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Science and Commitment in Social Research

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Social research, unlike natural science disciplines, investigates the activities, beliefs, values and situations of other human beings. Though natural science is also a human activity and its agenda and social relations are shaped by human concerns, its subject matter is not. Gravity discriminates against no one and its effects are not subject to human redefinition or opinion. Many of the things social researchers investigate are. They are socially constructed and subject to redefinition. What counts as poverty now is not the same as in the past and definitions of it will vary from place to place. This character-istic of the social world has produced a contentiousness about the purpose and possibility of social research that is absent in the natural sciences. Certainly there is plenty of debate about scientific method and the ethics of science in the latter, but few would seriously doubt the efficacy of science *per se* in investigating the natural world. In investigations of the social world it is different with, historically, two broad groupings: those who believe that a scientific approach to investigating the social world is just as appropriate as in investigations of the natural world. Second, those who believe scientific approaches are inappropriate and instead the methods of the arts or humani-ties should be used. Traditionally these groupings have mapped onto the two methodological approaches I mentioned in the Introduction with the 'scientists' favouring quantitative methods and the 'humanists' favouring qualitative methods. However, like so many things in life, it is not that simple, because many of the scientists use or advocate the use of qualitative methods as well, though few humanists advocate quantitative approaches. Moreover, as I hinted, there are many shades of opinion in each grouping and some who would deny the distinction altogether. Its sharpness here is intended to bring some initial conceptual clarity.

The difference extends beyond method to why we do social research at all. Again, two groupings which roughly map onto the scientific/humanist distinction, are those who believe that social research should be objective and serve no ideological or political master and those who see social research precisely as a tool to achieve ideological or political goals. There are, then, two big conceptual issues at the heart of social research, the question of science and the question of commitment.

The aim of this chapter is to help make sense of these issues and their ensuing distinctions and to show how social research is transcending them. But if they are being transcended, you may ask, why bore us with history? Three reasons: first, it helps in order to make sense of the reasoning (which has evolved historically) underlying quantitative and qualitative approaches. Second, if these divisions are being transcended, then what is it they are transcending? Third, any choice of method comes with some underlying assumptions which can make a difference to its usefulness in describing, explaining and understanding the social world. In this chapter I will proceed as follows.

In the first part I will discuss the uneasy relationship between scientific and non-scientific approaches and the possibilities for a social research that transcends these difficulties. In the second part I will consider the issue of objectivity. I will conclude that a rigorous effective social research requires both a commitment to a moderate view of science and a belief that social research can be objective.

Science and art in social research

In a pithy book about the relationship of science to art (and a justification for the utility of the former) Raymond Tallis (1995) provocatively entitled the first half of the book 'The Usefulness of Science' and the second 'The Uselessness of Art'. Now it is not the intention of Tallis to suggest that art has no value for human beings, nor indeed that they can always be sharply divided. Rather, Tallis wants to say that for the betterment of the human material condition (in health, nutrition, technology that makes life easier, etc.), art is of no use. Indeed, to make the benefits of art widely available we need science's emergent technology.

In studies of the social world things are not so clear-cut. I believe Tallis is still right if we regard studies of the social world as science, but it is not the case that all studies of the social world are science. Although some say none are, I think some are and some aren't and even those that are must make use of the techniques of art. Before I look at the historical perspective I will briefly try to separate out (in very rough terms) those kinds of studies that are art and those that are science (even though they may use techniques borrowed from art[1]).

Art can tell us much about what we like as individuals and about the human condition and it does so through music, literature and paintings. The

statements of the artist are personal, but they are to be shared and perhaps (often unconsciously) they are statements that seek an empathy. Now perhaps this is a rather bland statement to make – of course, art is about the human condition, but how is it about investigation? There is an area between the humanities and what we might term social studies that is overtly about investigation. Often this is experimental art or literature, or historically an investigation of existing texts by scholars. Originally the latter work (in medieval times) was undertaken by scholars on biblical texts and aimed to uncover hidden meanings, to find a deeper interpretation of the texts. This was the art (or science – these things are never clear-cut!) of hermeneutics and its great champion in the study of human affairs was Giambattista Vico (1668–1744). Vico was especially interested in the study of history and maintained that because 'nations' and their history were made by humans, then it was open to ways of understanding not available in the natural sciences. To study history is to seek the meanings of the actions of the people involved. To use the anthropologist Clifford Geertz's (1994) expression, the aim is to find 'thick description', in which layers of meaning are uncovered.

Since Vico, many have held that this can be the only way to study the social world (I will return to them presently), but we can certainly agree on two things. First, that the self-reflecting, self-constructing nature of human society makes it conceptually different to the physical world and, second, that at least some of this is about art. A great deal of what we know about the world comes from the pen, or the brush of the artist. In other words, artists interpret the world (and that's good!), but sometimes the point is to change it! Art changes the world, but it changes it at a subjective level, it is the world of caprice and emotion.

Yet while social research must call upon the artist's methods of interpretation (and these I will discuss in Chapter 3), it needs also to be dependable and cannot therefore be art. We need more from social research. It needs to be able to give us information that will help the policy-maker resolve problems such as crime, poverty and discrimination. We need data about population projections that will help us plan for health, education, social security, etc. and we need trustworthy reports of people's opinions if we are to provide the goods and services they require. Harold Kincaid makes a succinct plea for dependable social science in warning us of the alternative

> If no social science is possible, if the best social scientists can do is to give us many different kinds of literary 'thick' description of social reality, then social policy is groundless. Imagine that we could have no real knowledge about social processes. Government intervention in social and economic affairs would be inane. How could we evaluate educational programs, prison reform, economic policy and so on without having well-confirmed generalisations about the causes of the social phenomena? (Kincaid, 1996: 7)

Put like that, who could disagree? Certainly if we cannot aspire to these things then social research is indulgent, and if it cannot produce results, then who will be prepared to pay for it? The quotation from Kincaid comes early in a book mostly devoted to a defence of the position of scientific naturalism

in social research – briefly, the perspective that the reasoning and methods of the natural sciences are just as much part of the social scientific project. In other words Kincaid's view is by no means without challenge. But what is it that is being challenged? Textbooks often tell us that it is something called positivism, so let us look at what is meant by the term and what are the objections to it.

Positivism

The term 'positivism' was coined by Auguste Comte (1798–1857) and later became associated with Emile Durkheim (1858–1917). Both have been influential in sociology, particularly the latter who combined analysis of numeric data with writings justifying the scientific approach to investigation, particularly emphasizing the importance of observation. However, despite their fame as positivists, a greater influence was that of logical positivism (sometimes called logical empiricism), a 'scientific' version of the philosophy of David Hume (1711–76). Hume's version of empiricism has two strands important to us here: the centrality of sense data (and therefore observation) and a scepticism towards knowledge claims that could not be grounded in observation. Hume's influence had been important throughout the nineteenth century, but in its logical positivist form dominated philosophical thinking about natural and social science in the first half of the twentieth century[2].

The influence on social research was not usually direct, but much more via the importance attached to statistical evidence and the consequent prioritization of statistical data for policy-making, particularly in the United States (see Platt, 1996). Social research was dominated by a profession 'trained in statistical methods, with mutually reinforcing motivations to win promotion and produce the "facts" needed by mayors, presidents and corporations' (Manicas, 1987: 226). The methods of this period were primarily the social survey, in which standardized data were collected from identified groups in the population on a range of topics as diverse as political polling and army life. What became known and accepted as positivism was never in this period any kind of unified approach (though a few such as George Lundberg (1939, 1947), tried to make it so). It was certainly a commitment to scientific social research, but a particular form of scientific social research. In other words, positivism does not exhaust the ways in which social research can be scientific. There are a number of very good books written on positivism, in particular, *Positivism and Sociology*, by Peter Halfpenny (1982) is to be commended and many authors have summed up what positivism consists of (see Phillips, 1987: Chapter 4; Bryman, 1988: 18–19; Williams, 2000a: 20–2). It is important not to conflate scientific method with positivist method, so what I will do here (in Table 1.1) is to list some key characteristics of science (as I see them) and some characteristics of positivism. Positivism believes in the characteristics of both columns, whereas the methodologist committed to scientific method need only align themselves with the items in the first

Table 1.1   Key characteristics of science and positivism

Scientific Method	Positivism[3]
Explanatory	Prioritizes observation
Predictive	Verificatory (procedures should show whether a statement is true or false)
Evidence based	Value free (moral values have no part in science)
Seeks truth	Operationalist (scientists can only deal in those things which are measurable).
Objective	
Logical	
Parsimonious	
Numerative	

column and may (if she is a realist, critical rationalist, etc.) substitute other items for those in the second column.

I will refer, to varying extent, to all of the items in column one in further chapters (but particularly in the next chapter), but first what happened to positivism?

Well, it got itself killed off by a philosopher called Karl Popper (1989, Chapter 11). Though he took quite a lot of his long career to say it, his refutation of logical positivism is simple and elegant. Positivists believed that all propositions about the world are only meaningful if we can show how they can be *verified*. Now this was stronger than saying scientists have to produce evidence, it meant that scientists must have two sets of statements: theoretical statements and *observation* statements. If you have a theory that children from one-parent families under-achieve, then you need to say what will count as under-achievement and the children will be seen to have under-achieved, if and only if that under-achievement can be measured. Priority is placed upon measurement rather than a prior articulation of the concept. Now we all know that terms like 'achievement' are subject to definition and redefinition, but it took Popper to point this out. Popper was not talking about one-parent families of course, but about complex ideas in physics. The problem for social science was the followers of logical positivism (for example, the above-mentioned Lundberg) believed that there could be unproblematic observation statements in the social world about such matters as class, alienation, etc.[4] The second (and most famous) part of Popper's refutation was to do with observation itself. The logical positivists believed observation could show a theory to be wrong and the theory should be discarded, but if instead it was verified, then it stood as knowledge. Positivists, then, believed in facts. These were the result of rigorous observation. Popper raised an objection that had actually been pointed out by Hume, but ignored, mainly on the grounds of the observational success of science since. The objection was, how can you know that all of your sightings of X fully describe all Xs? He illustrates this with the neat example of swans. Until European settlers travelled to Australia all swans sighted were white, so it

was therefore believed all swans were white. The first white settlers in Australia soon discovered that in Australia there were black swans. Thus the statement 'all swans are white' was falsified.

However much observational evidence you have can never prove any-thing; indeed, said Popper, science (or at least good science) does not pro-ceed in this way. Instead it proceeds through criticism, through setting out to falsify a theory, rather than proving it.

There's a lot more to Popper's argument than the brief summary I have given and it has been subject to a lot of critical debate since (see, for example, Lakatos and Musgrave, 1970), but nevertheless it did cause the logical posi-tivists to lose heart rather and positivism withered on the vine, eventually to be replaced by much more sophisticated versions of empiricism in natural science (see van Frassen, 1980). Despite this, if we are to believe the many, many textbooks on social science method, positivism is alive and well in social science. It isn't, it's dead (see Phillips, 1987 for a full discussion of its passing). Indeed, the reason for the brief excursus into Popper was to show why this is the case. Its continued ghostly existence is due to two things.

First, quite a lot of social research (particularly surveys) exhibits a kind of naïve empiricism (Williams, 1998: 14), whereby it is implicitly accepted that the measurements devised measure the reality as theorized. Now this is pretty much what the positivists believed, except they believed we could only talk of *sensations*, not realities. Take the example of homelessness. What I characterize as naïve empiricism is the belief that when you measure or describe homelessness, in a social survey, you are directly describing or measuring a reality of homelessness. It is more complex than that. First, it is very difficult to produce a definition of homelessness that will capture the many forms it takes and, second, respondents who may experience the same objective housing conditions may well interpret or define their experiences in different ways. The 'reality' of homelessness is heterogeneous and dynamic (Williams and Cheal, 2001).

Critics are right to a point when they claim to discern a continuing posi-tivism within the social survey, though the naïve empiricism I describe is not absent in interpretive research (see Bryman, 1988: 119). The second reason why many think positivism is still alive is much more to do with the ideo-logical success of anti-science in social science. In this view positivism and science are interchangeable (see Guba and Lincoln, 1982; Denzin, 1983). Now there is much in (let's call it) humanism that is of value and some of its intel-lectual pedigree is impeccable, but it is sometimes hard to separate out the rhetoric from the sound methodological points.

Humanism

There are two important strands to humanism: ideological and methodologi-cal. Though the first is important in the history of the social and natural sciences, I shall discuss this only briefly (but see Williams, 2000a Chapter 4 for a full discussion) and spend a little more time on the methodological.

Ideological strand: only the mad, or those of great vision, questioned science up until Hiroshima and then ironically they were often scientists themselves (Sagan, 1996). The real opposition to science only got going in the 1960s. Disillusionment with the arms race, the inability of science to feed the world and the horror of Vietnam produced a counter-culture that simultaneously looked to alternative non-Western philosophies for inspiration and to humanistic Marxism (Roszak, 1995 [1968]). The latter in particular emphasized the ideological nature of power, thus science was powerful, but was the product of a bourgeois hegemony. It followed that if there was to be revolution, then bourgeois science must be likewise overthrown.

The specific ideological criticism of science gained credence from the writings of Thomas Kuhn ([1962] 1970) who claimed that science was not simply a triumphal progress toward truth, but in fact what counted as truth and progress were often contained within an overall 'paradigm' which consisted not just of 'verified' discoveries, but also social and psychological assumptions about what counted as legitimate knowledge. Kuhn's critique, though often only half-understood, was actually much more influential on social science than Popper's.

All of these things contributed to a widespread (as Gerald Holton put it) 'delegitimation of science' (Holton, 1993). This took a while, especially in the United States where through the 1960s it was pretty much business as usual (Platt, 1996). Up until then social theory had been strongly influenced by the work of Talcott Parsons and political economy by the Keynsians, both emphasizing scientific method, and both came to be seen as apologists for capitalism. In both the United States and Great Britain government social research remained dominated by statisticians, mostly trained in the heyday of positivism. By the end of the 1960s social science as 'science' was being challenged in Europe and the United States. It was accused of being cold, impersonal and most of all in the service of the ruling class. The period of social revolution, in the late 1960s, coincided both with the elevation of young people as important consumers and citizens (as opposed to the comparatively subservient youth of the 1940s and 1950s), but also the expansion of higher education in the USA and Western Europe. This expansion favoured the social sciences and humanities and the new cohort of graduates had very different views on the purpose and method of social science to their predecessors. Though survey research was still favoured by government and market research, the shift to interpretation was marked in academic social science, especially sociology and anthropology and the reaction against it was muted (Goldthorpe, 2000: 65–6). The rise of postmodernism and post-structuralism in the 1980s provided further impetus and at the time of writing there is evidence to suggest that this approach still dominates British academic sociology.[5]

Methodological strand: the social sciences expanded rapidly in the 1960s, both in numbers of graduates, but also in terms of intellectual production. The implications for method were enormous, but can be summed up as marking a shift from explanation to interpretation, from number to language. This was combined with a specific commitment to social change,

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rather than simply describing social life.[6] What was at issue was whether study of the social world should be nomothetic or ideographic. The nomothetic is equated with *abstract* generalizable law like statements, and the ideographic can be equated with specific instances or moments. The best known champion of interpretivist method, Max Weber, held that social science should be both (Weber, 1975). Unfortunately rigorous attempts to resolve the dispute in Weber and later in the work of Herbert Blumer (Hammersley, 1989) were forgotten and the complex issues became rather simplified in the revolution against 'positivism'. Positivism became a mythical beast that had to be ritually slain in print at every opportunity.

As with the positivist case, which was far from being all bad, the anti-positivist humanist case was based on some sound reasoning. An important figure in this was Peter Winch, who in 1958 published a groundbreaking little book called *The Idea of a Social Science* (Winch [1958] 1990).

Winch argued that the social world was not law-governed, so the search for causes was mistaken. What the 'positivists' had taken to be laws were in fact rules, and, as we know, the defining characteristic of a rule is that it can be broken. Moreover, rules only apply locally. They can be seen as local grammars, which we must come to learn and, when we are proficient, are capable of changing. Until we are proficient we cannot understand, or we misunderstand a rule when seen from the outside. Rules themselves are often literally grammars and are expressed through language, therefore many of the mistakes we make are linguistic ones, which carry social consequences. The British comedian Jasper Carrot tells an apocryphal story of when a traffic cop pulled him over during a visit to the United States. The cop asked for his licence and noticed it had two 'endorsements'. 'Say, you must be a very good driver', says the cop 'your licence has been endorsed twice.' In Britain a licence endorsement is the accretion of penalty points for bad driving! On a rather similar note of cultural difference between the USA and Britain, few men called Randall use the diminutive in the latter country.

At the risk of bowdlerizing Winch's complex work, the argument runs like this. Social life is rule-governed, rules are linguistic and languages, or the use, or the nuances of languages are local. To know the local, you must be a local. Winch's conclusion is pessimistic about studying social life at all, but the followers of Winch took the linguistic point seriously and many embraced epistemological relativism – the belief that in interpreting other cultures there are no truths universal to all cultures. Nevertheless, most assumed that some kind of interpretation was possible, but it had to be an interpretation that was faithful to the culture being interpreted. It should not be the researcher, but the voices of the researched we hear.

Now this is a long way from hypothesis-driven surveys, where the researcher tries to find out how many people have characteristics X or believe Y and possibly goes on to produce mathematical models to explain characteristics, or behaviour. For many new to studies of the social world, what has been termed the 'linguistic turn' seemed altogether a more liberating method for both the researched and the researcher. Indeed, most of us have experienced a frustration when faced with a choice of a, b or c on a

questionnaire, none of which fit our views or circumstances. 'Shall I put you down as a "don't know"?', says the interviewer. Such practices distort our view of social life, say the interpretivists; we must learn to understand the meanings people hold for what they do, or say. We must learn about social life in context.

The experience of interpretive methods in anthropology, particularly the use of the techniques of 'participant observation' or 'unstructured inter-viewing', has fed back into the work of sociologists and policy researchers. Participant observation requires the investigator to partially, or wholly, immerse herself in the culture of those he or she investigates. This might be within an 'alien' culture, such as that investigated by anthropologists (see, for example, Geertz, 1979) or more 'familiar' cultures such as the inner city (see, for example, Whyte, 1955 [1943]; Suttles, 1968; Spradley, 1970; Hobbs, 1988). The participant comes to understand (it is claimed) the culture from the point of view of a person from that culture. There are no firm prescrip-tions for the collection of data, or what kind of data are collected. The inves-tigator uses whatever is best or possible (notebook, tape recorder, or just memory) and records those things that help to build a coherent picture of the culture that faithfully captures the meanings of its participants. Likewise the unstructured interview (confusingly sometimes called the focused inter-view), though often guided initially by some questions from the interviewer, is intended to allow those investigated to develop their perspective (Bryman, 1988: 47). No hypothesis is being tested, standardization of the interview (questions, format, etc.) is unnecessary, the aim simply being 'to make sense of an object of study' (Taylor, 1994: 181).

Now this sounds very plausible and many feel it is a better way of doing things than an impersonal survey. Unfortunately the above approach and Kincaid's strictures do not go well together, or at least interpretive method comes at a price.

The first problem is the one that Winch identified. If only a member of a community, knowing the rules of that community, can really know that com-munity, how can an outsider ever achieve accurate knowledge? Now Winch meant this in respect of very different cultures to ours, though it can be a problem even within our own Western culture (see the Jasper Carrot story above). How can the researcher know that the interpretations he or she makes are the 'valid' ones of those interpreted? Alan Bryman (1988: 74) dis-cusses this in relation to school studies, where to an extent all adults are strangers!

While it is generally acknowledged among interpretivists that some kind of solution, either at the philosophical or technical level, is possible to the above problem of internal validity, the problem of external validity is both more difficult and more contentious.

Survey research is based upon the well-established statistical principles of sampling. That is when we have an identified population of people or things, it is possible to devise procedures whereby a sample taken will reflect the characteristics of the wider population (see Chapter 4). For all sorts of reasons this is not a strategy open to the interpretivist. As we will see in

Chapter 4, interpretivist researchers do attempt to select, even sample, but groups investigated cannot be sampled in a conventional sense for a number of methodological reasons: for example, because they are elusive, rare, the research must be covert or a large enough sample is unachievable.

If there can be no systematic approach to sampling, then the researcher cannot know whether the findings are typical, or simply confined to the group being researched (Williams, 2000b). However rich the descriptions are, however sophisticated the interpretation of meaning, the researcher does not really know the likelihood of this being a one off.[7]

There are other associated methodological problems, which I will discuss in more detail in Chapter 3, but briefly, the advantage of the unstructured interview is that it allows respondents to prioritize or choose categories appropriate to them, but this also means that any standardization of procedure is limited. There are no questionnaires, often no predesigned questions (though there will usually be lists of topics) and consequently many of the questions that are asked in interviews, or the scenes recorded during observations, are not repeatable (or at least are not repeated). Finally, although the aim of interpretivism is to conduct research in 'natural' surroundings, the researcher can never know to what extent he or she has changed the nature of the surroundings by being there at all. Added together these problems inevitably lead to the question of whether interpretivists can ever know if they have got it right or wrong. There are no criteria of external validation (Jones, 1998).

It is not just the critics of the ideographic approach who have realized these problems; they are recognized by many interpretivists themselves. Broadly, there are two responses to this. There is the relativist approach embraced by postmodernists, which simply sees all accounts as epistemologically equal and not to be privileged one over another (Rosenau, 1991: Chapter 7). In its extreme this approach is art and art in the romantic tradition and represents precisely the kind of 'research' Kincaid complained of. I will return to this below in the context of values. Conversely there is an opposite approach of moderate realist ethnography, often influenced by the Chicago School of research (more about them in Chapter 3). It is represented in the methodological writings of Martyn Hammersley (1989; 1998; 2000) and embraces the view that while interpretivism has inherited a great many methodological problems, technical and philosophical solutions are possible and must be sought in order to preserve the great utility of the method. As no doubt you will have discerned, my own sympathies are very much closer to Hammersley than the postmodernists, though I recognize that between the two positions there are a host of others that have at least some methodological value.

Methodological pluralism

In the foregoing I have outlined two extremes, which have been for the most part abandoned by mainstream social research. Nevertheless there are

postmodernists out there still fighting the good fight against positivism (see Denzin and Lincoln, 1994, for examples) and a few unreconstructed mathematical junkies wanting to explain the social world solely through statistical algorithms. Most researchers have embraced what are sometimes termed post-positivist positions on methodology, believing that the principal objections to positivism and interpretivism can be overcome within a methodological pluralism. The spectrum of post-positivist methodologies is wide and embraces (for example) critical theory, some forms of feminism, critical rationalism, realism and a number of hybrids of these. I shall say no more about these because this is a methods book and not intended to be an extensive discussion of philosophical and theoretical issues underlying research (though see further readings recommended at the end of this chapter),[8] but I think we can identify and summarize a middle ground which overcomes some of the more serious difficulties I discussed above.

THE LIMITATIONS AND POSSIBILITIES OF SCIENTIFIC METHOD   All science begins with theoretical constructions which are turned into testable hypotheses. After Popper and Kuhn we know these are social constructions, but to a point they are testable, though we may need different methods to test different kinds of theories and hypotheses. The answers we get are not final, but subject to further redefinition and testing. We may hypothesize that Community A is more 'religious' than Community B and we may test this through measuring formal public religious worship in a survey. However, it is likely we will decide that formal public worship may only measure one aspect of religion and to fully develop the concept and the survey questions we may have to use interpretive methods to find out what counts as 'religious' in each community.

THE LIMITATIONS AND POSSIBILITIES OF INTERPRETIVISM   Two big problems exist for interpretivism. The first is, how can a stranger come to know society S; the second is how can we know whether the features of S are unique, or common to other societies? To continue with the hypothetical example of religion. First, in trying to understand a religion there are almost certainly characteristics that will be present in all religious observance, for example a belief in some form of superhuman existence and engagement in worship or contemplation. These kinds of things provide initial identifiers for the researcher and they are unlikely to exist in a cultural setting that is without any other kinds of referents to other cultures. Winch described extreme examples rarely found nowadays and there will usually be some kind of cultural consistency and continuum. This does not mean that we can know whether a particular feature is general or specific, say, a particular kind of religious ceremony, but as with the survey, the limits of this method can be transcended by using other methods.

Methodological pluralism is not necessarily the view that we must use multi-method approaches (though I will discuss these things in relation to research design in Chapter 9), but it is a perspective which maintains that social science needs a particular methodological approach that can combine

scientific reasoning with methods suitable to social investigation. This means recognizing both the potential and the limits of each methodological approach. The critique of positivism and the counter-critique of interpretive methods did much to clarify the potential and limitations of these methods and paved the way to methodological pluralism. How this might be brought about in the context of a social *science* will be discussed in subsequent chapters, but I turn now from the question of science to the question of objectivity and commitment in social research.

Social research and commitment

I, like so many others, did not become a social researcher through methodological commitment. The motivation was social commitment: I wanted to learn about social science, so that I could use that knowledge to make the world better. This remains the case, but I also have come to realize that the causes in which we believe are best served by truth. This, however, is not always a simple matter. Natural and social scientists alike now mostly accept that the activity of each is socially situated, that 'value freedom' is no longer tenable. In social research it has become almost a cliché to say that values enter the process at every stage. When people speak of values they usually mean moral values, but the word value can have a broader meaning encompassing both utility and measurement. To say something measures 3 degrees and not 4 degrees is to make a mathematical and logical distinction between two states. To say something is 'good' is to make a similar logical contrast between a desired and not desired state. These different uses of the word value are less distinct that would first appear, as Steven Shapin (1979) has shown in the story of phrenology and cranial topography. Though the former was motivated by now discredited moral values, much of what we know of the latter was due to measurements undertaken by nineteenth-century phrenologists. A second example can be found in the story of the *t* tests, widely used in social statistics. Here we have to thank beer drinkers for its existence. William Gosset was a research chemist working for the Guinness brewery in Dublin in the early twentieth century when he worked out the *t* distribution. He was not allowed to publish under his own name and used the pseudonym 'student' – hence the 'student's *t* test' (Clegg, 1982: 87). Though in the second case there was more of a commercial desire to sell beer than moral prescription, but what both of these examples tell us is that even measurement values have a social history.

Some go further than this claiming that measurement itself is a moral value, born of particular cultures. Prominent in this view are some forms of standpoint feminism which hold that the attempts at the language of value neutrality in science disguise deeply held masculine social values. Most standpoint feminists are not intentionally anti-science, but believe the whole scientific project needs rescuing from androcentricity. I will return to this below.

Social science has not been immune to such criticisms. The anti-positivist movement of the 1960s and 1970s was not just methodologically radical, but it was also radical in terms of values. The above mentioned Thomas Kuhn claimed that there was an 'incommensurability' between paradigms. This was not just the way of doing science, but what counted as discovery or the resolution of a problem – truth itself. Though Kuhn never intended it to be the case, his claim led to a conclusion of epistemological relativism among some philosophers of science and social scientists. This means that the truths of one culture are not the truths of another, nor are they translatable into each other. No overall, encompassing truth is available. The relativist con-clusion was therefore not the same as that of the standpoint feminists. The latter accepted the claim about the moral basis of values, but drew the conclusion that the problem could be transcended.[9]

The acceptance of the value-laden view leads to three broad positions on values and commitment. These positions have not just moral implications for social research, but methodological ones to do with bias as well.

Relativism

Epistemological relativism is the starting point of postmodernist and many poststructuralist accounts of the social world. For the reason I set out at the beginning of this chapter, that research should be dependable, I do not think these accounts qualify as research at all and I only mention them because of their prominence in the (mainly) anthropological and sociological literature in the past 15 years. The absence of any acultural standards of truth about the way the world is leads to a comparative method of alternative 'interpretations' of aspects of the social world in which no interpretation can claim any privilege. It follows from this relativism about what is, that a similar position about what ought to be, is held: 'that no cultural tradition can analytically encompass the discourse of another cultural tradition' (Tyler, 1986: 328).[10] The plea is instead for multiple voices to be heard, but none to be privileged – a sort of moral agnosticism. In a curious way this extreme scepticism has come full circle to meet the value freedom of the positivists in that neither claim any prior moral com-mitment. For examples of relativist accounts in anthropology see Clifford and Marcus (1986), and for a powerful critique of these positions see Jones (1998).

Normative approaches

What I term 'normative' approaches are all committed to a moral view of what is right or wrong, desirable or undesirable in the social world. The best known of these is Marxism. There are fewer Marxist researchers around now than there used to be (presumably because they've paid off their mortgages) and Marxism is perhaps more interesting in the positions on values it has influenced, in particular feminism and critical theory.

There are many strands of Marxism, but central to them all is the view that no one occupies a politically neutral position. All are members of one or

other class and this will determine one's political perspective. Neutrality in social science occupies a similar position to equality before the law: it is a bourgeois myth. Therefore social research will reflect its own class bias and if you are a Marxist then you should orientate what and how you research to the interests of the proletariat. This does not stop you being objective as a researcher, but objectivity is grounded in a particular class agenda (see Wright, 1997).

Similarly, critical theorists insist that knowledge is obtained from a perspective. Also, like Marxism (from which critical theory originated), there is a commitment to emancipation. The role of research is in the furtherance of that emancipation. However, the difference between this perspective and a Marxist one is that critical theory is first and foremost committed to social science as critique, or as Nancy Fraser puts it: 'A critical social theory frames its research programmes and its conceptual framework with an eye to the aims and activities of those oppositional social movements with which it has a partisan, though not uncritical, identification' (1989: 113).

Later versions of critical theory have placed emphasis on the role of self-reflection in the research process (May, 1998a) and the importance of the provisional nature of knowledge (Fay, 1996).

A third example of the normative perspective is that of feminism, particularly the standpoint feminism I mentioned above (see, for example, Rose, 1983; Harding, 1986; Hartsock, 1987; Harding, 1996). As with critical theory, emancipation is the aim, the difference being that a particular emphasis is placed on producing objective accounts of reality. This is a very different kind of objectivity to the traditional scientific view (and the one to which I'll refer below). Science, including social science, is seen as a gendered activity that simply reflects a patriarchal value system. The view that science can be objective under these circumstances is itself a patriarchal myth. Such myths distort science and its results. A more objective view of the world must be grounded in women's experience, an experience shaped by patriarchal subjugation. The particular importance of the feminist perspective lies in the methods adopted, which begin from the express position of women and their subjective experiences. This often leads to alternative methods such as autobiography and diaries (see Ribbens and Edwards, 1998).

The justification for most normative positions is that researchers cannot be neutral observers and in the case of the 'strong' positions I have outlined, have a duty to take a stance on values if they wish to be agents of change. Moreover, it is often held, the whole project of social science is itself a normative one, not just about explaining or understanding the social world, but about changing it.

The difficulty with all normative positions is that of agreeing what the goals of the ideological or political project shall be. There are substantial differences within critical theory or feminism about how emancipation shall be achieved, or indeed about what counts as emancipation. Second, when truth is seen as subject to context, then a change of context can change what is seen as truth (see Hammersley, 2000: Chapter 6, for a discussion of this). This can make a difference even in respect of the methods adopted. Feminisms

and critical theory have mostly eschewed survey methods, in the former case because they are seen to embrace the very androcentric values of science that had subjugated women for so long. The need for non-hierarchical methods was argued for by Anne Oakley (1981) as a result of her research on mother-hood. The richness of her data was seen (by her) as resulting from a personal engagement with the mothers she interviewed. However, 19 years later things had changed and Oakley was arguing that the gendering of methods had produced an unhelpful ideological reaction (Oakley, 2000). Things had gone too far.

Objectivity

In my view normative positions throw up as many difficulties for the researcher as that of value freedom, especially as at a pragmatic level the adoption of particular ideological positions about research outcomes tends to make the research less believable to those who do not hold such positions. If I am conducting research on homelessness I do not want to be believed only by those who see homelessness as an unacceptable consequence of the commodification of housing, I want also to be believed by those doing the commodifying and those who have no position.

This view might be seen as a successor to both that of positivistic value freedom and normative positions. It is accepted that values permeate the entire research process and that value freedom is unobtainable. Indeed, as I have shown, it is the case that non-moral values, such as measurement values, themselves often have moral antecedents. However, to abandon objectivity is to abandon any kind of dependability in research with the possible consequence that there is no difference between the believability of research findings and fiction. An unfortunate consequence of anti-positivism is that it has left a legacy of conflation between value freedom and objectivity, leaving normativism as the only value position (as opposed to relativism) to occupy.

What I have termed the position of 'objectivity' has a history traceable to Max Weber (Weber [1925] 1978). Rather confusingly, (though in keeping with the time in which it was written) Weber's version of objectivity in sociology is described as 'value free', though it is quite different to that advocated by the positivists. Instead Weber says that the sociologist must *begin* from a position of values. He regarded values as a defining characteristic of human beings. Nevertheless he also recognized (Albrow, 1990: 243) that if we accept that all values are subjective or culturally contingent, then this leads inexorably to relativism. Instead Weber believed that only by embracing values whole-heartedly can a scientist achieve value freedom. Moreover, he also recognized that science itself is founded on values such as truth, rigour and clarity.

Weber's position rests on the acceptance that in science in general and specifically in sociology, there will be a debate about 'ends', about what policy should achieve and therefore what research should be done (Weber, 1974: 75). It does not follow from this, however, that the investigation itself

need be biased by those values. For example, interpretive methods allow us to know the meaning of social practices for the people we investigate, but it does not mean we have to take sides on the values they hold. One does not have to be a Fascist to understand Mussolini and why he took Italy into World War II; one does not have to be deviant to understand deviance. In sociology Weber's objectivity is to make the subjective values of those investigated the subject matter of the discipline, to show how their values guided their actions. Objectivity does not imply neutrality, as the positivists believed. Alfred Tauber (1997: 30) cites Robert Proctor's distinction between the two. Neutrality implies that science does not take a stand, whereas objectivity is about whether science is dependable. Proctor uses the term reliable.[11] Particular sciences may be completely objective, but may serve economic or political interests.

A lot of good social science has followed this dictum of Weber's. Yet leaving one's values behind is not always as straightforward as it seems. Feminists are not wholly wrong when they claim that the methods of science are themselves culturally influenced (see, for example, Harding, 1996). The development of scientific social science was less to do with its perceived methodological efficacy in its own right and much more to do with the success of scientific method in investigating nature. Despite this, and here all would agree, science (and social science) need methods of some kind and moreover some kinds of method do seem effective in obtaining data. A recent thinker to recognize this is Helen Longino (1990), herself a feminist. She speaks of science having both constitutive values that are internal to it (things such as accuracy, reliability, etc.) and contextual values, those external things which shape what it is science is about, or what particular sciences should strive for. This position is similar to Weber's, but she goes further and says that contextual values do and should inform constitutive values. The history of both natural and social science would bear out that this is indeed so. Developments in methodology have nearly always been linked to contextual goals. The rapid industrialization of the United States in the first half of the twentieth century coincided with the growth of scientific sociology, both survey method and experiment (Madge, 1963; Platt, 1996). Likewise it was not just idle curiosity that led to the development of urban ethnography by the Chicago School, but the perception that the poverty that grew out of urbanization was a serious social problem. The context of the slum shaped the ethnography itself. Longino's resolution of the conflicts of interest this might entail is to redefine objectivity as (what she calls) 'transformative interrogation'. The values are acknowledged, but they are examined communally, so that, unlike Weber, she does not believe the decision about objectivity is a burden an individual should carry.

The idea of a critical community of social or natural scientists seems splendid, but does it follow from this that objectivity would indeed emerge, or would it be the case that consensus, whether or not such consensus was right, would prevail?

Objectivity cannot be a matter of democracy or consensus, for it leaves no room for the maverick and may well punish innovation. Yet objectivity can

be a public good that we strive for, but it must be a good we also strive for individually. It is possible to accept that objectivity is indeed a social value, but one which has the unusual property of helping us get a little closer to the truth. Proctor's redefinition of objectivity as reliability seems useful to me. Two social scientists could, for example, disagree on what ends a research programme should serve and may disagree on the theoretical motivation for the research, but they could nevertheless come to agree upon the objectivity of statistical testing, of scaling within a survey question, or about good and bad technique in participant observation. Objectivity as reliability is a striving to make research as dependable as possible by eliminating as much error as possible. Box 1.1 shows the values in social research.

Box 1.1   Values in social research

Moral or political values can enter into social research at a number of stages. Here are some of the principal ones:

- What to research: the first and most fundamental entry of values is why, who and how decisions to research X or Y are made. In the UK the Economic and Social Research Council (ESRC) (http://www.esrc. ac.uk) is the major conduit for government funding of social science and although nominally independent, it must continue to demonstrate to government that the research it funds is useful and relevant. Views on usefulness and relevance are likely to change with governments.[12] Very little research these days is 'resource free', those resources have to come from somewhere and will usually come with some control over what is researched. The researcher has some control over which funds or jobs to apply for, but this will be constrained by what can be funded. In social research, as natural science research, political and societal values will influence what research is sponsored.
- Values of the researcher: even though many of the strategic decisions about what kinds of things will be researched are external to the researcher, the values of the researcher will play a major role. Obviously he or she must make the decision as to whether to work on a project or not, but researchers are citizens too and will be more motivated by one thing than another. This might be purely academic interest. Most famous sociological and anthropological studies, such as Goffman's study of asylums (Goffman, 1961), Malinowski's study of the Trobriand Islands (Malinowski, 1937), or Lynd's study of an American small town (Lynd and Lynd, 1929) are such, but even here the choice of studying a particular topic in a particular place is the expression of a value. Most research is conducted as a result of a particular concern about a problem, whether that problem is

articulated in a specific way, such as particular instances of inner city deprivation, or a more widespread societal concern about the changing nature of urban community.

- Choice of method: this does not sound like a candidate for a question of moral values, but, a decision to use unstructured interviews, for example, will give a much more individual perspective on a question than that of a large-scale survey, which may emphasize structural features. Choice of method will therefore make a difference to outcome.
- Theory choice: more than one theory can be used to explain our data. A Parsonian functionalist, for example, and a Marxist would explain differences in the amount of days lost to strike action from quite different perspectives.
- What to analyse and how to analyse: every research project will yield far more data than can be analysed (this applies to all methodological approaches). Some data may support our hypotheses (see Chapter 2) and some may refute it. In practice, firm confirmation or firm refutation are unusual. Secondary analyses of data using different theoretical assumptions or statistical techniques may produce quite different answers to those originally obtained (Dale et al., 1988: 54–5).
- What to report: even the most honest researcher must decide which findings he or she will prioritise and just how much credence he or she will give to a confirmation or refutation of a hypothesis. I will return to this question in Chapter 10.

Values and bias in social research

How do values enter social research and how do they relate to 'bias'? Traditionally bias has been seen as pejorative in science, a distortion of method or findings. However, with the discrediting of the positivist value-free position, things are not so clear-cut. Indeed, as Martyn Hammersley and Roger Gomm note, the term bias is used in many different ways in social research (2000: 155). Where one stands in terms of values will make a difference to whether one sees bias as something to be avoided, reluctantly accepted, or celebrated. Moreover, much depends on the point at which bias enters. A standpoint might be seen as a bias, for example, and a deliberate and necessary one, though whether one consciously adopts a standpoint or comes to occupy it is contended (Harding, 1986: 136–51). A later version of the feminist standpoint, Harding's 'strong objectivity' (Harding, 1996) is a deliberate attempt to prioritize and examine those social distortions that may reduce objectivity. This strategy is often called reflexivity (and I will discuss this further in Chapter 3) and in various forms has been advocated by interpretivists for a long while and to a varying degree of introspection and deconstruction (Seale, 1999: Chapter 11). As a form of personal auditing and accounting this is valuable, though the problem is knowing to what

extent the researcher can really know what kind of values he or she holds and which of these might bias her research.

Box 1.1 shows some of the ways and the points at which values enter research. From the position of objectivity I advocate, some of these are legitimate, even desirable – they are Longino's 'contextual values', which will underlie the kind of research we do and our values as citizens and researchers. For example, we may not agree with the particular funding priorities of the ESRC or SSRC, but we would have to agree that funding councils would have to make decisions about what to fund. These things may be bias, but they are not error. Error can lead to bias, but the reverse is not always true. Hammersley and Gomm (2000: 162), though taking a slightly different position to this, helpfully focus on the many different ways error can enter and influence research. Some error is to do with outcomes. Error may be systematic or haphazard. It may be culpable or non-culpable. An important facet of error and bias in social researcher is that the researcher does not simply read off accounts of the social world with differing degrees of competence or accuracy, but instead is active in creating those accounts. For example, a great deal of feminist interpretivism has been an active engagement in helping women to gain a 'voice', to make public those things that would have otherwise been private and unknown; see, for example, Melanie Mauthner's account of sister relationships (1998).

What might be see as culpable on non-culpable error is not clear-cut in terms of this kind of engagement and this is particularly the case in interpretive research. Fortunately there are areas of research where things are a bit simpler. Some kinds of bias are obvious and culpable. For example, deliberate selection of respondents who will confirm one's views, or the prioritization of convenient findings over inconvenient ones. However, some kinds of error are unintentional and lead to a form of bias about which there is widespread agreement. These are, for example, sampling bias, bias in question design or choice of analysis strategies. I will return to these in subsequent chapters.

Conclusion: art and science, objectivity and subjectivity

Social research requires commitment. It requires moral commitment, a motivation to change the world or to know our humanity a little better. Without this kind of commitment we lose sight of the difference between the social and the physical world. The latter is morally inert, though often changed by, or imbued with human values. The former is morally active and our investigation is itself a moral act. We could not be uncommitted even if we tried, therefore our commitment is a matter of reflection (May, 1998b).

All of this would lead to a narcissistic introspection unless we hold on to the value of objectivity, the cornerstone of any kind of investigation. To be objective is to be transparent but to aim to get as close as possible to the

truth. The truth may not always be clear and, like the rainbow's end, we will never fully possess it (and if we did, how would we know?). This in turn must commit us to following the best method toward our investigative goals.

In the chapters that follow this translates into a pluralism and a pragmatism about method, though never losing sight of the importance of the deeper methodological implications of choosing one approach or technique over another. In the next chapter I will consider how we begin to take the first steps from the commitment to a research goal, and methods to achieve it.

Questions for reflection and discussion

Seek out two or three research projects that interest you and ask the following of them:

1   Who funded the research and why?
2   Did the research claim to be 'scientific', or use 'scientific methods'?
3   Who would benefit from the research?

Suggested further reading

The issues I have discussed are complex and more nuanced than could possibly be captured in one chapter. There is a very large literature on the methodological themes I have discussed.

Some of the issues in this chapter are developed further in my own *Science and Social Science* (2000a). A perennially useful book on methodological issues raised by the quantitative/qualitative divide is Alan Bryman's *Quantity and Quality in Social Research* (1988). The issue of values in social research is considered in depth in Martyn Hammersley's *Taking Sides in Social Research* (2000). Fiona Devine and Sue Heath's edited collection *Sociological Research Methods in Context* (1999) considers many of the issues raised in this chapter and others in later ones. A thoughtful and sometimes provocative book is John Goldthorpe's *On Sociology: Numbers, Narratives and the Integration of Research and Theory* (2000).

Notes

1 Actually I think it goes further than that. There is a great deal of 'art' in science, even natural science. One only has to look at the beautiful equations of the great mathematicians and physicists to realize this. There is more in common between Einstein and Bach, or Hilbert and Sisley than is often allowed (but see Tallis, 1995).

2 There was plenty of opposition to logical positivism, even in the USA, particularly from R.S. Lynd and C. Wright Mills (Horowitz. 1983), but opposition usually took the form of a debate with positivism.

3 I discuss the definitional issues of positivism in more detail in Williams (2000a).

4 Lundberg and later Blalock favoured a doctrine called operationalism. This was the view that what counted as alienation, achievement, etc., was what could be measured as such (see Blalock and Blalock, 1971; Williams, 1998).

5 This claim is based on current research being conducted with Geoff Payne and Suzanne Chamberlain. The research is a documentary analysis of key characteristics (method, topics, authors, universities, etc.) of articles published in three generic and one specialist British sociology journals in 1999–2000. Only 14 per cent of the papers in the generic journals used quantitative methods. Thus 40.6 per cent used qualitative methods and 7.4 per cent used mixed methods, while 37.7 per cent were non-empirical.

6 The methodological debate in social science, from the 1960s onwards, was a re-run of the *Methodenstreit* (battle of methods) dispute at the end of the nineteenth century. In the earlier dispute traditionalists favoured a value-free scientific approach to political economy, that science was 'pure' and investigations could and should be free of moral (social and political) values. In opposition there was the 'historical' school of Gustave Schmoller (Manicas, 1987: 124) who maintained that the social equivalent of natural laws could not be established and that economy should be studied in historical context as only one part of social life.

7 For many anti-positivists taking 'randomly selected samples of human experience' (Denzin, 1983: 133) is not a legitimate strategy because there is a rejection that research should be 'generalizing' from individuals or groups to wider society in the first place. It is held that there is too much cultural variability for sampling and generalization in social life.

8 Though in the next chapter I will briefly discuss and advocate a broadly realist approach to research.

9 Though it is often claimed that 'standpoints' lead eventually to either essentialism (for instance, that women come to occupy standpoints because of particular defining character-istic), or it leads to relativism where there are many competing standpoints (Halberg, 1989).

10 As Hammersley (2000: 157n) points out, we should not conflate epistemological relativism (as described above) with cultural relativism, the view that there can be multiple cultural perspectives in the social world. It is quite legitimate to be a cultural relativist (as most social scientists presumably would be) without any commitment to epistemological relativism.

11 Proctor is using this in a somewhat different sense to its use in survey research (see Chapter 5).

12 In the USA the funding regime is slightly different. The Social Science Research Council (SSRC) http://www.ssrc.org/ is independent of government and at the time of writing was even actively sponsoring academic contacts between the USA and Cuba, an anathema to the US government. In the USA and the UK there is also a great deal of direct government fund-ing, through departments, of specific research projects.