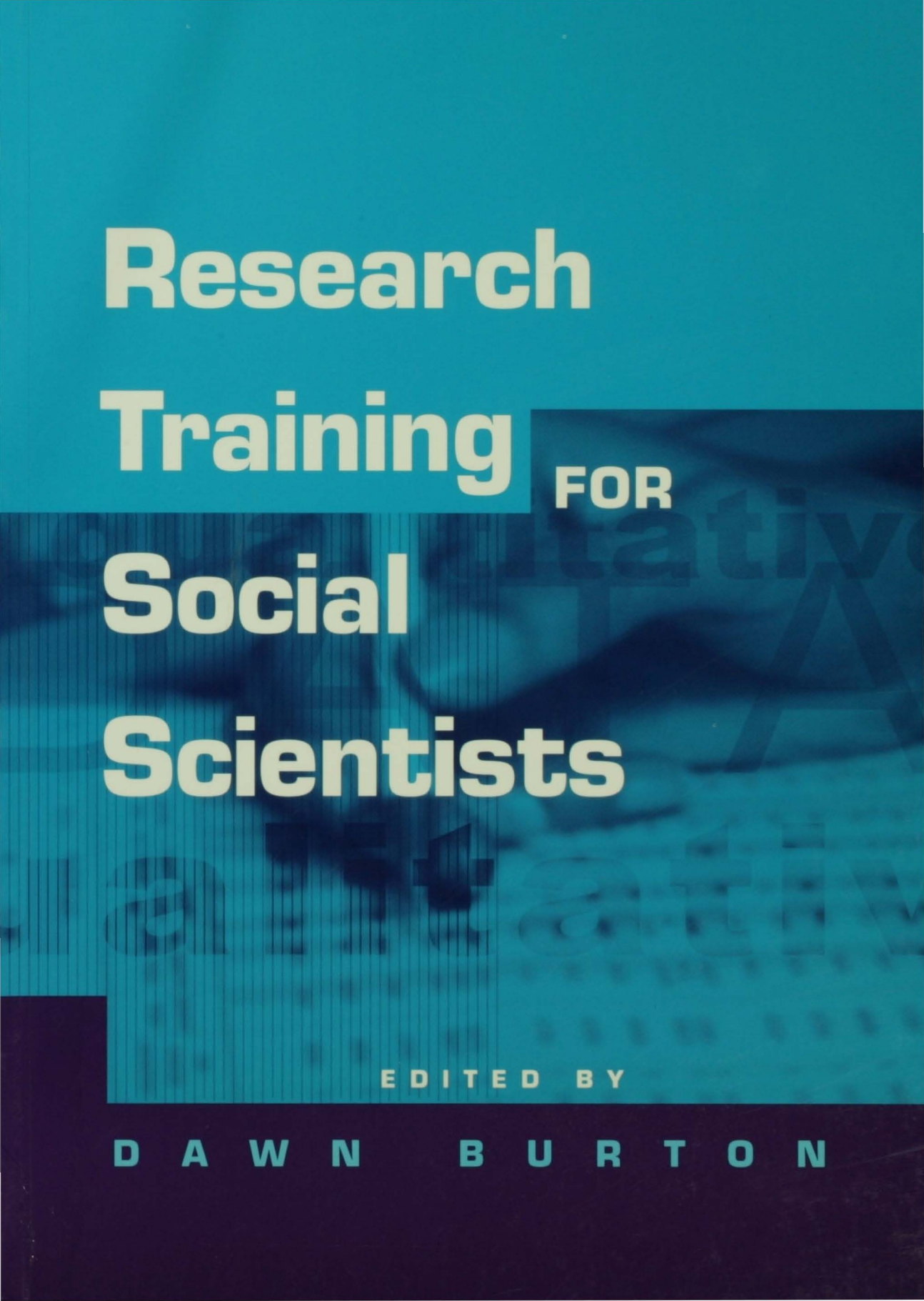




Research Training FOR Social Scientists

EDITED BY

D A W N B U R T O N

Research Training
for
Social Scientists

Research Training for Social Scientists

A Handbook for
Postgraduate Researchers

edited by

DAWN BURTON



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Peter K. Smith is Professor of Psychology at Goldsmiths College, University of London, having earlier been Professor at the University of Sheffield. Besides interests in the philosophy of science, he has researched extensively in the area of children's development and co-authored *Understanding Children's Behaviour* with Helen Cowie (Blackwells, 1988, 1991). His work on school bullying resulted in two books edited with Sonia Sharp, *School Bullying: Insights and Perspectives* and *Tackling Bullying in Your School* (both Routledge, 1994). He co-edited *Theories of Theories of the Mind* with Peter Carruthers (CUP, 1996).

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Paul White is Professor of Geography at Sheffield University having taught there since 1974. He has also held temporary appointments at the universities of Oxford, Paris-Sorbonne, Zaragoza (Spain) and Cagliari (Italy). His research interests are in international migration and ethnicity, particularly relating to urban areas in Europe. He has supervised over fifteen successful PhD students, and has wide experience as an external examiner for doctorates in both the UK and France.

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When I was appointed as Director of Social Science Research Training at the University of Sheffield in 1994, it was my first full-time academic appointment. It was a considerable task for me to take on the responsibility for reorganizing, developing and managing a programme which involved significant numbers of research students drawn from sixteen different departments and research centres. The task would not have been possible without the help and support of a considerable number of my social science colleagues, some of whom have contributed to this book and from whom I learned a great deal. It is a testament to their supportive attitude towards research training and the view that experienced researchers should pass on their experiences, both good and bad, that this book was developed. It has taken an enormous amount of effort to get the text written and edited and I hope that everyone concerned thinks it has all been worthwhile. I sincerely wish to thank them all for their help, kindness and good humour. A special thanks must go to Katherine Steele who is still the administrator and secretary in the Social Sciences Research Training Office and who worked so diligently to keep the Office in order.

FOREWORD

Research training has become a hallmark of doctoral work in the social sciences during the 1990s. In the early part of the decade attention was given to filling a gap in understanding and using research methodologies. The emphasis was on bringing all social scientists who held a research studentship from the Economic and Social Research Council (ESRC) up to a minimum standard whereby they became acquainted with a range of methodologies.

Such was the success of this form of doctoral training that many social scientists argued that these courses were an essential part of doctoral work for all students. The programmes on offer covered philosophy of the social sciences, quantitative and qualitative research strategies and methods, ethical issues of social research, strategies of data analysis and sessions on writing and dissemination.

Much of this material has been taught in the first year of doctoral training (for full-time students) or the first two years (for part-time students). The challenge for those who teach these courses has been to find an approach that covers the field while providing relevant material for the doctoral student. A further problem that looms large is whether the training courses provided are *postgraduate in level*.

In addressing these issues doctoral students and their teachers demand a range of suitable material. It is rare to find a comprehensive range of material on social research methodology brought together in one volume. Yet Dawn Burton has assembled a collection that provides a comprehensive discussion on a range of issues together with detailed bibliographies that will assist students to develop their knowledge in depth.

Overall, this volume provides a starting point for doctoral students who wish to embark on research training that is relevant for postgraduate study.

Robert G. Burgess

Vice-Chancellor, University of Leicester

Chairman, Postgraduate Training Board,
Economic & Social Research Council

INTRODUCTION

This text is based on the Social Science Research Training Programme which was developed for first-year research students at the University of Sheffield. The chapters are based on distance learning units which were originally designed to fulfil the research training needs of part-time research students who were unable to attend the taught courses. The book provides guidance on research training which the Economic and Social Research Council (ESRC) believes is appropriate for first-year research students in all social science disciplines. Although it is clearly not the case that all students in British universities studying for a PhD are funded by the ESRC, or any other sponsor for that matter, the guidelines are considered to be the kitemark of quality and are therefore widely adhered to. Since the ESRC guidelines are extensively used in Britain as a benchmark for research training, the text is representative of research training practices in most British universities. The organization of the book reflects the knowledge and skills that are required of social scientists in order to become competent and effective researchers.

The text is written in an accessible style to meet the needs of students from a wide variety of social science backgrounds who often have highly varying degrees of research experience and expertise. The book is deliberately wide ranging in its coverage and specifically designed as a one-stop text. The range of training issues covered makes it distinctive and hopefully good value for money. The book is constructed so that it parallels the research process. It begins with some discussion of ethical and philosophical issues as they relate to social science research. Following this, students are taken through the process of using relevant literature to develop social research projects. Quantitative and qualitative data collection and analysis techniques are discussed in considerable depth. The final section includes material about how to write up and present research projects. The book therefore reflects the life cycle of the research project and will hopefully help students to develop their own research projects in addition to providing advice on particular aspects of researching for a PhD.

The extensive range of issues covered by the book make it distinctive, but this was not the reason behind the development of the text. It was the concern of many academics involved in the research training programme that there was not an appropriate text available to meet the needs of research students. Many of the existing texts provide a very prescriptive analysis of the process of undertaking research. This approach is not

particularly useful for research students because it can often give the impression that research is a relatively unproblematic activity. This is clearly an unrealistic view and one which needs counteracting. The purpose of this text is to provide readers with a realistic view of how social science research is conducted. This includes owning up to mistakes so that others can learn from them in addition to documenting good practice. In summary we all hope that the book will go some way to making the research process more transparent. While the text is specifically designed for research students, we anticipate that the book will be useful for all students who are trying to get to grips with the exciting and demanding task of undertaking research in the social sciences.

PART I

PHILOSOPHY OF THE SOCIAL SCIENCES

Research students often question why they are required to have an understanding of the philosophy of the social sciences literature as part of their research training. Training is often associated with the acquisition of research skills that will help students collect, process and analyse data, and not to study philosophical issues which are often regarded as marginal and rather a waste of time. According to this interpretation, social science research skills should be acquired and then applied as though they were the skills of a craft. What this view fails to acknowledge is that philosophical assumptions about human nature and how society is conceptualized are directly related to issues about social research, whether that be the nature and status of data that is collected and the validity of the methods by which data is analysed, interpreted and understood. To quote Hughes (1993: 11) 'Whether they may be treated as such or not, research instruments and methods cannot be divorced from theory; as research tools they operate only within a given set of assumptions about the nature of society, the nature of human beings, the relationship between the two and how they may be known.'

Methodological debates in the social sciences cannot be understood without reference to the wider cultural setting in which those discussions take place. It is also arguably the case that the social sciences have not been as successful in their ability to produce analyses of the social world as the natural sciences have of the natural world. It is therefore not surprising that a significant amount of energy has been devoted to comparing the methodology of the 'natural' and the social sciences. Whether the social sciences are in fact 'sciences' is a controversial issue. Peter Smith addresses this debate in the first chapter by assessing the philosophy of science literature and exploring its relevance for the social sciences. A number of features are discussed, specifically: what is meant by philosophy of science; whether social scientists can productively apply methods used by scientists, whether they can be modified in some way, or if they should be rejected outright.

The focus of Chapter 2 by Nick Stevenson is hermeneutics. Although rather a grand word, hermeneutics is simply another way of referring to the process of interpretation. Interpretivist or 'humanistic' views of social behaviour are in direct opposition to positivistic, 'scientific' notions of human behaviour which reduce social life to the interaction of variables. The negative consequences of the positivistic view of human behaviour are that it provides only a partial account of social life and distorts the nature of human interaction in profound ways. Nick discusses the ways in which understanding and interpretation are bound up with linguistic practices; the impact of symbolic culture on the nature of interpretation; and ways in which hermeneutics are likely to become more rather than less important in the future.

In Chapter 3 Sue Webb evaluates some of the main issues associated with feminist methodologies for social researching. Some of the main philosophical issues raised by feminists about social research and why feminist discussions should be perceived as an important contribution to contemporary debates are considered. How feminists have distinguished their research activities from others and reasons why they have taken this turn are also evaluated, along with feminist debates about quantitative and qualitative methods and the issue of methodological pluralism.

Peter Jackson's chapter on race and racism provides a welcome contribution to the philosophy of the social sciences debate, since it is an area which is very often neglected. A focal point of the discussion involves evaluating essentialist versus social constructionist approaches to 'race'. He argues against an essentialist view of 'race' which is highly biologically deterministic, in favour of understanding 'race' as being socially constructed across time and space. He suggests that the concepts of race and racism cannot be divorced from the wider politics of 'race' in society which in turn raises questions about the neutrality of social science researchers and the need for more committed approaches while simultaneously retaining intellectual integrity. This should be read in conjunction with Chapter 13 by Wanda Thomas Bernard, who deliberately chose participative research methods because of their emancipatory potential in her research with black men in Britain and Canada.

SUPPLEMENTARY READING

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PHILOSOPHY OF SCIENCE AND ITS RELEVANCE FOR THE SOCIAL SCIENCES

Peter K. Smith

Are the social sciences really sciences, or is this a misnomer? In the UK, the Social Science Research Council (SSRC) had to change its name to the Economic and Social Research Council (ESRC), in part because of a belief in government at the time that the social sciences were not sciences. And academics also debate these issues. This is not just a matter of a name – it affects how we carry out our research and what we think is the status of ‘facts’, ‘evidence’ and ‘theories’. These are issues which confront all researchers. At times, we may get by unthinkingly, doing as colleagues have done previously; but we may also be challenged – by new ideas or by other disciplines – and these issues will come to the forefront.

As part of such considerations, it is important for social scientists to understand something of the ‘philosophy of science’ even as it applies to the traditional ‘hard’ sciences, the physical sciences especially, and the biological and earth sciences. After all, a significant part of the debate about procedures in the social sciences is whether we can profitably apply – or whether instead we routinely misapply – methods and procedures from the physical sciences. Often, too, the procedures of the physical sciences are misunderstood. So, it is very relevant to know how the traditional sciences work, or are thought to work, whether we as social scientists then imitate these methods, modify them, or reject them.

In this chapter I will define what is meant by Philosophy of Science and give a brief historical survey of the main issues. I will review the traditional ‘inductivist’ view of science, the hypothetico-deductive view of Popper, and the alternative views of Kuhn and Lakatos, including more recent critiques and ideas (see also Chalmers, 1982; Hacking, 1981; Losee, 1980).

WHAT IS MEANT BY PHILOSOPHY OF SCIENCE?

The philosophy of science is concerned with questions such as:

- 1 What characteristics distinguish science from non-science?
- 2 What procedures should scientists follow?

- 3 What conditions must be satisfied for a scientific explanation to be correct?
- 4 What is the cognitive status of scientific laws and principles? (See Losee, 1980: 2.)

These are fundamental questions: (1) and (2) are definitional for the scientific method; (3) and (4) may seem more abstract, but they too are fundamental.

Question (4) was a matter of life and death in the case of Galileo. Traditionally (and following obvious perceptual information) people believed that the sun revolved round the earth. Following the work of Copernicus, Galileo (1564–1642) argued that in fact the earth revolved round the sun. This brought him into conflict with the Catholic Church at the time. In 1615 Cardinal Bellarmine corresponded with Galileo about this. It is permissible, he said to Galileo, for you to argue that the earth revolving around the sun is a possible mathematical model; in fact, it is even permissible for you to argue that it is the best model at the moment; but you must not say that it is actually, physically, true. Despite this warning, Galileo continued to assert that it was true. In 1633 the Inquisition condemned Galileo's views, which he subsequently recanted. Only in 1992, after 359 years, did the Catholic Church admit it was wrong to condemn him. This was a debate about the status of a scientific law. Interestingly, most modern philosophers of science would accept the 'best model' compromise without qualms, rather than insisting on an actual physical truth which, ultimately, is provisional rather than certain.

As a discipline, the philosophy of science is related to other areas, notably:

- *the history of science* – how science has actually developed, whatever the 'ideal' science might do;
- *the sociology of knowledge* – how social structures and institutions, scientific societies and journals and the social networks of individual scientists affect the growth of science;
- *the psychology of research* – how individual scientists develop ways of thinking about and interpreting the world; pressures for conformity and bursts of creativity.

Of twentieth-century philosophers of science, Kuhn has reached out to the history of science and the sociology of knowledge. However, the origins of the philosophy of science go back long before the twentieth century.

HISTORICAL ORIGINS OF THE PHILOSOPHY OF SCIENCE

Systematic writing about the philosophy of science can be dated back to the ancient Greeks. Aristotle (384–322 BC) provided a foundation for

speculating about 'the nature of things' which had an enduring influence. In particular, he had an 'inductive-deductive' view of how we obtained systematic knowledge. According to this, we first 'induce' certain regularities in the world around us. For example, we might notice the regularity of flowering plants in springtime; we 'deduce' that next spring the plants will flower again. This very simple example could be made more sophisticated by induced explanatory principles such as the effects of rain and sun on plants. We could then deduce that a drought or lack of sunshine will prevent or delay plant growth in the spring.

Aristotle also started the consideration of what is meant by causality. Looking at the regularities or 'correlations' in observed phenomena, he clearly distinguished between accidental correlations and causal correlations. As an example of an accidental correlation, at the time of year when plants start blossoming, birds start singing (plant blossom does not cause birds to sing; bird song does not cause plants to blossom; both are caused by the increase in temperature and hours of daylight during spring). As an example of a causal correlation, when we feel the wind blowing strongly, we see clouds scudding across the sky (the same wind which blows on us also causes the clouds to move). However, Aristotle was not an experimentalist. As we shall see, the role of experiments was a gradual, later development in the philosophy of science, linked to the greater importance given to deductivism.

The works of Aristotle and other Greek philosophers from the classical period were translated from Greek into Latin and Arabic (since Arab philosophers kept these works alive during the European 'dark ages'). Latin translations of Aristotle's writings on science became available to European philosophers as learning revived during the twelfth and thirteenth centuries.

Roger Bacon (1214–1292), for example, affirmed Aristotle's 'inductive-deductive' pattern of scientific inquiry, but took it one stage further. Bacon argued that the factual base available for induction to operate on could be augmented by active experimentation on the world. At the time there was much interest in magnetism (and its possible uses in compasses and for navigation). What happens if you break a magnetic bar or needle? You get two magnets, each with its own N and S poles. These simple kinds of 'experimentation' would be useful, Bacon argued. Note, however, that Bacon was not testing any theory here; rather, this was experimentation to 'see what happens'. Bacon and a few other philosophers at the time did begin to point to the need to test exploratory principles arrived at by induction, but this did not proceed very far.

In fact, another tradition from classical Greek writings laid the foundation for hypothesis testing and experimentation. Euclid (c.300 BC) and Archimedes (287–212 BC) developed the idea of axioms, or hypotheses, in mathematics and geometry. Given certain axioms, then certain consequences follow – hypothesis and deduction. But this approach was used in the abstract realm of mathematics. In the seventeenth century, Descartes

(1596–1650) elaborated this hypothetico-deductive method and laid the groundwork for its application in science. But it was not until the twentieth century that this hypothetico-deductive approach became central in the understanding of science, together with a full appreciation of the role of experimentation in actively testing hypotheses.

THE TRADITIONAL 'INDUCTIVIST' VIEW OF SCIENCE

Aristotle's view came under more critical scrutiny as the philosophy of science developed in modern Europe. By and large, the inductivist view of science held sway and was further augmented. Among many contributions, we can take John Stuart Mill (1806–1873) as a prominent example from the nineteenth century. Mill argued that there were four primary inductive methods which could be used (for example, to distinguish accidental and causal correlations). These were agreement, difference, concomitant variations and residues.

As a fictional illustration of this in the social sciences, suppose we have induced a correlation between having the death penalty for murder and a reduced number of homicides. According to Mill, we could infer causation – that hanging deters homicides: if there are few homicides at times/places where the death penalty is enforced (*agreement*); there are many homicides at times/places where the death penalty is not enforced (*difference*); there are fewer homicides when the death penalty is enforced strictly and more when it is interpreted more leniently (*concomitant variations*); and presence/absence or variation in other possible causes (e.g. unemployment, marital instability) do not affect the number of homicides (*residues*).

Mill argued that the processes of inference and induction, could lead us to deduce causal relations. If these were verified – if they explained observations and other causal relations did not – then we could regard the hypothesis as verified. Mill cited Newton's inverse square law of force (that the gravitational attraction between two bodies reduces as the square of the distance between them – a crucial part of explaining planetary motion) as an example of a completely verified law. This law could then be considered 'true' in some absolute sense.

Mill's work epitomizes the 'traditional' or 'inductivist' view of the scientific method. In brief, this holds that science proceeds by collecting factual data through observation and by experimentation which serves to increase the observational data base. By inductive methods, generalizations and causal laws could be arrived at. In principle, induced laws could be completely verified if all the deductions from them were correct. This view held sway in many quarters well into the twentieth century. For example, Karl Pearson (who developed the well-known product-moment correlation coefficient) wrote: 'the classification of facts and the formation of absolute judgments upon the basis of this classification . . . *essentially sum up the aim and method of modern science*' (1892: 6; author emphasis).

A crucial part of the traditional view is that *hypothesis follows observation* (this refers to procedures, question 2 of our four questions at the start of the chapter). It also argues that *we can achieve completely verifiable, 'true' theories* (this refers to questions 3 and 4). Yet few modern philosophers of science accept either of these conclusions. In fact, most would argue that *hypothesis precedes observation* and that *we cannot achieve completely verifiable, 'true' theories*. Thus, the 'traditional' view has come to be radically overthrown.

THE HYPOTHETICO-DEDUCTIVE VIEW OF POPPER

Karl Popper (1902–1994) has been one of the most well-known philosophers of science to attack the traditional view and to establish an alternative, hypothetico-deductive view (Popper, 1959, 1963, 1976, 1979, 1986; see Magee, 1982). Another well-known figure who has propounded similar views to Popper, is Peter Medawar (1915–1988). This hypothetico-deductive view also has a long intellectual history and as an example we can consider statements by Charles Darwin in the 1860s (quoted in Medawar, 1969: 11): 'I have an old belief that a good observer really means a good theorist' and 'how odd it is that anyone should not see that all observations must be for or against some view if it is to be of any service'.

From Darwin's notebooks we know that he was formulating ideas about evolution well before the publication of *The Origin of Species* in 1859. Even during the voyage of HMS *Beagle* (1831–6) he may have been directing his observations towards testing ideas that were fermenting in his mind. As Medawar (1969) put it, 'we cannot browse over the field of nature like cows at pasture'. If Darwin had gathered data randomly, this would not have provided nearly such good evidence for natural selection as the systematic data he did collect – on where fossils were found, on how the beaks of finches varied in different habitats, and so forth – which allowed him to confirm or disconfirm his hypotheses.

Popper holds that science and knowledge progress by advancing hypotheses, making deductions from them and using observations and experiments continually to test these deductions until they are falsified; then revising or changing the hypothesis to cope with this. (Note the increased role of experiments here explicitly to test hypotheses.) Hence, the hypothetico-deductive method: Hypotheses come first and observations follow; 'observations are interpretations . . . in the light of theories'. It is easy to underestimate the importance of this view, which is fundamentally different from the traditional view and itself leads to other differences. Essentially, it is saying that we do not collect facts, as Pearson had implied; we do not gather unbiased observations. Rather, we interpret our observations in the light of biases, preconceptions, hypotheses and theories. We choose which aspects of incoming stimuli to attend to and what interpretation to put on them. This view relates to some modern ideas on the psychology of perception and the psychology of development.



FIGURE 1.1 Ambiguous figures: old lady or young lady?

Psychology of perception

The study of ambiguous figures, as in Escher drawings and visual illusions, or the interpretation of minimal sketches or cartoons shows that the human brain actively interprets visual (and other sensory) stimuli. Depending on our expectations and previous experiences, different people may experience a certain stimulus in different ways – as an old lady or a young lady in Figure 1.1, for example. Thus, preconceptions are biasing our observation.

Psychology of development

There is a developmental history to our preconceptions. Ultimately, there is an evolutionary history in that our sensory systems themselves are 'biased' to respond to certain kinds of external signals (e.g. wavelengths in the visible light range; sounds in our audible range) because the 'hypotheses' that such signals were important were successful in the natural selection of our ancestors. Looking at individual development, a human baby has biases about which kinds of stimuli to attend to and readily develops hypotheses about human faces, about depth and about causal relationships. Jean Piaget's theory of cognitive development views the developing child as trying out hypotheses in the world. This is very explicit in his 'formal operational thinking' stage of adolescence, which is itself very similar to a Popperian view of scientific method in its testing of hypotheses. The individual scientist can be viewed as someone who carries out formal operational thinking systematically and consistently in his or her scientific domain.

Thus, Popper is saying that observation is 'theory laden'; that is, there are always hypotheses implicit or explicit in observation (even, ultimately, back to innate perceptual hypotheses in the newborn infant). However, this implies a different status to 'facts' than Pearson and the traditional thinkers had in mind. Facts or observations are open to reinterpretation in the light of a different theory. Also, a theory may fit the observations now, but future observations may disprove it – the deductions from a theory, even if satisfied now, may not always be satisfied. This leads to two related points:

- 1 A theory can never be verified in the sense of proved correct, but it can be falsified.
- 2 All knowledge is provisional, there is no absolute truth, but we can prefer one theory over another.

In forming these views, it is very likely that Popper was influenced by developments in physics at the beginning of the twentieth century. For a long time, Newtonian physics held sway and seemed to provide a perfect explanation of force and motion and, via the inverse square law of gravitational attraction, of the motion of the planets and comets in the solar system. The predictions of this theory seemed very well confirmed. Yet there were a few anomalies, for example, the detailed orbit of Mercury, the innermost planet, was not exactly as predicted. Other difficulties were discussed. Physics entered a period of ferment at the turn of the twentieth century, which was resolved when Einstein's theories of special and general relativity (as well as the theories of quantum mechanics developed by Schrodinger, Heisenberg, and others) provided a totally new and different basis for understanding physical reality. In particular, Einstein's theories replaced Newton's as a basis for predicting planetary motion and did so better, for example, correctly predicting the orbit of Mercury.

If a theory as apparently well established as Newton's could be overthrown, what theory was safe? This was a dramatic example of how many prior confirmations do not safeguard a theory against future refutation. Effectively, Einstein had falsified Newton's theory. This did not mean that Einstein had achieved absolute truth – perhaps his theories will be overthrown in the future – but he had provided a better explanation than Newton.

This illustrates the final crucial point about Popper's view – that theories cannot be proved, but can be falsified, and that falsifiability is the criterion separating science from non-science. Using this criterion, Popper addressed directly the first of our questions at the start of the chapter, providing a 'demarcation criterion' which he claims can be used to separate (or demarcate) the sciences from the non-sciences. One can falsify Newton's theory, but one cannot falsify a painting. Paintings can be beautiful and valuable, but they are not science.

Popper placed much (scientific) value on falsifiability. He argued that highly falsifiable theories should be preferred to less falsifiable ones –

provided, of course, that they had not actually been falsified. Also, scientists should try to falsify their theories, rather than confirm them: 'the wrong view of science betrays itself in the craving to be right'. Planned experiments have a crucial role in attempts at falsification, potentially deciding whether one theory or perhaps another can be disproved. Using his falsifiability criterion, he also attacked two prominent theories in the social sciences – psychoanalysis and Marxism.

Popper and psychoanalysis

Popper regarded psychoanalysis as non-science by the falsifiability criterion. (In fact, he regarded psychology generally as 'riddled with fashions, and with uncontrolled dogmas'.) He argued that psychoanalytic ideas could be used to explain any example of human behaviour; thus, they could not be falsified; therefore, psychoanalysis was not science. Grünbaum (1979) argued that, in fact, Freud's theories could make predictions which were in principle falsifiable; for example, that early severe toilet training would lead to an anal personality; and that some of these predictions were confirmed (not falsified) by cross-cultural studies.

However, Popper (1986) reiterated that 'every conceivable case [of human behaviour] could be interpreted in the light of Freud's theory'. He gave as example an argument attributed to Grünbaum, that a Freudian prediction is that 'if people do not repress traumatic experiences, then they will not become victims of neurosis'. Popper argues that this is untestable since who decides what is traumatic, and what is repressed: 'Who has never been hurt, never suffered a trauma? And who has not tried to get over it by forgetting about it – which means 'trying to repress it'? But if so . . . all [such] so-called predictions are untestable.'

Popper and Marxism

Popper attacked Marxism, or Marx's theories, not for being unfalsifiable but because Marxists ignored the falsifications which had happened. In *The Open Society and its Enemies* (1945), Popper argued that Marxism predicted that only fully developed capitalist economies would become Communist (falsified: the principal Communist countries such as Russia and China were pre-industrial); Communist revolutions would be based on the industrial proletariat (falsified: Mao Zedong's revolution in China relied on the peasantry); capitalism should, through its own contradictions, lead to increased inequality, crisis and revolution (falsified: many capitalist countries have achieved less inequality through social democratic governments). Marxists ignore these falsifications and find 'excuses' to preserve the theory. Thus, according to Popper, Marxists abandon pretensions to be scientific. Popper more fundamentally criticized 'historicism' in

the sense of any preordained prediction of history, given what he saw as our ability and responsibility to control our own destiny.

Popper's views have become very influential, not only in the physical sciences but in some areas of the social sciences. His views are a radical alternative to the traditional view and the emphasis on requiring prior hypotheses and then attempting to falsify them may seem a refreshing antidote to a lot of pseudo-science. However, it is worth noting that some traditions in science and in social science are not too compatible with this. In science, consider the work of Konrad Lorenz and Niko Tinbergen, Nobel prize winners in ethology (the study of animal behaviour). Ethologists emphasized the importance of getting rid of preconceptions when studying a species of animal. In order to enter the animals own experienced environment, or *umwelt*, one should try to discard (so far as possible) one's anthropomorphic expectations. Ethologists would acknowledge that prior hypotheses bias our perceptions, as Popper does, but unlike Popper they would see this as a hindrance rather than an advantage. They would argue that theories should emerge later, from immersion in the data.

This view shows correspondences to that of grounded theory in the social sciences. This is one way of treating qualitative data obtained from people through observation and/or interview. Again, a considered aim of this approach is that concepts, and subsequently theories, should emerge from an (as far as possible) unbiased immersion in what the environment or setting throws up in the way of data. Hypothesis testing is not rejected, but the intention is to go some way to the induction-deduction-testing cycle rather than the straightforward deduction-testing cycle which Popper espouses.

ALTERNATIVE VIEWS OF KUHN

Popper's views have also been criticized more directly within the philosophy of science literature. A major protagonist to Popper has been Thomas Kuhn (1922–98), especially in his key work *The Structure of Scientific Revolutions* (1970). Kuhn agreed with Popper (and most other recent philosophers of science) in seeing observation as 'theory laden', and science as a problem-solving activity which cannot arrive at an absolutely verifiable truth. However, he disagreed about the role of falsifiability and about the criteria demarcating science and non-science. The main thrust of his view of science is summarized in Figure 1.2.

In his work, Kuhn paid much more attention than Popper to the history of science and the way in which scientists have actually worked. For example, he drew particularly on the development of modern chemistry from the earlier work of alchemists, as well as the development of physics. Kuhn characterized a mature branch of science as having an accepted 'paradigm' (a basic set of assumptions, or ways of problem solving). In a very early

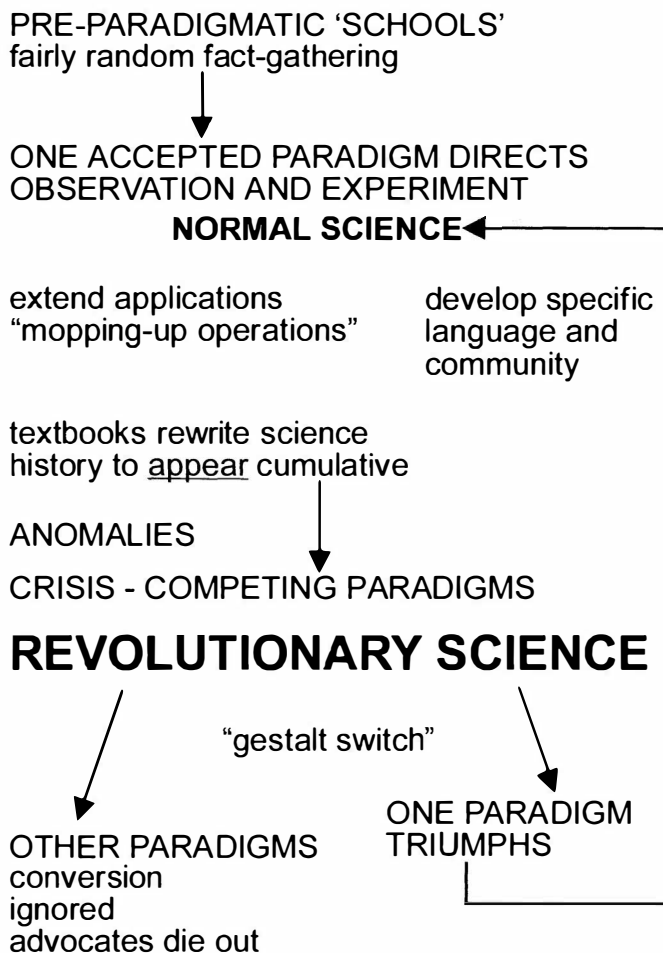


FIGURE 1.2 A diagrammatic view of Kuhn's view of science

stage, a discipline might be pre-paradigmatic, characterized by many schools which quarrel about fundamental issues, and by rather random fact gathering. With maturity, one paradigm is accepted and directs observation and experiment.

As an example of this shift, Kuhn pointed to the alchemists as being pre-paradigmatic. With the vague aim of seeking life-enhancing elixirs, or turning base metals to gold, they mixed anything to see what happened. Chemistry only became a science when Dalton proposed his atomic theory, and the concepts of particular elements made up of atoms of different atomic numbers and weights (which could not be transmuted). This provided a basic set of assumptions which could guide future work – the identification of missing elements, the examination of how particular elements combined, etc.

Kuhn here has advanced a demarcation criterion between science and non-science (question 2, at the start of the chapter), but one different from Popper's: a field is a science if it has a paradigm. Science is distinguished from non-science by being a problem-solving activity with an accepted paradigm.

This period of chemistry (or any other area where a paradigm becomes accepted, for example, Newtonian theories in physics, plate tectonics in geology) would be an example of what Kuhn calls 'normal science' (see Figure 1.2). This is a period of 'mopping-up operations' in which paradigm applications are extended. 'Mopping-up operations are what engage most scientists throughout their careers' (Kuhn, 1970: 22). Methodologies are developed and characteristic jargon appears which is accessible to those in the paradigm but not to others (in chemistry such terminology would be 'element', 'atom', atomic weight', 'atomic number', etc.).

Interestingly, Kuhn also looked at how normal science is transmitted to the wider community and taught to the next generation. He argued that textbooks characteristically reinterpret past history as leading to the current paradigm. For example, chemistry textbooks may portray the work of alchemists as leading to modern chemistry. Physics textbooks may portray Newtonian physics as leading on to Einstein's theories. In fact, Kuhn argued, these transition points are much more chaotic and unfocused than such simplified accounts would suggest.

Normal science continues, despite the existence of anomalies or falsifying instances. Kuhn argued that scientists are quite content to ignore difficulties while developing a new paradigm (for example, early chemists had to ignore the problem that certain elements had varying atomic weights which were not whole numbers – an anomaly only solved by recognizing the existence of isotopes, elements of the same atomic number but different atomic weight, itself only fully explicable by nuclear theory, much later). The paradigm is doing well generally, so why reject it because it temporarily fails in some areas?

The paradigm is not rejected unless, apart from anomalies accumulating, a potentially superior paradigm appears. 'To be accepted as a paradigm, a theory must seem better than its competitors, but it need not, and in fact never does, explain all the facts with which it can be confronted' (Kuhn, 1970: 17–18). This view is different from Popper's; rather than trying to falsify their theory, Kuhn suggested not only that scientists avoid falsifying their theories, but that this is a necessary part of normal science.

As can be seen, the role of falsifiability is limited in Kuhn's approach. All paradigms always have anomalies. But Kuhn obviously had to recognize that theories are sometimes falsified, or disproved; like Popper, he was very aware of Einstein's overthrowing of Newton's ideas. However, Kuhn described such events as a period of 'revolutionary science' (see Figure 1.2). Such a period comes about when an accepted paradigm, despite a period of development, has not dealt with anomalies, and indeed anomalies have begun to accumulate to an embarrassing extent. At this

point, competing paradigms may appear. There will be a period of some confusion or chaos, as the previous paradigm loses adherents, but no one new paradigm predominates. Eventually, one paradigm triumphs, in part through resolving some anomalies, but also perhaps through making some new successful predictions or appearing more precise or elegant. This new, triumphant paradigm then becomes 'normal science' in its turn (Figure 1.2). Adherents of the older, or alternative, paradigms are converted, ignored, or eventually die out.

Kuhn argued that each paradigm embodies such different assumptions that a 'gestalt switch' in perception is needed to move from one to the other. Just as in Figure 1.1 the switch from 'old woman' to 'young woman' requires a sudden, complete reinterpretation of the same stimulus information, so (Kuhn argued) a paradigm shift (for example, from Newtonian to Einsteinian theory) requires a complete reconceptualization, in new language, of information previously interpreted in the old paradigm. Kuhn also stated that competing paradigms are 'not only incompatible but often actually incommensurable' (1970: 103), that is, only partially comparable in logical terms. Proponents of different paradigms characteristically 'argue past each other', employing such different language and different basic assumptions that meaningful dialogue is difficult if not impossible.

Kuhn's work has inspired many thinkers in the social sciences who have taken up the idea of 'paradigm' enthusiastically, claiming that their area represents a new paradigm for their discipline (see Lambie, 1991; Peterson, 1981). However, Kuhn saw 'controversies over fundamentals' as 'endemic among, say, psychologists and sociologists' (1970: viii). He seems to have viewed the social sciences as at an early, pre-paradigmatic stage in science, though this was not discussed in depth in his writings.

In fact, the nature of a 'paradigm' has been one of the two major criticisms made of Kuhn's work (e.g. Lakatos, 1970). Kuhn defined paradigms as 'universally recognised scientific achievements that for a time provide model problems and solutions to a community of practitioners' (1970: viii). Besides the circularity of including 'scientific' in this definition of science, it has been pointed out that Kuhn provides many other explanations of 'paradigm' in his 1962 book, which vary appreciably (some 111 different definitions, according to Masterman, in Lakatos and Musgrave, 1970). Is it a grand theory, a localized hypothesis, a new tool or technique? Kuhn recognized the force of this criticism and in the Postscript to the second edition of his book (1970) distinguished between a more global meaning as 'disciplinary matrix', or network of shared conceptual assumptions; and a more localized meaning as 'exemplar' or useful problem-solving methodology.

The other main criticism of Kuhn's work related to the nature of paradigm change. Was Kuhn saying that such change was rational, or was it more due to fashion and social pressure? For Popper (as for more traditional philosophers of science), scientific change was seen as progress. New

theories were more powerful than old ones, encompassing more known observations and successfully predicting new findings. For Kuhn, it was not so clear cut. Certainly, a paradigm gets into trouble when anomalies accumulate. But Kuhn stated that all paradigms have anomalies and falsification is in reality not used as a primary criterion for paradigm rejection. He states (1970: 8) that 'competition between segments of the scientific community is the only historical process that ever actually results in the rejection of one previously accepted theory or in the adoption of another'. This, together with the incommensurable nature of different paradigms, leaves open the door to suggestions that a paradigm shift occurs because one group of scientists is just more powerful or has more social influence than another.

Clearly science is influenced by social pressures. The history of Lysenkoism in the former Soviet Union (in which Lysenko, through his influence, promulgated what are widely held to be false ideas of effects of plant breeding) is one example. This and other examples could be dismissed as aberrations in science. Kuhn's work, however, has opened up a debate as to whether even normal science and the competition between paradigms is strongly influenced by such social forces. This debate has obvious relevance to the social sciences. So far as the philosophy of science is concerned, this debate has led to a defence of rationality in science (e.g. Lakatos, Bunge) and to further attacks (e.g. Feyerabend, Collins, and recent sociologists of science).

VIEWS OF LAKATOS AND OTHERS

A compromise between the positions of Popper and Kuhn was advanced by Imre Lakatos (1922–74). Lakatos agreed with Kuhn that Popper was wrong in emphasizing falsification as the demarcation criterion between science and non-science; but he wished to reject the relativism that Kuhn was near to espousing. Lakatos sought for ways to keep Popper's ideas of scientific progress, while retaining Kuhn's insights into how science actually changes (Lakatos, 1970).

Lakatos distinguished three kinds of falsificationism in science:

- 1 *Dogmatic falsificationism*: this would be the repeated overthrow of theories by 'facts'; a single disconfirmation would lead to a theory being discarded. (This would clearly be pointless as a disconfirmation might later be shown to be due to some mistake, such as measurement error or faulty procedure. Popper in fact recognized this and was not as guilty of it as some of his critics imply.)
- 2 *Naive methodological falsificationism*: to safeguard premature rejection of theories, Popper says that 'criteria of refutation have to be laid down beforehand'. But Lakatos (like Kuhn) does not believe that science actually progresses this way.

SCIENTIFIC RESEARCH PROGRAMME

PROTECTIVE BELT OF AUXILIARY HYPOTHESES

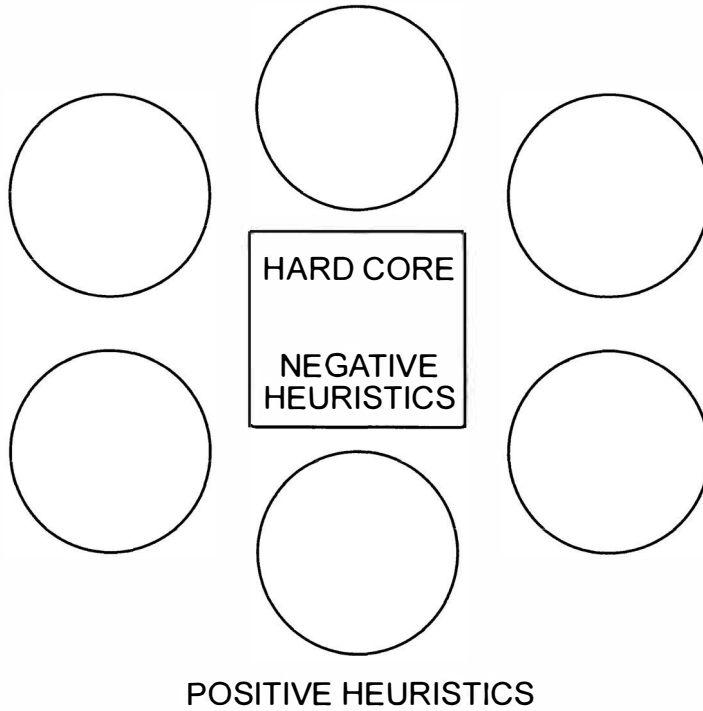


FIGURE 1.3 A schematic view of Lakatos's concept of a scientific research programme

- 3 *Sophisticated methodological falsificationism*: according to Lakatos 'there is no falsification before the emergence of a better theory'. A theory T is falsified only if a new theory T' explains all the unrefuted content of T and makes further predictions, some of which are corroborated.

Lakatos's key concept was that of a 'scientific research programme' (see Figure 1.3). A research programme encompasses a set of theories and methods which can change over time. However, there is a 'hard core' of very basic theoretical assumptions which do not change (for example, in chemistry, this could be atomic theory). This hard core would not be questioned by adherents of the research programme; indeed, they would defend them vigorously ('negative heuristics'). However, the hard core would have generated a range of 'auxiliary hypotheses'. These are much more open to question. They are the frontiers of the research programme,

ideas which are being tried out and tested for their range and explanatory power. If they prove successful, well and good. If they do not, they will be jettisoned, or changed to accommodate anomalies or falsifications, without threatening the 'hard core'. These 'positive heuristics' of active testing and questioning complement the 'negative heuristics' used to protect the hard core assumptions.

Lakatos then saw mature science as a history of competing research programmes. His is a sophisticated conception of science which seems to advance beyond what Lakatos saw as the naive methodological falsificationism of Popper, retaining the insights of Kuhn into historical processes of competing programmes (similar to Kuhn's 'global' paradigms). Kuhn had emphasized the revolutionary nature of scientific change – to the extent that his approach was caricatured as seeing science as 'a series of widely spaced upheavals separated by lengthy dogmatic intervals' (Watkins, in Lakatos and Musgrave, 1970). Lakatos showed the extent to which each research programme also embodied positive heuristics of testing and debate.

Unlike Kuhn, Lakatos rescued the idea of rational progress, at least in part, by describing research programmes as 'progressive', or 'degenerating'. Rational criteria are available to decide whether a particular research programme is 'progressive' or 'degenerating'. A 'progressive' programme anticipates novel facts and produces novel theories, which have 'heuristic power'; changes in the auxiliary hypotheses are productive. A 'degenerating' programme, by contrast, patches up anomalies in ad hoc ways which do not generalize to other situations; changes in the auxiliary hypotheses are unproductive. Nevertheless, Lakatos concedes that there is necessarily some subjectivity in these judgments and also that, over time, what had seemed a degenerating programme may become progressive again (or vice versa).

These ideas have been applied to the social sciences (Friman *et al.*, 1993; Rowell, 1983). For example, Gholsen and Barker (1985) described traditional learning theory in psychology as a degenerating research programme. One could mount a similar argument for psychoanalysis or Marxism.

Similar ideas have been proposed by Larry Laudan (b. 1941), who argues that even the 'hard core' of a research programme can be modified over time. He also postulates a close and reciprocal influence between the philosophy of science and the history of science (Laudan, 1977).

By contrast, Paul Feyerabend (b. 1924) emphasizes the non-rational aspect of Kuhn's ideas of paradigm conflict. In this relativist position there are no logical grounds for preferring one theory over another one (Feyerabend, 1975). Kuhn's relativist views are developed further in recent trends in the sociology and history of scientific knowledge (e.g. Collins, 1985). This holds that 'the results of scientific experiments are more ambiguous than they are usually taken to be, while theory is more flexible than most people imagine. This means that science can progress only

within communities that can reach consensus about what counts as plausible. Plausibility is a matter of social context so science is “a social construct” and that ‘in a sociological or historical investigation, act as though the world does not constrain what scientists believe about it’ (Collins, 1994).

The view that science is (just) a social construct is of course vigorously debated; for a critique of these views, see Bunge (1991, 1992). As Richard Dawkins put it:

When you take a 747 to an international convention of sociologists or literary critics, the reason you will arrive in one piece is that a lot of western-trained scientists and engineers got their sums right. If it gives you satisfaction to say that the theory of aerodynamics is a social construct that is your privilege, but why do you then entrust travel plans to a Boeing rather than a magic carpet? As I have put it before, show me a cultural relativist at 30,000 feet and I will show you a hypocrite. (Dawkins, 1994: 17–18)

QUESTIONS OF HERMENEUTICS: BEYOND EMPIRICISM AND POST-MODERNISM

Nick Stevenson

To write an introduction to something called 'hermeneutics' might at first glance appear to have little contemporary relevance. In an age that has come to be increasingly dominated by theoretical deconstruction, post-structuralism and post-modernism a concern for hermeneutics might seem old-fashioned. Yet the tradition of hermeneutics already occupies many of the assumptions of these better known trends and, in certain instances, offers powerful corrections to a number of theoretical evasions. On the other hand, much research in the social sciences continues to steer clear of dense theoretical disputes, aiming to give us a clear window on a changing world. Here my argument is that if post-modernism leads us astray then a naive empiricism fares no better. In summary, I claim that a hermeneutic disposition equips us well for a variety of different research agendas.

I want to start with the connection between so-called theory and so-called facts. I think that an encounter with a rich tradition like hermeneutics allows us a qualified scepticism about two positions that have gained a foothold in certain sections of the social sciences. The first is the mistaken view that we can present facts in such a way that is free of theoretical baggage. The desire to adopt such a position may come from a variety of motivations (including the avoidance of more political disputes). Such a view, as we shall see, inevitably blinds us to the notion that facts are also social constructs and that certain theoretical positions will inevitably prefer certain facts. The other position that hermeneutics asks us to distance ourselves from is a form of radical post-modernism. By this I mean the view that theoretical paradigms totally construct our view of the world. This would make much empirical work unnecessary as we would only find such material that reinforced our theoretical prejudices. The tradition of hermeneutics, however, construes social enquiry as a more dialogical form of analysis than such a position allows. This is not to argue that hermeneutics can give us the answer to everything or that there is not considerable dispute within its borders; more that a genuinely 'hermeneutic' approach continues to have much to offer present generations of social researchers.

Before moving on let me make two points. First, while the term hermeneutics sounds very grand and intimidating, it is actually meant to

describe a rather mundane and practical activity. We all do hermeneutics. That is the practice of hermeneutics is concerned with the practice of interpretation. The aim of hermeneutics is to make clear an object of study or area of enquiry that is currently unclear and requires further clarification. Charles Taylor (1985) in this respect has argued that notions of interpretation are intimately connected with the need of human beings to make sense of their experience. Human beings, according to Taylor, have a need to position their experiences in narratives, to reflect on the kind of persons that they want to be and to form understandings of their relationships with nature and history.

Taylor argues that all forms of interpretation are an attempt to bring to the fore an underlying sense that is currently lacking. This activity presupposes at least three phenomena:

- 1 a field of texts, persons, documents or objects about which we can reasonably talk of their 'coherence' or 'non-coherence';
- 2 that we can make a distinction between the expressions of a particular group and the meanings that might be made clearer through further elaboration;
- 3 that these expressions are meaningful for a human subject.

In short, for Taylor what we call a successful interpretation is one that is able to make clear a meaning which was originally present in a confused or an unclear way. For instance, I remember when I first read the work of Raymond Williams I was unsure why certain terms like 'complexity', 'structures of feeling' and 'experience' had such resonance for him. I later discovered through much effort and further reading that Williams's attachment to these concepts had a multitude of interrelated sources connected with his own historical context, political position and individual particularity. It was only when I began to bring together Williams's personal and intellectual history that these notions began to resound with meaning. Take the idea of 'feeling': this was important as it spoke of the post-war New Left's desire to break with economistic Marxism, introduce an understanding of cultural relations, reconnect politics to humanist conceptions of the subject in defiance of the perceived brutalities of Stalinism and articulate an aesthetic dimension. So what am I seeking to demonstrate here? That after a period of reflection and confusion what initially appears strange and extraordinary can eventually make sense. In other words, simply having more information about Raymond Williams would be of little help unless I am able to link his expressions to interpreted meanings.

Now all this might, with further qualification, be accepted, but does it make for a correct or final interpretation? Hermeneutics resists such absolute formulations. What I am offering with respect to Williams is a particular reading of his life, intellectual context, worth, etc. This reading will be challenged, passed over and made sense of by other interpreters in

the future. Hermeneutics then offers a vision of the academic project as always incomplete and uncertain, responding to new questions and problems in the quest for sense. Indeed, if my particular understanding of Williams was, as it well might be, disputed by others, the answer could only be more interpretation: that is, all things being equal, I would attempt to enter into debate with others to identify areas of consensus and disagreement, before either we decided we were never going to agree, one of us got bored, or indeed we actually changed our minds and gave up on old ways of thinking. This process of continual and never ending critique and counter-critique points to the extent to which intellectual life and our identities are dialogically constructed out of the ebb and flow of conversation. More specifically it makes us aware that in coming to interpret a text, social group or person we can only appeal to interpretations that refer to other interpretations; this is usually referred to as the 'hermeneutic circle'.

More often this is experienced by the practitioner as being related to the uncertain nature of our interpretations which have a degree of openness attached to them due to their inevitably provisional nature. Of course these interpretations can be said to 'harden up' during the course of finishing a particular piece of research, but they never lose their open quality. This is summed up by E.P. Thompson in writing about the practice of the historian when he describes his craft as 'less an experiment in historiography, than a way of muddling through' (Thompson, 1977: 71). There are then no clear rules that we can point to which will guarantee a correct interpretation. All hermeneutics is able to offer in this respect is a reminder that we may ourselves come to change our minds or that readers at some point in the future may find our conclusions absurd.

BREAKING OUT OF THE CIRCLE

Against notions of muddling through, there have been various attempts to break out of the hermeneutic circle and offer more definite forms of knowledge. These can be characterized in two main ways:

- 1 rationalist attempts to offer up logical incontrovertial truths (I think, therefore I am);
- 2 empiricist arguments that the case is not built upon interpretation but on so called 'brute data'.

I am particularly interested in the second of these arguments, which is usually associated with positivist or scientific attempts to offer something by the way of evidence that cannot be questioned by another interpretation. When I think of this argument I often picture a man in a white coat pointing to a row of figures, looking to the audience and insisting that the 'facts' speak for themselves. As should be obvious by now, hermeneutics

argues that the debate over meaning cannot be brought to a halt in this way. This is because, at least at some level, social reality cannot be identified in abstraction from the languages that we use. This refers back to some of my initial remarks about theory and facts. Indeed this point has caused a great deal of confusion and upset in the social sciences.

Take the example of global warming. There are two positions to be avoided in this debate that can be connected to what I shall call subjectivism and objectivism. The first, along with hermeneutics, would argue that the way we seek to understand ecological processes is caught up with the language or cultural criteria within which we ourselves are enmeshed. So we might get a very different understanding of these processes from a radical ecological group and a government minister. Where certain critics have gone wrong, however, is to argue that such reflections can only be about the prejudices or individual perspectives of particular groups. Through a shared language both the government minister and the ecological group are trying to open out perspectives on the world in which we currently live. That is, while language does not mirror reality, it does of course refer to it. The other mistake in dealing with these arguments that is more strongly connected with empiricist claims is the objectivist illusion that we can know about global warming without taking the turn through language. The picture that is offered here is of reality standing apart from language. Of course the languages that we use help us picture the world that we inhabit. Post-structuralist and post-modernist writers of a variety of persuasions have helpfully pointed towards the textuality of nature; that is we need to attend to the historically shifting linguistic and cultural signs of nature. It is true that we can make no clear separation between the 'reality' of nature and the way that it is represented. The limits of our language are also the limits of our world. But, as Kate Soper (1995) has eloquently argued, 'this does not justify the conclusion that there is no ontological distinction between the ideas we have of nature and what the ideas are about'. Another way of expressing this view is that we can re-describe nature any way we might choose, but this will not ultimately affect 'real' processes of global warming and rising levels of pollution.

How then might we apply a hermeneutic sensibility to questions of this type without succumbing to a hard empiricism that suggests we could put complex cultural questions aside, or succumbing to the view that the 'natural' has disappeared into a post-modern simulation? Before we can answer this question we need to look more closely at the nature of language.

LANGUAGE

As we saw, for most contemporary theories, and in this hermeneutics is no exception, understanding and interpretation emerge through a linguistic dimension. But if language cannot be described as subjective (held inside people's heads) or objective (standing apart from the social world) it might

best be described as intersubjective. That is we refer to linguistic practices as intersubjective as they are bound up with everyday social practices, are changed by practical action and are constitutive of taken for granted social meanings.

John Searle (1969) provides a good example of intersubjectivity when he writes about the distinction between regulative and constitutive rules. If we take the example of a game of tennis then there are certain rules of conduct that are there to regulate the activity of the competitors and prevent foul play. On the other hand, tennis has a number of rules without which we could not imagine the game. These could be the practices of point scoring, serving, volleying and returning, all of which constitute the game of tennis. Just as language is neither objectivist nor subjectivist, so it is with the constitutive rules of tennis. Linguistic practices as constitutive practices make up a stock of shared background meanings that arise in the public spaces between people. For instance, we have to share certain background meanings that allow us to have a conversation or go to a lecture in the first place. The sociologist Garfinkel (Heritage, 1984) through his famous 'breaching experiments' attempted to reveal the extent to which human beings share a similar intersubjective web of pre-understandings when entering into conversations with others. For instance, in the next conversation that you enter, try asking 'why' each time your partner tells you something. The reason that he or she will eventually get annoyed is because you are breaking the taken for granted consensually held rules of what it usually means to enter into a conversation with another. In other words, social life is only made possible by the extent to which humanity shares certain background meanings that allow party elections, playing music and gossiping in the laundrette.

To take this argument a little further, understanding, then, always depends as we have seen upon background understandings. What happens when someone comes along who lacks the necessary background understandings? We have all come across irritating people who do not seem to understand that a good conversation has its own rhythm of turn taking and subtle intersubjective clues. Linguistic practices presuppose certain shared rules that enable us to 'go on' in a multitude of settings. Wittgenstein points to this when he says 'obeying a rule is a practice': that is an intersubjective practice can be illustrated if we think for a moment how we might persuade the boorish person who prefers the sound of his own voice to have a more mutual conversation. This might prove impossible as there is no fixed definition as to when it is acceptable within a conversation to talk for an extended period. This is something we just know how to do. There are, if you like, no abstract rules that govern what a good conversation is like that stand apart from the variety of contexts within which we talk to others. For Wittgenstein we will know how to have a conversation when we know how to follow the various shared conventions and rules that go into making a conversation. Again this points to a dialogical rather than a monological or disembedded view of the subject.

Wittgenstein was referring to questions of this order when he argued that there is no such thing as a private language. By this he meant that language and meaning are by their very nature public phenomena. To understand how our language works, we have to understand it as a shared intersubjective practice. Wittgenstein famously wrote:

Our language can be seen as an ancient city: a maze of little streets and squares, of old and new houses, and of houses with additions from various periods; and this surrounded by a multitude of new boroughs with straight regular streets and uniform houses. (Wittgenstein, 1958: 8)

I think that this is a helpful metaphor. To think of language as being like a city in which we live means that we cannot take up a position 'outside' language. Second, we might think of the way new streets are added to our city as referring to the way a language might change historically without losing its essential character. Language, he seems to be suggesting, does not develop through changes in 'external' reality (he rejects a correspondence theory of language) but needs to be understood as a practice in its own right.

Returning to our ecological question then, how might these reflections help us? First, a hermeneutic approach would seek to point to the different discourses or language games that were evident in respect of different ways of representing nature. Then we could search for the areas of agreement or consensus that must exist for us to be able to come into a dispute over the natural order in the first place. This process of clarification might then enable the participants to discover they had more (or indeed less) in common than they originally thought. Again such a strategy differs from certain post-modern viewpoints that seek to emphasize the radical difference and incommensurability of different perspectives on nature. Such a view also differs markedly, as I have indicated, from more positivistic approaches that would ignore our shared intersubjective and cultural backgrounds altogether.

SYMBOLIC CULTURE

The anthropologist Clifford Geertz (1973) adds an extra dimension to our discussion so far when he stresses the symbolic nature of culture that retains an openness to further interpretations by the lay actors themselves or the investigative social scientist. Here there is a need to distinguish between first and second order interpretations: that is, a separation needs to be made between the intersubjective meanings produced by the agents themselves (those whom we are investigating), and the sense social scientists make of these interpretations. Cultural expressions are meaningful for social agents as well as the researchers that study them. Often, as Anthony Giddens's (1984) notion of the 'double hermeneutic' implies, this

situation is further complicated as the interpretative worlds and frameworks of the social sciences are not as separate from those of lay actors as many have traditionally assumed. For instance, anyone getting married these days is aware of the increasingly high divorce statistics that are transforming traditional family patterns within western democracies. In the large part, these are made available for us through the practices of the social sciences. According to Giddens's later work (1990) this produces a situation of radicalized reflexivity within the modern era: that is, 'expert' cultures are not only the property of academic communities, but are continually finding their way into popular discourses through mechanisms such as the mass media. This radicalizes Geertz's original model in that what is being offered is not only an interpretation of an interpretation, but a reflexive reinterpretation of numerous layers of reworked unstable cultural meanings. We might be tempted to read such features, as many sociologists and cultural theorists are doing, as opening out a culture of radical reflexivity. Such a view would suggest (put very crudely) given the expansion of communication systems and the decline of tradition more generally within modernity that we are living in a more hermeneutically complex age than ever before. On one level we could argue that such a view does away (in certain branches of the social sciences at least) with an ivory tower view of academic practice. If the actual research findings, ideas and concepts of the academy are constantly interrupting the shared understandings of lay actors, then such a view is difficult to maintain. Perhaps more seriously, however, this view disrupts clear boundaries and divisions between what Geertz called second and first order interpretations.

Again this returns us to the problem as to what a good interpretation might be. As might be expected, different writers give different pointers on this issue. According to Geertz, a good interpretation of a particular linguistic community is not governed by the author's cleverness but by his or her ability to take the reader to the 'heart' of the symbolically produced common meanings. Ricoeur (1981) similarly talks of a valid interpretation as being one that is 'verifiable'. By this he means that the interpretations which are offered must not only be probable, but be more probable than the others on offer. On this question, he continues, it is important to search for agreement, but that this may not be possible, and indeed the desire to have the last word is probably connected to a violent and indeed totalitarian impulse. For hermeneutics a good interpretation is less of a method and more a rule of thumb. Ricoeur goes on to argue that valid arguments 'proceed in a cumulative fashion through mutual reinforcement of criteria – which if taken in isolation would not be decisive – that is a good interpretation is one that is convincing' (Ricoeur, 1981).

These are obviously not hard and fast rules which will guarantee a good interpretation, but Ricoeur tells us that if our arguments move along in this fashion then we could well be on the right lines. Rather than pointing to how discursive paradigms secure a field of research or looking to

theory-free factual evidence, such a view returns us to questions of debate and the clarification of an underlying sense, all of which can be associated with the hermeneutic circle.

THE GADAMER–HABERMAS DEBATE

To close I want to turn very briefly to what I take to be the central contemporary debate within hermeneutics. While I can hardly do justice to it here, I hope at least to indicate why it has been so influential. This discussion takes as given many of the points I have been trying to make so far in respect of the nature of language, but more centrally addresses questions of relativism on the one hand and power and ideology on the other.

Habermas (1990) has argued that understanding requires what he calls 'communicative action'. For Habermas the very fact that we are language users means that we are communicatively able to reach an understanding with one another. He argues that in every act of speech we are capable of immanently raising three validity claims in connection with what is said. These three validity claims, he adds, constitute a background consensus of normal everyday language use in western society. The three claims – that are used by agents to test the validity of speech – could be characterized as: truth claims, normative claims related to appropriateness, as well as claims connected to sincerity. These intersubjectively-held validity claims mean that we should accept that 'in principle' our own perceptions and utterances have the same status as those who we are seeking to understand. We should, according to Habermas, open ourselves up to reciprocal forms of conversation without having previously decided who is going to learn from whom. This would entail giving up the perspective of the 'observer' for an equal partner in conversation. Second, the interpreter also has to grapple with the 'context' within which interpretations are offered, as we should not assume in advance that we necessarily understand the group's background assumptions. Finally, as language cannot be said to mirror the social world, it is better to say a good interpretation 'fits' or 'suits' the meanings of the social group in question.

Habermas is clear, along with most other versions of hermeneutics, that the process of interpretation is inevitably tied to the horizons or value judgements of the interpreter. This, however, raises questions regarding issues related to the objectivity or validity of our findings. There are two responses, according to Habermas, within the philosophical tradition we call hermeneutics, to this problem. They are empathy theory and relativism.

Empathy theory suggests that we place ourselves in the position of the social group or historical period we are investigating. This was indeed what an earlier branch of hermeneutics advocated. Schleiermacher's version of romantic hermeneutics suggests it was the task of the interpreter to understand the author better than they understood themselves (similarly

Dilthey wished to recreate the author 'behind' the text). Such a view then suggests there are no interpretative barriers to putting on the shoes of the person or persons we are currently writing about. For example, if we were in the process of writing the life story of my now dead grandfather William Stevenson we could attempt to tell his story as he might have wanted to tell it. The only limitation that might be imposed upon us by this view might be an unwillingness of existing family members to tell his story or a missing diary that might reveal crucial information. The point is that there are no necessary barriers to reconstructing the view of the author behind the text or person behind the life.

Gadamer (1974), however, famously sought to critique the positions that can be associated with empathy theory. His views are related to the cognitive dimension he calls *Bildung*. Put briefly, individuals and cultures can be said to be in the possession of *Bildung*, the extent to which they retain an openness to other forms of life. *Bildung*, or learning from others, while opening oneself up for experience, is opposed to dogmatism. Gadamer, in this respect, argues that our 'prejudices' are necessary to our forming an understanding. Prejudice does not mean, as it usually does in ordinary language, the refusal to accept the rational arguments of another in favour of that which has no justification. Prejudice is instead, in Gadamer's terms, those cultural horizons through which understanding is made possible. One sees through the horizons of one's cultural tradition in such a way as to reveal, draw comparisons with and reflect critically upon past historical periods and other cultures. By merging horizons with other 'experiences', both sets of cultural presuppositions are brought into question. To write in a spirit informed by *Bildung* is to be aware that the historical horizon in which one finds oneself embedded is not a fixed, final point. This involves the recognition that different generations and different authors will necessarily ask different questions of history and culture. Thus romantic hermeneutics and empathy theory turn out to be mistaken in that we can never gain access to the 'other' by imagining ourselves outside our own location within culture and society.

To return to the example of my grandfather's biography, it would make a great deal of difference who decided to write an account of his life. If it were written by me, his grandson, it might contain romantic tales of a street-fighting man who was a trade unionist, spoke fluent Urdu, fought in the Balkans and later in life took to growing beautiful flowers. Gadamer's point is that it is a mistake to argue that a more 'objective' account could be written by someone else. It is, if you like, our own projections and interests that make the subject alive to us in the first place. However, the biography I write and construct is written in the knowledge that others would form different understandings of my grandfather's life and should be written in the spirit of open inquiry and critical reflection. It is then not the point to write an 'objective' study, but to be aware as to how the process of writing and researching could well reshape the questions I want to ask and my most intimate projections.

Habermas argues, however, that Gadamer's position outlined above ultimately reproduces a form of relativism. This is because it asserts that different value orientations will produce different research questions. As we saw in the example, Gadamer's position is quite open to the charge that the biography becomes an invention of the biographer. Further, we may give up talking to others, under this rubric, as we would never agree given that we have radically different perspectives. It is true to argue that Gadamer suggests that we should search for a consensus with others (what he calls the 'anticipation of completeness') accepting that agreement may not be possible. Habermas, on the other hand, argues that the main reason why agreement is not possible is due to the operation of power and force. As we saw, there was nothing about the structure of language itself that prevents those involved in rational conversation from coming to an agreement with one another. If this is the case, reasons Habermas, then failure to reach an agreement with and understanding of another must be due to the operation of material factors outside language. Further, Gadamer overlooks the extent to which 'consensus' could equally be the product of force and ideology.

For Habermas, we should go beyond language to investigate the 'extra-linguistic' factors which shape cultural perspectives. A rational consensus on this view can only be achieved if language is not deformed by the operation of power and ideology. This notion is connected to what Habermas invokes through his critical court of appeal or 'ideal speech situation'. The ideal speech situation is best represented as communication with the absence of barriers. This can only be satisfied where there is an equality of opportunity to participate in communication and where a statement is only true if it could potentially command the free consent of everyone. Thus rational consensus can only be arrived at once everyone has a right of participation and where those involved in the conversation are concerned for the well-being (or exhibit empathetic sensitivity towards) his or her neighbour. In this way, Habermas aims to overcome relativism through the forging of a rational consensus.

How might Gadamer respond to charges of conservatism and relativism? First, on the question of force and ideology, Gadamer would make the obvious point that we can only become aware of so-called 'extra-linguistic forces' through language and conversation. While I think Habermas is more attuned than Gadamer to some of the social forces that might 'distort' communicative flows, the answer to the problem raised is more talk and interpretation. Second, I think Habermas has a tendency to view disagreement as being purely the result of power and domination. Gadamer, it seems to me, is much more attuned to the notion that we could have free and open discussion and still fail to reach an agreement. In short we might point to areas of ambiguity or interpretative conflict where we are simply unable to reach an agreement with others from different cultural, historical or social contexts. We might indeed be able to point to certain inter-subjective areas of consensus without ever being able to reconcile the

numerous interpretations that exist concerning a certain issue. Here I take Gadamer's respect of pluralism rather than relativism as instructive. *Bildung*, despite Habermas's claims to the contrary, does not necessarily entail a relativistic conclusion that all cultures and forms of life are of equal value. The writer, alternatively, should engage with others in argument and debate developing insight into the variety of shifting perspectives that are available on a given issue. The enlarged mentalities that emerge from such a conversation are likely to clarify areas of disagreement, respect the difference of those involved in the conversation and, possibly, help us recognize exactly what the barriers to understanding and consensus are. Again to place the emphasis on what we might learn and the enlargement of horizons is, I think, to avoid charges of relativism.

If we take relativism to mean that one viewpoint is just as good as another, then Gadamer escapes this charge. To return once more to my grandfather's biography, a relativist position would suggest there are simply different biographies. Indeed this is what certain versions of post-modernism would try and argue. My grandfather's life would be viewed as a text that could be simulated from a number of radically different viewpoints. In that respect, there would be no way of privileging my account over that of other people. All we could do is point to the different discursive registers that are mobilized in trying to tell the story. One might be the story of a masculinist patriarch, another of working-class heroism and still a further of a dutiful grandfather. However, a Gadamerian approach would argue that the process of good biography writing would be the product of a dialogue with the past. The post-modern viewpoint could not ultimately prefer a dogmatic account of my grandfather's life suggesting he betrayed his comrades during the war which ignored evidence to the contrary. Such an account, within a post-modern perspective, could only point, as we have seen, to the different fictions about my grandfather's life. A more Gadamerian project would at least need to be open to the complexity of reconciling these different positions with one another, learning what I could through experience and reasoning and opening myself up to the critical interjections of others.

All of these issues remain important within theoretical perspectives in the social sciences. To end I just want to point to ways in which hermeneutics is likely to become more rather than less important in the modern world. First, the increasingly globalized and culturally hybrid world in which we live means that issues of cultural conflict, translation and interpretation are likely to become more and not less important in the future. While post-modernism through a discourse of fragmentation and difference retains a certain descriptive relevance, it is unlikely to be of much help in trying to foster common rules of intersubjective engagement. Whether locally, nationally or globally, there will be an increasing need to form common frameworks for cultural exchange, dispute and discussion. In this process hermeneutic understandings are likely to prove important in revealing the cultural specificity of the self and others: that is, the things

we share and of course what holds us apart. One only has to remember the Gulf War to consider that misunderstanding and violence often go together. Said (1993) wrote of the forms of intersubjective misrecognition that characterized this conflict:

Thus Muslims or Africans or Indians or Japanese, in their idioms and from within their own threatened localities, attack the West, or Americanisation, or imperialism, with little more attention to detail, critical differentiation, discrimination, and distinction than has been lavished on them by the West. The same is true for Americans, to whom patriotism is next to godliness. This is ultimately a senseless dynamic. Whatever the 'border wars' have as aims, they are impoverishing. One must join the primordial or constituted group; or, as a subaltern Other, one must accept inferior status; or one must fight to the death. (Said, 1993: 376)

Said's concern for ethnicity, global conflict and misunderstanding best characterizes the direction in which hermeneutic concerns and questions are likely to be of service in the future. What is not clear, to me at least, is that we can say the same of certain aspects of post-modernism which seemingly blandly celebrate human forms of plurality, or of more empirical orientations that remain on the level of description. In this respect, the continuing desire of hermeneutics to represent the plural nature of modern societies, along with a concern for social and historical contexts, continues to ensure its contemporary relevance.

3

FEMINIST METHODOLOGIES FOR SOCIAL RESEARCHING

Sue Webb

Once upon a time, the introduction of writings of women and people of color were called politicizing the curriculum. Only we had politics (and its nasty mate ideology), whereas they had standards. (Robinson, 1989: 319)

Such women's writing and research has the potential to disrupt traditional ideas of how we create social science texts and knowledge (Becker, 1986; Denzin, 1997; Haraway, 1988; Harding, 1986). It has called into question the surgical gloves of objectivity that appeared to prevent contamination of the research data by the researcher, and enabled the researcher to demarcate his [sic] personal and public life. A spill-over between these personal and public spheres has been recognized by many feminists who have struggled to overcome the systematic ways that the researcher's power to construct research stories has effectively silenced accounts that might change women's lives (Fine, 1994; Ribbens and Edwards, 1998). The consequence has been a focus on philosophical issues. This chapter will examine these issues, and begin with a discussion of why feminist contributions to social research should be considered. Some key features of feminist research will be identified by examining briefly how those who position themselves as feminists have practised as researchers. This analysis will show that central to many feminist accounts of doing research has been a distinction between method, methodology and epistemology. The reader will be guided through discussion of these terms and the chapter will describe how they have been used by feminists to distinguish their research activities from those of others.

WHY LOOK AT FEMINIST ISSUES?

As the tourist guide book says:

The institutions and organisations of Academia are masculinist in two closely related senses. The first is that historically the knowledge makers, guardians and teachers of this tribe have been male . . . The second is that knowledge is by

definition rational, scientific and universal. Those seminal characteristics are counterposed against those of emotionality, the natural and particular, and these and related characteristics – termed ‘binaries’ – are associated with the known characteristics of the sexes. Moreover, gender among the Academic tribe has both the power to magnificently increase the size and attractiveness (in the male) and can produce near invisibility (in the female). (Stanley, 1997: 2–3)

In focusing on gender among the Academic tribe, Stanley may be thought to be describing the antics and beliefs of some exotic society of the past and the account may be open to question. Alternatively, it may invoke feelings of identification with the writer, a feeling that one has visited this place before. Stanley’s account may increase the understanding of one’s experience of entering academia. This may be because fundamental to this type of feminist analysis of academic research and its communities is the view that women have been excluded from the knowledge-making process by men, and that the rules and regulations for constructing knowledge have been developed by and serve the interests of these men rather than women. Kramarae and Spender go further and claim that an achievement of this feminist scholarship that exposes the gendered construction of knowledge is that it ‘explodes the traditional knowledge making practices, and their products’ (1993: 1). They argue that an effect of feminism is that research has been unmasked, and that any understanding of research requires a focus on its philosophical assumptions, rather than on methods and techniques. In other words, it requires a focus on issues of epistemology (the criteria for determining what is acceptable knowledge) and ontology (one’s claims about one’s being or existence, and the effect this has on how the criteria for deciding what can be known about the world).

In essence the idea that research is a social activity and is affected by social organization and relationships of power, such as those between women and men, is a simple but challenging idea. It also resonates with other debates about positivism and empiricism and the discourse of the natural sciences, which include, for example, those within the philosophy of science that suggest science is a social practice (Kuhn, 1970), and those from the hermeneutics and interpretive traditions that argue that knowledge is partial and bounded by different perspectives (Habermas, 1972). Similarities can also be found with post-modern approaches that suggest that through its practice, which includes the production of research texts, research creates meaning and governs what can be known, and so a post-modernist account should seek to disclose this contingency and partiality of knowledge by critical reflexivity (Lyotard, 1984). As a consequence of these approaches, feminism along with these other philosophical approaches has had an impact on the way that topics have been conceptualized and investigated within many of the disciplines that inform social research, although the impact has varied (Kramarae and Spender, 1993).

CHARACTERISTICS OF FEMINIST RESEARCH: METHODS, METHODOLOGY OR EPISTEMOLOGY?

In adopting these philosophical concerns, feminist approaches to research are more than a debate about methods. Instead they are about a way of *being* and *doing* research in which there has been a shared assumption about the need to place the diverse experiences of women at the centre rather than the margins of social investigation, and to deconstruct research that has neglected women's experiences or assumed that male experiences are universal (Hall and Hall, 1996). Such commonality of approach is evident in spite of the diversity of feminist thought and positions. Feminist researchers have shared a focus in making problematic informal and formal institutional structures, phenomena and texts, and the social relationships that have framed women's and men's situations and experiences, in order to develop theories that advance social justice for women (Olesen, 1994).

Abbott and Wallace argue, 'it is the way in which research is carried out and the framework in which the results are interpreted that determine if research is feminist or not' (1990: 205), but the question still arises about exactly how feminists have carried out research that provides a non-subjugated or, even as some claim, an emancipatory knowledge? Taking an empirical approach to this question one might use a range of methods to do the following: ask questions of feminists and non-feminist researchers; observe and listen to what feminist and non-feminist researchers do and say; examine feminist and non-feminist research documents looking for examples of similarities or differences in methods used, and for signs of similarities or differences in meaning and experiences. In addition, the researcher's interpretations could be examined reflexively and could be cross-checked with those who have been studied or by comparison with information from other sources. Reinharz (1992) undertook a similar investigation such as the one discussed above and she found that feminists had employed a 'multiplicity of methods [that] allows us to study the greatest possible range of subject matters and reach a broad set of goals . . . There is little "methodological elitism" or definition of "methodological correctness" in feminist research . . . Feminist research is amoeba like; it goes everywhere, in every direction . . . The amoeba is fed by the women's movement. The women's movement, in turn, is fed by women's outrage and hope' (Reinharz, 1992: 243–244). Arising from her investigation, Reinharz (1992: 240) has identified ten themes associated with feminist social research:

- 1 Feminism is a perspective, not a research method.
- 2 Feminists use a multiplicity of research methods.
- 3 Feminist research involves an ongoing criticism of non-feminist scholarship.
- 4 Feminist research is guided by feminist theory.

- 5 Feminist research may be transdisciplinary.
- 6 Feminist research aims to create social change.
- 7 Feminist research strives to represent human diversity.
- 8 Feminist research frequently includes the researcher as a person.
- 9 Feminist research frequently attempts to develop special relations with the people studied (in interactive research).
- 10 Feminist research frequently defines a special relation with the reader.

This empirical investigation suggested that 'method' may not be the distinguishing feature of feminist research and bears out the arguments of those who advocate that the nature of knowledge needs to be recast through a feminist praxis based on feminist ways of knowing (see, for example, Bowles and Klein, 1983; Stanley, 1993). In contrast, others have questioned the apparent essentialism of this argument (Barrett, 1987; Currie and Kazi, 1987) and have argued that by adopting appropriate research strategies and techniques 'reality' can be known (see also Hammersley, 1992). In these ways, a concern with techniques and methods has continued to underpin debates about feminist research, even though in many cases feminist research practice reveals a preference for qualitative rather than quantitative methods (Maynard, 1994; Olesen, 1994; Reinhartz, 1983; Ribbens and Edwards, 1998). In order to understand this apparent contradiction between the practice of researchers who claim to be feminist and their writing about feminist research, something more than just an empirical investigation of their perceptions and methods is needed. Further consideration of the methodological and epistemological issues raised by feminists is required because decisions about the research tools used are frequently bound up with theories about how the research should be developed. For example:

One reason it is difficult to find a satisfactory answer to questions about a distinctive feminist method is that discussions of method (techniques for gathering evidence) and methodology (a theory and analysis of how research should proceed) have been intertwined with each other and with epistemological issues (issues about an adequate theory of knowledge or justificatory strategy) in both traditional and feminist discourses. (Harding, 1987: 2)

FEMINIST EPISTEMOLOGIES: FEMINIST WAYS OF KNOWING

The existence of different feminist discourses suggests there are distinct feminist epistemologies which provide frameworks for specifying the construction or generation of knowledge about the social world (see Harding, 1987; Lennon and Whitford, 1994; Stanley and Wise, 1990). Epistemological concerns are about the 'what' and 'how' questions concerning knowledge. These include questions about definitions of knowledge and the processes involved in its production, for example: