ONE

Introduction

The widely accepted view of science is that it is the means of securing truth in knowledge about the world. It is for this reason that many sociologists have taken the view that sociology, along with other social sciences, should follow the scientific method: it should be a science of society. This view has been challenged by those who see society as closer to the arts and humanities than to the sciences: they argue that sociology is a humanistic discipline in which the subjectivity of the individual sociologist is central to the knowledge produced. The most radical variants of this argument see social investigation as totally relativistic.

The implications of this go beyond sociology, and the view of science as an objective basis for truth has been challenged. Those working in social studies of science have developed a view – often characterised as 'social constructionism' – that appears to challenge the objectivity of science and its claims to produce 'true' knowledge. Scientific knowledge, they argue, is a product of the constructive practices of scientists and cannot be seen as an unproblematic reflection of a world external to science.

This is the basis from which we address the question of objectivity and subjectivity in social research. We each believe that this debate is significant for sociologists and other social scientists but challenge some of the simplistic understandings of and values attached to so-called 'objective scientific' approaches and 'politically subjective' ones. We come to this debate from different directions, and these shape the differing conclusions and emphases in the arguments presented in this work. Before continuing with our introduction of the main issues and outlining the structure and organisation of the rest of the book we begin by each introducing ourselves through brief biographical snapshots of our intellectual influences and starting points.

Influences and starting points

Gayle

MY INTEREST IN OBJECTIVITY AND subjectivity, the relationship between them, and in methodology and epistemology more generally began when I was an undergraduate. In my first year of study I wrote an essay which required me to consider the political aspects of the process and product of sociological study and used the work of Max Weber, C. Wright Mills, Howard Becker, Alvin Gouldner and others. Browsing through the library bookshelves in year two, I found the first edition of *Breaking Out* by Liz Stanley and Sue Wise (1983), and although feminist epistemology was not officially on the curriculum until year three, for me this added to my already growing interest in the status of the claims we can and cannot make from research and our relationship with and responsibility to respondents and the academic community. This interest, indeed fascination, with accountability remains.

In a paper written in 1999, Liz Stanley described herself as a ‘child of her time’, suggesting that intellectual/academic socialisation affects our interests and approaches. I too am a ‘child of my time’ and one consequence of the development of my sociological imagination (Mills 1959), alongside the awakenings of my feminist consciousness, has been a constant concern with the relationship between the process and the product(s) of research; how what we do affects what we get. Some of my substantive research and writing interests – which include reproductive and non/parental identities, working and learning in higher education and travel mobilities – also relate to experiences and influences inside *and* outside the academy. For my first piece of individual research (as a third-year undergraduate) I chose to study women’s meanings of miscarriage (Letherby 1993), an event I had myself experienced four years earlier. In 1990, when I began my PhD on identity and definition with reference to ‘infertility’ and ‘involuntary childlessness’ (which I write in single quotation marks to highlight the tensions in meaning), I fit the medical definition of ‘infertile’ and was at that time ‘involuntarily childless’ (e.g. Letherby 1999, 2002a, 2003a; Exley and Letherby 2001). In the mid-1990s I became a ‘step-parent’, which influenced other writings, including a recent piece focusing on experiences of social motherhood (Kirkman and Letherby 2008). Thus, some of my work in this area (I have also undertaken research in the area of foster caring, teenage pregnancy and young parenthood, and long-term conditions in pregnancy) relates to my own autobiography and throughout my career I have been concerned to reflect on the significance of my own experience to my work. All of the projects I have worked on, whether close to my own experience or not, have had an impact on me both intellectually and personally. I’ve been interested, pleased, angry, sad, and so on. Research is an endeavour characterised by politics, power and emotion, and it is important to reflect on the implications of this.

Obviously, it is not always possible *or* indeed desirable to research issues close to our own experience. I do not believe that identification should be seen as a prerequisite to ‘good’ research and it is inaccurate to assume that *all* research is grounded in the autobiography of researchers. Furthermore, researchers do not always identify with respondents and vice versa, even when they share an experience and/or identity, and involvement at any level brings its own challenges and problems within research. Neither do I believe that researchers must always reveal all in their research and research writings. However, I do believe that the life experience and identities of researchers are present at some level in all that we do and that it is important to acknowledge this.

With all of this in mind, I argue that critical reference to the knowing/doing relationship is an essential aspect of all research. In my previous writings in this area I have tried to work towards a position that challenges traditional claims to objectivity and recognises both the personhood of the researcher and the complexity of the researcher/respondent relationship and yet allows for useful things to be said (e.g. Letherby 2003b, 2004, 2011b). For me, then, what we need to do is focus on the theorisation of the subjective (which includes the researcher's motivation and practice and the respondent's expectations and behaviour) and its significance to knowledge production. My starting point thus recognises the values (both positive and negative) of the subjective, the significance of experience, but is not a rejection of the need to be critical, rigorous and accurate.

Malcolm

I BECAME CONCERNED WITH ISSUES of objectivity through applied social research. My own inclinations are towards a left liberalism, indeed my political background was (and remains to a great extent) libertarian socialist. A concern for social problems, particularly severe housing need, brought me to the social sciences. As a mature student and later a researcher of homelessness, I came to realise that the latter was politicised not just at the level of tackling it, but also in matters of explaining and even measuring it. People on the left wanted to 'prove' how prevalent homelessness was and were often prepared to use methods and rhetoric to achieve this. Meanwhile those on the right believed that homelessness was overestimated and due to the fecklessness of individuals. They preferred not to research it at all. Both sides were concerned to show they were correct, regardless of the number of homeless people there actually were. I wanted to tackle homelessness, but I also first wanted to know what the reality of homelessness was.

This led me to a general question, which has remained with me – 'how can social scientists be committed to progressive change, but remain rigorous investigators?' My intellectual journey to try to answer this question led me to a critical engagement with scientific method. In the 1970s and 1980s the left was sceptical of science, for it had brought us nuclear weapons, nuclear power and pollution. Moreover, the writings of Thomas Kuhn, Paul Feyerabend and their followers, had convinced many that scientific method was simply a rhetoric of persuasion, a cultural and social artefact, a story among other stories about the world. The social sciences were inhabited by people of the left, who possibly as a result of a rejection of science, but also as a result of embracing the emergent philosophies of poststructuralism and postmodernism, turned against the more traditional modes of 'scientific' enquiry in social science.

In some ways these new 'turns' to the cultural, the linguistic, humanistic produced new insights, sociologically and methodologically. Books like Cicourel's *Method and Measurement in Sociology* (1964) demonstrated that there were limits to the more traditional forms of measurement and explanation in social science. Scepticism about science was often sophisticated (see, for example, Brian Appleyard's *Understanding the Present* (1992)) as was the attack on 'positivism' in social science, but they left us with a big and a small question. The big question was: if we see current science as a tool of ideology and methodologically flawed, what if anything do we envisage replacing it? In rejecting the technology we disapprove of, do we reject all technology and the scientific endeavours that produce it? Do we give the same epistemological weight to shamanism as we do to the laws of physics? The small question was: if social 'science' itself rejects science and its methods, how can we provide reliable and valid data that will help us to tackle social ills?

In my view, the political and methodological critique of science had gone too far – it was illogical and hypocritical. Similarly, the rejection of science in social science was so often based upon a mythical science that was value free, always (claimed to be) truthful and accurate. The philosophy and history of science I read taught me that science was a social enterprise, a faltering, sometimes successful search for the truth about the world. It was always ideologically driven, but so often (Galileo comes to mind) it was able to overthrow an ideology by showing that the world was different from that which had hitherto been believed. There were Eureka moments, but mostly the progress of science towards more accurate explanations of the world had to be seen in the long historical view.

Around 12 years ago I worked on a method of counting homeless people that would allow local authorities and NGOs to know approximately how many homeless people lived in particular locations. The method 'capture–recapture' was adopted from biology and was originally used to count penguins, but some adaptation could be used on transient human populations. The method had (and still has) its flaws, partly because of the difficulties of defining homelessness and partly because of the counting methods being less reliable than the statistical method they support. Indeed, at one conference, where I had given a no-holds-barred critique of our capture–recapture work, I was berated for introducing a method of counting the homeless that was 'flawed'. I admitted to this, but my question was how can we do better? At the time we could not and I truly believed it was the best way to count such populations. It is still good, but I'm glad to say new methods that rely on multilevel modelling are challenging it.

Yet, 'how can we do better?' is not a bad credo for social science, both as a tool for improving the lives of our fellow citizens, but also as a tool of investigation. I believe that objectivity and recognition of our subjectivity and of the intersubjective nature of social science are crucial issues in our quest to do better.

John

MY ENTRY INTO SOCIOLOGY WAS directly from school. Unlike Gayle and Malcolm, I did not have a prior work life, apart from weekend and vacation jobs. For this reason, perhaps, I did not experience any great conflict between practical concerns and the academic life. Being male, there was also little discrepancy between my personal, subjective experience and the demands and expectations placed on me by university study. After a conventional education at a boy's grammar school, the idea that knowledge comprises both an objective representation of the way the world actually is and a true account of how it came to be that way did not seem at all problematic.

I began my studies in 1968, when the idea that sociology is 'the science of society' seemed unproblematic to the established teachers of the subject. The views of Auguste Comte were taken as the founding statements of this 'positive' science, and the word 'positivism' had not yet attracted the unfortunate – and very misleading – pejorative connotations that it was later to acquire. The course that I took in 'Theories and Methods of Sociology' presented me with standard arguments from the philosophy of science to buttress this view of the scientific status of sociology.

Yet 1968 was also, of course, the high point of student radicalism, when 'bourgeois' science was being challenged by a rediscovered Marxism and all orthodoxy was subjected to 'critical' reassessment in the light of practical, political concerns. Even a College of Technology in suburban London – not yet a polytechnic, let alone a university – could escape such ideas. I had been brought up in a Labour-voting family of the first-generation middle class, and the politics of the student movement resonated with me and brought home a realisation that sociology cannot be separated from practical concerns with inequality and injustice.

The young teachers recruited to teach sociology were also influenced by the intellectual and political climate of the times, and this influenced the way in which they delivered the established curriculum. Peter Winch's *Idea of a Social Science* (1958) was an established text and was read as a justification for a radical cultural relativism and, therefore, a questioning of the objectivity of western social science. Kuhn's *Structure of Scientific Revolutions* (1962) was also taken up as a manifesto justifying a view that no scientific perspective could be accorded absolute status and all were subject to degeneration and change. I began to encounter the view that there is a variety of competing perspectives in sociology and that diversity is to be encouraged and embraced: sociological understanding rests on 'values' that differ from one social group to another and so sociological theories must be equally diverse. Howard Becker's question, 'whose side are we on?' (1967), became the watchword.

By the time that I began to teach sociology myself, I had begun to wonder how these contrasting views – objectivity and partisanship – could be reconciled. I drew on the arguments of Alisdair MacIntyre (1967), encountered in a course on 'Ethics and Social Philosophy', and found myself attracted to the argument that Stephen Toulmin was putting forward in the first (and only) volume of his work on human understanding (Toulmin 1972). Both MacIntyre and Toulmin put forward the view that concepts are rooted in culturally diverse historical traditions, yet they argued also that principles of rational discourse can be employed within each tradition and can mediate between traditions. For Toulmin, in particular, the possibility existed that both natural and social science could be seen in developmental terms as moving away from misunderstanding and towards improved – but never perfect – understanding.

My views on how this 'improvement' in knowledge was to be demonstrated was sharpened by the publication of Roy Bhaskar's *Realist Theory of Science* (1975), which brought into focus the ideas that I had discovered in Rom Harré (1972). Bhaskar recognised that knowledge developed in a 'transitive' dimension of cultural and historical variability but that the reality to which scientific knowledge referred was 'intransitive' and provided the ultimate basis for judging the adequacy of knowledge. This was the basis on which I felt I could reconsider the various views that I had encountered and could reconcile their ostensibly divergent claims.

I concluded that all sociological work originates in personal and political standpoints that orient us by providing distinctive perspectives on the world, but also concluded that the rational discourse and methods shared with other intellectual disciplines allow us to incorporate divergent perspectives in a more comprehensive account that more adequately grasps the real objects that lie behind all knowledge. Sociological knowledge, that is to say, can be 'objective knowledge'.

Understanding factual descriptions

So, if we all agree on the need for and the possibility of an approach that is both accountable and has value, if not objective in the traditional sense, how are we to defend this view and convince our readers that this is the direction that social science should take? This is our task in the rest of this book. We present a view that concludes that social science can be trusted to produce robust knowledge capable of reliably guiding practical decisions. (Social) science does not provide absolutely certain knowledge, but it can provide evidential support for its claims – even if this support can always be undermined by new evidence. Science faces the constant threat of revision and so is inherently uncertain. Nevertheless, science is the most reliable source of knowledge and has, therefore, been remarkably successful in its practical applications. This is the basis of scientific authority and expertise in the policy sphere. We will show, however, that the values and subjectivity of the scientist, far from being extraneous to science, are integral elements in its claims to objectivity and expertise, accountability and value.

Our descriptions of the world are always partial, selected and filtered by our perceptual apparatus, by the assumptions that we bring to our observations, and by the particular perspective or standpoint from which we view the world. The ways in which we interpret these observations and formulate them into statements that can be communicated with others, are, furthermore, dependent on the particular language that we use. Both our perceptions of the world and our descriptions of those perceptions are linguistically mediated. A language is always the collective property of a particular population or social group. It both constitutes and reflects the assumptions, experience, and history of that group or population. Both the vocabulary and the syntax of our language structure our observational reports. This adds a further selective mechanism to our attempts to describe the world. In all of these respects, then, observations are to be regarded as *cultural* constructions that depend also on the physical perceptual apparatus that we, by virtue of our 'natural' human characteristics, bring to our observations.

Statements of fact, then, bear a logically indeterminate relationship to the external and independently existing reality within which we live and that we observe. 'Reality' as perceived and described in statements of 'fact' may not correspond to reality 'as it actually is' independently of those descriptions. The important question, therefore, concerns what can be said about the 'truth' of observational statements and the accounts that we give of those observations.

The conventional scientific response to this has been, in the words of Sir Isaac Newton, that descriptions are, nevertheless, 'very nearly true' (Newton 1687/1969: Vol. 2: 456). Accepting neither a dogmatic absolutism of factual truth nor a sceptical relativism that reduces fact to opinion, most scientists have held that empirical reports are unlikely to be too far short of 'the truth' so long as scientists strive to eliminate bias and preconceptions and ensure that observations are as technically accurate as possible. Similarly, the objectivity of historiography has been claimed on the grounds that historical accounts that are presented undogmatically can be corrected by technically more reliable observations. For both physical scientists and historians, then, technical reliability provides the route to validity.

Such a position was also set out in the founding statement of sociological method by Auguste Comte in his outline of 'positivism'. Positive knowledge, Comte argued, is a product of the methods of investigation introduced in the Enlightenment and the scientific renewal that it initiated. Scientific methods provide a guarantee for the truth of scientific statements. This was the position that was largely taken over by Emile Durkheim in his *Rules of the Sociological Method* (1895), and applied in his study of *Suicide* (1897), a study that was adopted as the paradigmatic model of sociological research for much of the twentieth century.

For many, however, this is no solution at all. Marxism, Feminism, Postcolonialism, and a number of other radical alternatives to mainstream social science have resurrected and reinforced the spectre of relativism and a denial that anything even approximating to this commonsense view of truth can ever be sustained except by political fiat. If 'might makes right', then, perhaps, might also makes truth.

Developing a similar view in relation to the physical sciences, Thomas Kuhn (1962) argued that all paradigms of scientific description and explanation are subject to radical overthrow by the advocates of alternative paradigms. A successful paradigm is that which is able to attract the largest number of adherents, for whatever reason and certainly not for logical, intellectual reasons alone. Factual knowledge generated through the application of the paradigm remains ultimately contingent.

We argue against the relativistic implications of this point of view in order to defend an idea of scientific truth that respects the autonomy and importance of divergent values and standpoints.

Understanding Value Judgements

The conventional view of both physical and social science is based on an assumption of value freedom. While some take this to mean that science should only ever be undertaken 'for its own sake' and without any regard for its implications for human concerns and values, the core of the position is simply that science is *impartial*: its evaluation of evidence takes account only of cognitive values and does not – or should not – be influenced by moral values. On this basis, science is not so much 'value free' as free from *moral* values. Science can provide technical or instrumental knowledge and an assessment of the consequences of different policy proposals, but the scientist has no moral authority or superiority within the policy sphere.

This was at the heart of the sociological method set out by Max Weber and was developed by Robert Merton (1942) in his view that scientific activity was governed by values and practices of scientific communalism, universalism, disinterestedness, and organised scepticism. He recognised, like Weber, that scientists may have their own moral values and policy preferences, but he held these to be separable from scientific activity itself. Nevertheless, science and policy making have become ever more entangled as policy makers seek technical solutions to physical and social problems. The idea of value freedom is, therefore, more difficult to sustain: how is the technical authority of science in the policy sphere to be maintained if scientists are to be detached from policy debates and agnostic about moral values?

The growing role of science in policy, through its integration with government and commercial interests, has highlighted this question in relation to the moral responsibility of the scientist. Can the scientist evade responsibility for the uses to which that knowledge is put? Wernher von Braun, the German rocket bomb scientist, was famously parodied for holding this position by the satirical singer Tom Lehrer:

Don't say that he's hypocritical
Say rather that he's apolitical
'Once the rockets are up, who cares where they come down
That's not my department,' says Wernher von Braun.

This view holds that the scientists, concerned exclusively with purely factual and technical considerations, need not be at all concerned with the social consequences of scientific discourses and their application. The implications of this position were highlighted in debates over Robert Oppenheimer's Manhattan Project on the development of the atom bomb during the Cold War. Despite his own left-wing views and saying, on the explosion of the first atomic bomb, 'Now I am become Death, the destroyer of worlds', Oppenheimer continued to take a technocratic view of the applications of his scientific ideas and never openly challenged American nuclear policy.

In social science, similar moral issues have arisen in relation to the Project Camelot, in which political sociologists studied rebellion and revolutionary processes in Latin America as part of a project financed by the US State Department and with the express intent of suppressing radical social change in US client states such as Chile. Many participants chose to ignore the intended uses of the research, but Johan Galtung spoke out against it and the project was abandoned (see Horowitz 1967).

We argue that social scientists cannot evade issues of moral responsibility. While science may not privilege any particular value judgements, and while value positions cannot be put forward in the name of science, all scientists must reflect on the actual and potential uses of their research. They must make clear in public debate their personal, moral assessments of the dangers (and benefits) consequent upon the application of their research. They must participate in the public sphere, adding their voice to its debates. They must make clear their expertise, and its boundaries and limitations, and they must argue – as citizens and not as scientists – about the uses to which that expertise is put. One moment in the role of the sociologist, therefore, is to act as a 'public intellectual', stepping beyond the production of impartial knowledge and standing back from involvement in its policy applications, to engage in political discourse concerning the formulation of public policy.

Structure and Organisation of the Rest of the Book

The rest of this book represents the debate between us. Each of us is the primary author for Chapters 2–7, although each chapter begins with a jointly written introduction and each chapter ends with some discussion between us. We have indicated individual authorship, as in this chapter, by using our first names and ragged right text. Collectively written text is set as justified text. We continue our debate in Chapter 8, which is written as a *trialogue* – reflecting email and face-to-face discussions between us – although, for us at least, this is not the end of the debate but simply represents the moment in time at which we wrote our final words.

Chapter 2: 'The Philosophical Basis of Objectivity and Relativity', primarily authored by John, explores the philosophical basis of these debates from their beginnings in the Kantian position on the nature of knowledge. Despite Kant's own concern for objectivity and absolute knowledge, contemporary relativist views have also derived support from his argument. These debates are traced through Nietzsche and perspectivism to Weber's classic position on objectivity and value freedom, and on to contemporary standpoint and postmodernist theories.

Chapter 3: 'Relationism and Dynamic Synthesis', primarily authored by John, looks at the ways in which Karl Mannheim took up the arguments of Weber and provided an answer that also resolved the diversity of standpoint theories. The distinction between relativism and relationism is drawn out as a central element and it is shown that Mannheim proposed a relationist strategy of 'dynamic synthesis', arguing that objectivity results from genuine debate and dialogue among contending positions with the social researcher attempting to incorporate divergent but authentic standpoints in an overall synthesis. This is related to the arguments of Habermas on the emergence of consensus in an ideal speech community and Popper's argument that truth emerges in an 'open society'.

Chapter 4: 'Situated Objectivity in Sociology', primarily authored by Malcolm, picks up the discussion of value freedom and the disputes over whether social science is to be 'value free' or is to base its investigations on particular value positions. It is argued that the search for the truth about the physical and social world is a value position but, nevertheless, one that can produce actionable knowledge. While a freedom from values is untenable, this need not preclude the value of objectivity as a purposeful search for the truth about objects. This search is socially situated and will prioritise particular scientific goals at different times.

Chapter 5: 'Theorised Subjectivity', primarily authored by Gayle, attends to a recognition that, while there is a 'reality' 'out there', the political complexities of subjectivities, and their inevitable involvement in the research process, make a final and definitive 'objective' statement impracticable. Theorised subjectivity recognises the values – positive and negative – of the subject studied, but holds that this should not be automatically equated with involvement or partisanship. Reference to the similarities and differences between theorised subjectivity and 'strong objectivity' and feminist fractured foundationalism will be made.

Chapter 6: 'Social Objects and Realism', primarily authored by Malcolm, argues that the pursuit of conceptual synthesis must be complemented by an understanding of the adequacy of the conceptual objects synthesised. Objectivity and subjectivity imply the existence of social objects that exist as 'things in the world' and that we must inevitably see oneself in relation to these. It is held that social research is a search for explanatory adequacy at the level of cause and meaning, and that explanatory adequacy requires ontological assumptions about the composition of the social world as the outcome of contingent causal processes.

Chapter 7: 'Objectivity and Subjectivity in Practice', primarily authored by Gayle, explores issues of the moral and political responsibility of social scientists as they have been explored in debates in and around arguments for 'public intellectualism' and 'impact'. Political aspects of the research process and praxis as a goal will be considered with reference to the issues of accountability in light of the claims and counter-claims made for objectivity and subjectivity in the earlier chapters of this book.

In the final, concluding chapter, 'Objectivity Established? A Trialogue', we return to the divergent autobiographical reflections with which we began this chapter and we consider, in the form of a trialogue, the extent to which the explorations in the body of the book have resulted, or not, in a coherent account of objectivity and subjectivity and their place in contemporary sociological practice.