanjin*, 50, 2/3, 1991, p 391

3 Harry Oldmeadow, 'The Past Disowned', *Quadrant,* March 1992, p 63

4 Thomas Kuhn, *The Structure of Scientific Revolutions*, 2nd edn, University of Chicago Press, Chicago, 1970

5 Thomas Kuhn, *The Structure of Scientific Revolutions*, p 149

6 Thomas Kuhn, *The Structure of Scientific Revolutions,* p 150

7 Thomas Kuhn, *The Structure of Scientific Revolutions,* p 94

8 David Bloor, *Knowledge and Social Imagery*, Routledge and Kegan Paul, London, 1976

9 H. M. Collins, *Changing Order: Replication and Induction in Scientific Practice,* Sage, London, 1985, p 148

10 Karl Popper, *Conjectures and Refutations: The Growth of Scientific Knowledge*, (1st edn 1963), 5th edn, Routledge, London, 1989. Popper's other major work on the subject is *The Logic of Scientific Discovery*, Hutchinson, London, 1959

11 Alan Chalmers, *What Is This Thing Called Science?* 2nd edn, University of Queensland Press, St Lucia, 1982, p 28

12 Anthony Giddens, *The Consequences of Modernity*, p 39

13 Thomas Kuhn, *The Structure of Scientific Revolutions*, pp 146-7

14 Alan Chalmers, *What Is This Thing Called Science?* p 61

15 Imre Lakatos, 'Falsificationism and the Methodology of Scientific Research Programs', in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge*, Cambridge University Press, Cambridge, 1970

16 Thomas Kuhn, *The Structure of Scientific Revolutions*, p 156

17 Thomas Kuhn, *The Structure of Scientific Revolutions*, pp 94, 153

18 Imre Lakatos, 'Falsificationism and the Methodology of Scientific Research Programs', pp 93, 178

19 Paul Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge*, New Left Books, London, 1975

20 Paul Feyerabend, *Against Method,* p 275, his italics

21 Paul Feyerabend, *Against Method,* pp 285, 301-2, his italics

22 Paul Feyerabend, *Against Method*, p 299

23 Paul Feyerabend, *Against Method*, p 308

24 'The Worst Enemy of Science', *Scientific American,* May 1993, pp 16-17

25 Paul Feyerabend, *Against Method*, Chapter 16

26 Thomas Kuhn, 'Logic of Discovery or Psychology of Research' and 'Reflections on My Critics', in I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge*

27 Karl Popper, *Unended Quest: An Intellectual Autobiography*, Flamingo edn, Fontana Books, London, 1986, pp 128-32

28 Karl Popper, *Conjectures and Refutations*, pp 22-3

29 Karl Popper, *Conjectures and Refutations*, pp 23-5, his italics

30 Karl Popper, *The Logic of Scientific Discovery*, Hutchinson, London, 1959, pp 191, 204, 190

31 David Stove, *Popper and After: Four Modern Irrationalists*, Pergamon Press, London, 1982, p 30

32 Both examples are cited by David Stove, *Popper and After*, p 32. They are from Thomas Kuhn, *The Structure of Scientific Revolutions*, p 156; and I. Lakatos and A. Musgrave (eds), *Criticism and the Growth of Knowledge*, p 158

33 David Stove, *Popper and After*, p 41

34 This is a point that is exclusive neither to hermeneutics nor to the twentieth century. It was first made by the eighteenth century Italian historical theorist, Giambattista Vico, and is one of the theoretical mainstays of the early-twentieth-century movement in sociology known as symbolic interactionism

35 Hugh J. Silverman (ed.), *Gadamer and Hermeneutics*, Routledge, New York, 1991

36 Anthony Giddens, 'Nine theses on the future of sociology', in his *Social Theory and Modern Sociology,* Polity Press, Cambridge, 1987, pp 30-1. Giddens first advanced the concept of the double hermeneutic in *The Constitution of Society*, Polity Press, Cambridge, 1984, p 284

37 Anthony Giddens, *Social Theory and Modern Sociology*, pp 21, 198

38 Anthony Giddens, *The Consequences of Modernity*, pp 38-9

39 Anthony Giddens, *The Consequences of Modernity*, p 54

40 Anthony Giddens, *The Consequences of Modernity*, p 39

41 Anthony Giddens, *The Consequences of Modernity*, p 53

42 Anthony Giddens, 'What do sociologists do?', inaugural lecture, University of Cambridge, January 1986, published in his *Social Theory and Modern Sociology*, p 20, my italics

43 Anthony Giddens, *The Consequences of Modernity*, pp 39, 54, my italics

44 David Stove, 'Cole Porter and Karl Popper: the Jazz Age in the Philosophy of Science' in his *The Plato Cult and Other Philosophical Follies*, Basil Blackwell, Oxford, 1991, p 19; see also his *Popper and After*, P 51

45 David Stove, *The Plato Cu*lt, pp 12-18; see also his *Popper and After*, Chapter One

46 I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, p 164n, cited by Stove, *Popper and After*, p 10

47 David Stove, *Popper and After*, pp 11, 14

48 Anthony Giddens, *The Consequences of Modernity*, pp 46-7, 39

49 Karl Popper, *Conjectures and Refutations*, p 42, my italics

50 David Stove, *Popper and After*, pp 50-5

51 David Stove, *Popper and After*, p 101

52 Rudolf Carnap, *Logical Foundations of Probability*, University of Chicago Press, Chicago, 1950; Carl Hempel, *Aspects of Scientific Explanation*, Free Press, New York, 1965

53 Cited by David Stove, *Popper and After*, p 102

54 Carl Hempel, 'The Function of General Laws in History', and Alan Donagan 'Explanation in History', both in Patrick Gardiner (ed.), *Theories of History*, Free Press, New York, 1959. Other contributors to the debate collected in the Gardiner volume were Morton White, Ernest Nagel, u B. Gallic, William Dray, Charles Frankel and Michael Scriven

55 Stephen Jay Gould, *Wonderful Life: The Burgess Shale and the Nature of History*, Penguin, London, 1989 pp 277-91. For current approaches to the history of the universe, see George Smoot and Keay Davidson, *Wrinkles in Time: The Imprint of Creation,* Little, Brown and Co, London, 1993

56 Stephen Jay Gould, *Wonderful Life*, p 283

57 G. R. Elton, *The Practice of History*, Sydney University Press, Sydney, 1967, p 1

58 Stephen Jay Gould, *Wonderful Life*, p 285. Though Gould's work reappraises some interpretations Darwin's maxim of 'survival of the fittest', Gould argues that his own view of post-Cambrian evolution is quite consistent with Darwin's account

59 P. G. Cain and A. G. Hopkins, *British Imperialism: Innovation and Expansion 1688-1914*, Longman London, 1993, p 49

60 Paul Veyne, *Writing History: Essay on Epistemology*, (1971), trans. Mina Moore-Rinvolucri, Wesleyan University Press, Middletown, 1984

61 Michel Foucault, *Foucault Live: Interviews 1966-1984*, ed. Sylvere Lotringer, Semiotext(e), Columbia University, New York, 1989, p 3

62 G. R. Elton, *The Practice of History*, p 91

63 Anthony Giddens, *The Constitution of Society;* Christopher Bryant and David Jary (eds), Giddens' *Theory of Structuration*, Routledge, London, 1991

64 These are the distinctions satirised in a well-known passage of Edward Gibbon, *Decline and Fall of the Roman Empire*, Everyman edn, London, 1966, Chapter 66, p 374

65 G. R. Elton, *The Practice of History*, pp 63-4